

We thank the reviewers for their constructive and helpful comments on the revised manuscript. We are pleased they find the manuscript significantly improved and most of their initial concerns have been addressed. We have accounted for their further comments in the second revision.

Referee #1: Rantanen, Mika

The manuscript has been improved compared to the previous version, but the revision is not yet entirely convincing. While a fourth variable (winter total precipitation) has apparently been added, the changes have not been implemented consistently throughout the manuscript.

– In your response to my comment #2, you state that cumulative precipitation has been added to the supplementary material; however, I am unable to locate this, and Figure S3 appears to be nearly identical to Figure 3.

Apologies for this inconsistency. When we prepared the responses we thought it would be best to include this additional variable in the SI, but upon analysis of the new 'Winter precipitation' results we decided this would enhance the main results because the signals were clearer and more significant than for daily maximum precipitation, and so we included it in the main manuscript in Figures 3-8 in place of daily maximum precipitation.

Figure S3 does show the same as Fig. 3, but absolute values not % anomalies.

– Section 2.3 explicitly refers to four variables, but in several parts of the manuscript the winter total precipitation is not addressed at all (for example in the Discussion).

Additional sentences have been added to the Discussion to explicitly mention the additional hazard variable winter total precipitation. Note that in the first revision, Figures 5-8 were all based on winter total precipitation in place of daily maximum precipitation, so this variable is addressed throughout the manuscript.

– On line 161 there is a reference to Figure SX, which appears to be an error. This has been deleted.

– References to figure panels are incorrect in multiple places (e.g. lines 231, 234, 239, 270, 309, among others).

Thanks for pointing these out. The references have been checked and corrected where necessary.

– Figure 8 is missing panels a–d, despite these being referred to in the text.

These have been added

- Figure titles have years 1981-2020, but text says 1980-2020. Yet at line 309 they write 1980-2022. The authors need to explicitly say which winter seasons are included in the analysis.

Apologies for this inconsistency. These have been corrected. The year refers to the Jan/Feb of each winter and so the correct usage is 1981-2020 as in the figure titles.

Overall, in your response to my major comment #2, I would have liked more concrete handling of the issue, and at minimum discussing it in the Discussion section.

Reviewer #2

I thank the authors for addressing many of my concerns. I feel that all but one has been addressed to a reasonable level in the revised manuscript. However, the issue that remains for me is this: my main comment from my first review was:

"What does the utilization of ETCs add to the story?"

Your answer in the response to the reviewers says:

"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."

And yet, the revised manuscript (as in the 1st version that I reviewed) has the following as the 1st research question being addressed:

(Lines 67-68)

How are the two leading modes of North Atlantic atmospheric variability – the NAO and EAP – related to spatial variability in extreme ETC hazards due to wind gusts, daily precipitation rates and significant wave height?

This question seems to be a bit at odds with your comment in your response to my major concern. I would appreciate it if you could clarify your explanation, or change the questions in the manuscript. Or keep the questions but add some explanation on your thinking to provide clarity on this issue: are you focused on the ETCs or the hazards. Or if you are focused on both, explain that assumption to the reader.

We thank the reviewer for raising this point again, which they do not feel was properly addressed in the first revision. The reviewer is correct that one could examine the relationship of local weather hazards with PC1 and PC2 irrespective of the ETC footprints. However, it is our view that this would miss an informative physical connection. It is well known that the NAO and EAP are associated with variations in the North Atlantic eddy-driven jet, which in turn shapes the baroclinic environment that controls the formation of ETCs. It is also widely known that winter-time weather hazards in Europe are predominantly caused by ETCs. Therefore, it is expected (but not tested) that the results would be largely the same without conditioning on ETCs. However, we see this conditioning as an important physical 'glue' that connects the synoptic hazards to the seasonal large-scale flow. We have added text to the Introduction under the research questions to hopefully address this point better.

"We note that a similar analysis could be performed without restricting the hazards to those occurring within ETCs. However, the linkage with ETCs provides a robust physical connection, since the large-scale modes of variability are associated with variations in the North Atlantic eddy-driven jet speed and position, which in turn shape the baroclinic environment that controls the formation of ETCs. It is also widely known that the most severe winter weather hazards in Europe are associated with ETCs. The focus on ETC related meteorological hazards therefore provides a useful physical lens to interpret the influence of the modes of large-scale variability."