Responses to Reviewers' Comments for Manuscript egusphere-2025-6335

Identifying controls of extratropical cyclone intensity at genesis time and during intensification in the North Atlantic and Europe

Addressed Comments for Publication to

Weather and Climate Dynamics

by

Joona Cornér, Clément Bouvier, and Victoria A. Sinclair

Dear Prof. Christian Grams,

Please find enclosed the revised version of our previous submission entitled "Identifying controls of extratropical cyclone intensity at genesis time and during intensification in the North Atlantic and Europe" with manuscript number egusphere-2025-6335. The main revisions in the manuscript include a clearer explanation of the two types of intensity used, the intensity measures and intensity groups. The terminology regarding these has been checked and revised throughout the text to avoid any confusion. Text in the results section has been revised and some less relevant details have been removed and the discussion on the effects of climatology to the sensitivity signals has been moved to the supplement in an effort to make the manuscript shorter and more readable in essential parts. Overall, the manuscript is now more concise, as suggested by the reviewers.

Sincerely,

Joona Cornér, Clément Bouvier, and Victoria A. Sinclair

Note: To enhance the legibility of this response letter, all the reviewers' comments are typeset in coloured boxes. Text which has been added or rephrased in the manuscript is typeset in white boxes or quotation marks.

Authors' Response to Reviewer 1

General Comments. This study applies an ensemble-based sensitivity analysis to extended wintertime extratropical cyclones in the North Atlantic and Europe to identify precursor fields influencing maximum cyclone intensity using five different intensity measures. To quantify the controls on cyclone intensity, the authors apply the ensemble-based sensitivity analysis to multiple cyclone precursors at cyclone genesis time in a cyclone centered perspective. The study shows that the intensity of cyclones in terms of winds is controlled mostly by wind speed at upper levels and temperature gradients. Whereas cyclone intensity in terms of precipitation is mainly controlled by more moisture and warmer temperatures. As one finding of the study is that the genesis time has limited predictive value for cyclone intensity, the study additionally investigated the temporal evolution of the precursor fields. The manuscript is clearly written and will be of interest to many readers of WCD. It will make a valuable contribution to the literature on cyclone intensification. I do, however, have some comments that should be addressed by the authors, mostly regarding the length and conciseness of the manuscript. Otherwise, I think this manuscript is suitable for publication in WCD.

Response: We would like to thank you for the feedback and the valuable comments which helped improve the quality of our manuscript. We have carefully addressed all the issues item by item as follows.

Comment 1

While the manuscript is comprehensive, it is a bit lengthy. The readability could be enhanced by shortening and making it more concise to improve the overall flow. I would suggest to shift the focus more on the key findings. For example, but not limited to, Section 4 would benefit from greater conciseness and a clearer emphasis on the key new insights and main findings.

Response:

The manuscript has been shortened mostly by moving the discussion on climatological fields to the supplement from section 3 and by removing some details from sections 3 and 4.

Comment 2

It is a bit misleading that wording cyclone 'intensity' is used in two ways, first different measures of cyclone intensity, based on dynamical and impact-based measures like VO, WS850, and PRECIP are introduced. Second, cyclone types are introduced by grouping in rainy and windy groups. It would help the readability to distinguish more clearly between those two types of cyclone intensity and what the difference between those two types of cyclone intensity is.

In section 3.2 of the manuscript, the sensitivity of the genesis precursors is compared to different cyclone intensity measures for different cyclone groups (windy and rainy). In Section 4.1, the evolution of cyclone intensity based on different intensity measures is compared across the different cyclone groups. However, it seems somewhat naturally that cyclone types associated with precipitation exhibit higher intensity when precipitation based measures are used, as this is somewhat true by definition. Similarly, intensity measures related to wind impact naturally yield higher intensities for cyclone types classified as windy. Therefore, I am not fully convinced that it is meaningful to use the same intensity measures both to define the cyclone groups and subsequently to assess the influence of different precursors on those same intensity measures. The differences between cyclone groups and their associated intensity measure requires further clarification.

I do appreciate the focus on distinguishing different cyclone types and exploring how their intensity is controlled by different precursors. It might be more straightforward to compare the windy and rainy cyclone groups using an independent intensity measure, for example one based on vorticity, rather than relying on the same measures used for the grouping itself. Also, I am wondering whether the use of five different cyclone intensity measures is necessary. Would a smaller set, for example one wind based, one precipitation based, and one dynamical measure, be sufficient?

Response:

We have added and revised text in sections 2.2.2 and 2.5, which introduce the intensity

measures and intensity groups, respectively, in an effort to clarify the distinction between the two types of intensity. Included here (and a similar figure added to the supplement), Fig. R1 demonstrates the difference in nature between intensity measures (VO, WS850, PRECIP, MSLPa, FG10, and WFP; continuous distributions) and intensity groups (Dry+Calm, Dry+Windy, Rainy+Calm, and Rainy+Windy; considered categorical). We have also carefully checked and revised the manuscript to ensure that the terminology used regarding the intensity measures and groups is consistent and accurate throughout.

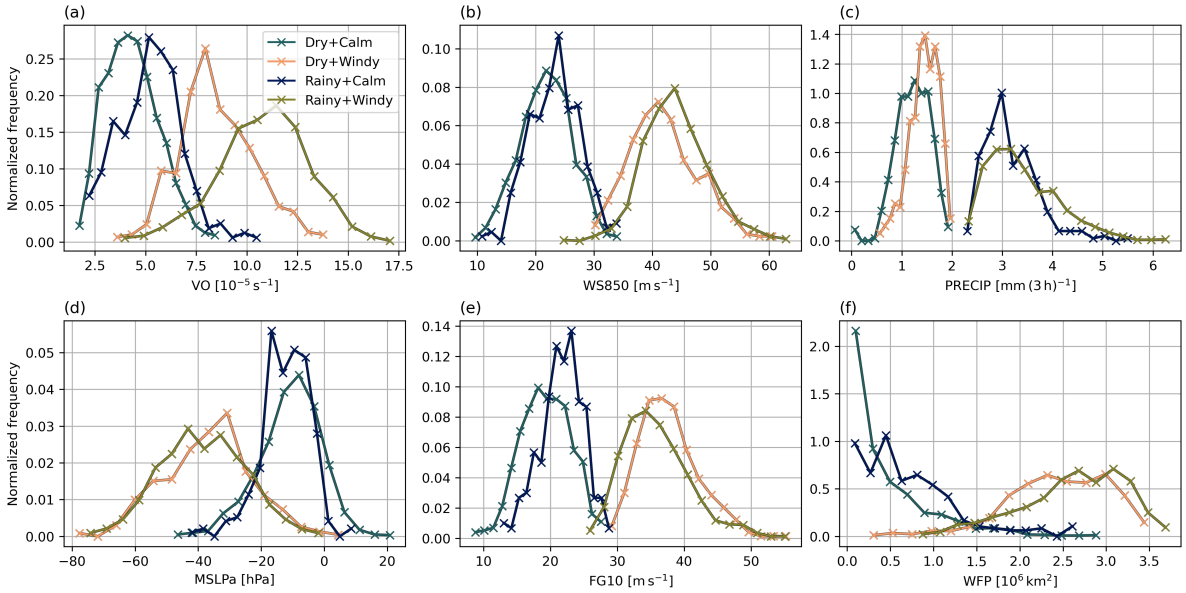

Figure R1: ETC intensity measure distributions at time of maximum intensity for the four intensity groups.

Regarding section 4.1, it is true that by definition Windy ETCs are expected to have larger wind speed values and Rainy ETCs more precipitation. However, the intensity groups are defined from the Sparse PCA with only the distributions of the intensity measures at the time of maximum intensity as input. Therefore, in section 4.1. the interest is in the evolution of intensity measures and how we end up with the differences in the intensity measures at time 0. The use of the five intensity measures is based on the results of Cornér et al. (2025). They defined the five intensity measures used here to represent the variability of ETC intensity "comprehensively and non-redundantly",

the meaning of which is now explained more clearly in section 2.2.2. We consider here the similar sensitivity signals for the wind-based intensity measures (VO, WS850, WFP, and SSI) to be a result on its own. As we describe in section 4, it is indicative of the fact that the non-linear growth in wind-based intensity measures is not represented by the ensemble sensitivity analysis, and a reason for investigating the evolution of multiple wind-based intensity measures. See also General Comment 1 by Reviewer 2.

Specific comments:

Comment 1

Title: 'controls of extratropical cyclone intensity': Maybe add something to also emphasize that different intensity types are considered as this is the focus of the study. For example, 'controls of extratropical cyclone intensity types/ groups.

Response:

Thank you for the suggestion. This actually is something we discussed when preparing the manuscript. While the analysis on the groups is an essential part of the paper, a main focus is also on the controls of intensity in the full set of ETCs, and we feel that modifying the title as you suggest would undermine this part of the research. Furthermore, an addition would make the already quite long title even longer.

Comment 2

Line 46ff: What is the point you want to make here? I am not sure the second half of the sentence is needed, or it needs to be stated how the coupled lower-level features are affected. This whole paragraph may be shortened.

Response:

The second half of the sentence has been removed due to being deemed irrelevant based on your suggestion.

Comment 3

Line 202: As you find that the genesis time has limited explanatory value on cyclone intensity, it would be great to justify why you choose to use genesis time instead of any other time. This could also be added in the introduction.

Response:

The following text has been added to the introduction:

> ESA is applied between ETC precursors at genesis time and intensity measures at time of maximum intensity. This choice is made in an effort to quantify the influence of the background environment through the minimization of the effect of the ETC's own circulation on its intensity. To understand the link between ETC precursors and intensity by using a maximal temporal offset between the two is also beneficial from a weather forecasting perspective.

Comment 4

Line 206f: Would it be possible to provide a range between genesis time and maximum intensity?

Response:

The following sentence has been added to the paragraph:

In the full dataset the genesis time relative to the time of maximum VO varies between 1 day before, which is mandated by the tracking criteria (Sect. 2.2.1), and 6 days before for 96 % of the tracks, with the median at 2 days and 3 hours.

Comment 5

Line 230: Since most of the cyclones are either in Dry+Calm or in Rainy+Windy, is there a distinction between impactful and non-impactful cyclones?

Response:

The sentences in the paragraph aims to clarify this:

It should be noted that while the application of Eq. 4 removes a majority of the "most average" ETCs from the sample, the qualitative labels given to the groups do not indicate that all ETCs in the groups have extreme values of both the wind speed measures and precipitation measures. However, the shape of the filtering function ensures that an ETC has "extreme" values in either PC1 or PC2, or more moderate, closer-to-average values in both PC1 and PC2 at the least.

This means that not all Rainy+Windy (or Dry+Calm) ETCs have extreme values in either wind speed or precipitation. Moreover, the intensity of an ETC as defined by the grouping, while correlated, is not directly indicative of any impacts.

Comment 6

Line 320: Here suddenly the following discussion is about the cyclone groups and their climatology background. This discussion could be moved further down, after the sensitivity of the cyclone groups was discussed.

Response:

In an effort to remove non-essential parts of the results from the manuscript, the climatology discussion has been moved to the supplement.

Comment 7

Line 379: With 'Precipitation' you mean PRECIP?

Response:

The word has been changed accordingly. We have also carefully revised the terminology in the paper, particularly regarding the intensity measures and intensity groups to avoid confusion.

Comment 8

Line 418: One could also argue that the sensitivity differs for ETCs with different intensities, considering that in Section 3.1 sensitivities for different intensity measures are found. Here the wording intensity could mean both, please specify.

Response:

We have added the phrase

> here defined as the four intensity groups

to clarify what is meant.

Comment 9

Line 451: It's great that MSLPa is included here, to provide another dynamical intensity measure!

Response:

Thank you, we fully agree it is useful to have multiple intensity measures.

Comment 10

Line 469f: 'The decrease in VO is almost as fast as the increase'. This sentence needs clarification on what increase is meant.

Response:

The text has been revised and now reads:

> In group Rainy+Windy VO increases very rapidly, especially in the 24 h before maximum VO and the decrease after the maximum value is almost as fast as the increase before.

Comment 11

Line 605: Please specify here, what kind of 'ETC intensity' is meant, the intensity in terms of intensity measures or the intensity in terms of type of impact, i.e. windy/rainy?

Response:

The sentence has been rephrased to specify the type of intensity as follows:

does not explain differences in the average magnitudes of the ETC intensity measures.

Authors' Response to Reviewer 2

General Comments. In this manuscript, the authors group extratropical cyclones based on their intensity (as described by various metrics) and consider the environmental precursors important for their development. Using ensemble sensitivity analysis it is shown that across all cyclones those that are identified as extreme based on wind metrics are most sensitive to the upper-level jet and low-level baroclinicity, whilst the cyclones that are extreme in terms of precipitation are more sensitive to temperature. The sensitivity analysis is then repeated for the different groups of cyclones but no clear differences are identified. Differences among the cyclone groups are found when considering the times between genesis and maximum intensity in terms of their composite evolution of the considered precursor fields. Differences between the cyclone groups for the different precursors generally agree with what one would expect from theory.

The manuscript is well written and structured and the story of the paper is easy to follow and well supported by the results presented. I think the manuscript contains some interesting results relating to the sensitivities for cyclones with differing intensity measures as well as the development of cyclones across the different classifications. These results would definitely be of interest to the WCD community. I do think the manuscript is quite long and some results could be cut without losing any impact (though I do appreciate the thorough investigation conducted by the authors). I also have some questions relating to methodological choices. I would therefore think the manuscript would be suitable for publication in WCD after the authors have addressed my comments below.

Response: We would like to thank you for the feedback and the valuable comments which helped improve the quality of our manuscript. We have carefully addressed all the issues item by item as follows.

Comment 1

This comment is regarding the use of intensity measures. The authors consider five intensity measure in the manuscript, with four based on wind speed and one on precipitation. I do not think there is much gained by showing/discussing results for all four wind-based measures: often the results are very similar comparing the wind based measures, e.g. the ensemble sensitivities. This is an obvious place to me to reduce the length of the manuscript without losing any impact. Focusing the results on one wind and one precipitation intensity measure could be sufficient.

Response:

We are of the opinion that the inclusion of more than one wind-based intensity measures enables easier comparison of our results to other studies. Moreover, we consider here the similar sensitivity signals for the wind-based intensity measures to be a result on its own. As we describe in section 4, it is indicative of the fact that the non-linear growth in wind intensity is not represented by the ensemble sensitivity analysis, and a reason for investigating the evolution of multiple wind-based intensity measures. See also General Comment 2 by Reviewer 1.

Comment 2

Choice of PV precursor. I was surprised that only one PV precursor was considered and the development of the cyclones seems largely insensitive to it (PV is not really mentioned in section 3.1). Do you have an explanation for this given that the magnitude and structure of PV anomalies varies a lot across different cyclones? It may be because you consider only PV anomalies and at only one pressure level. Have you included other levels or the full PV field to see if any differences appear? One would think the differences found for wind speed, baroclinicity, etc would be reflected in the PV.

Response:

We were in fact somewhat surprised by the small sensitivity values of PVa300. Partly motivated by this, other PV precursors such as averages between 900 and 700 hPa, and 700 and 500 hPa as well as their anomalies were included in the analysis but their composites and/or sensitivity fields were noisy and the sensitivity values were even smaller than for PVa300. The lower-level PV anomalies are not shown but are briefly discussed in section 4.2. An example of composite PV anomalies averaged between 500 and 700 hPa is shown in Fig. R2. We only get distinct values close to the time of maximum vorticity near the ETC centre, with the largest values in group Rainy+Windy. The reason for small sensitivity values from PV anomalies are possibly related to it having more influence on the intensification only later on (e.g. 24 h before maximum intensity). This may be related to the scale and/or transience of the PV anomalies compared to e.g. jet speed or temperature gradients.

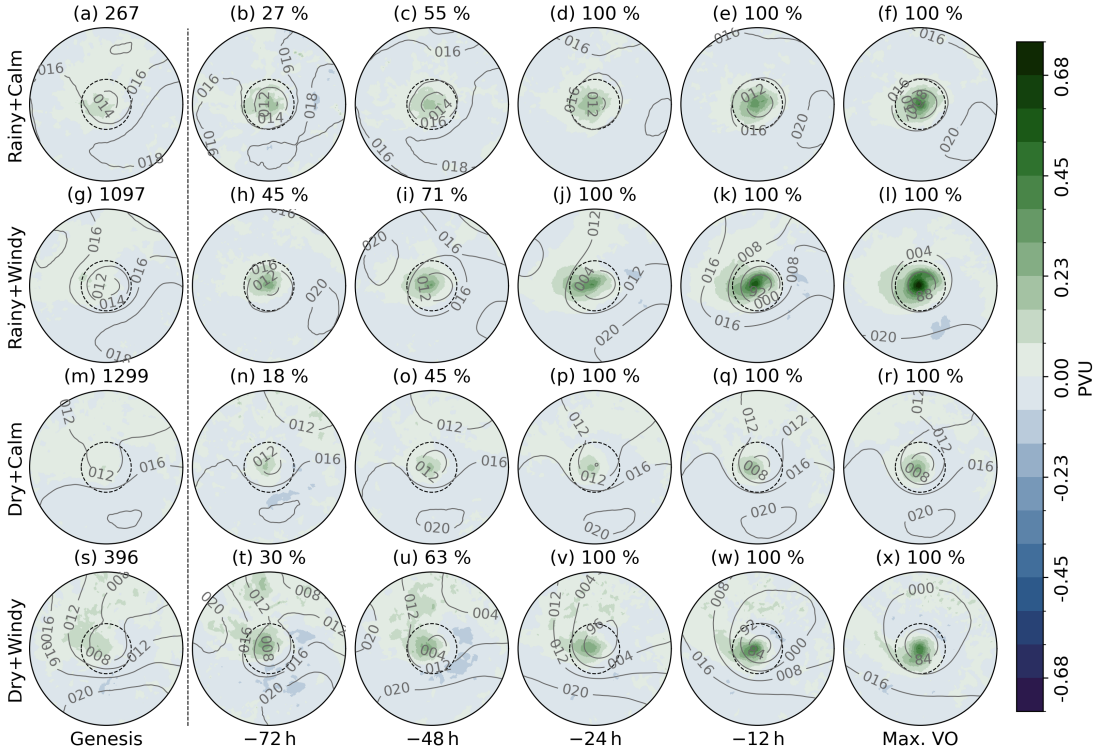

Figure R2: Composite evolution of PV anomalies averaged between 500 and 700 hPa in the four intensity groups.

Comment 3

This comment relates to the cyclone groupings. You define the cyclone intensity measures on mostly, I believe, absolute values of e.g. wind or precipitation, and then group those that are extreme in one (or both) of these measures. The storm track densities for the different groups therefore largely correspond to regions that are climatologically windier or wetter (for example a southward shift in the storm track when comparing rainy vs dry). If you defined the intensity metrics on local percentiles/extremes do you think the results of the study would change much? I realise it would be a lot of work to redo the analyses in this way and do not necessarily suggest you do that, but your insight onto the potential impacts would be useful.

Response:

This is a very interesting question. One intensity measure which is not included in the ensemble sensitivity analysis but is included in the temporal evolution analysis, the mean sea level pressure anomaly (MSLPa), is based on local climatological values. It shows anomalies from the local monthly mean values and therefore does not include the effect of the latitudinal variation in the climatological MSLP field. We do not calculate the sensitivity signals for MSLPa but based on its statistical distribution and strong correlation with VO, WS850, and WFP (Cornér et al., 2025), we would expect the results to be similar to those obtained for these three intensity measures.

The storm severity index is based on the exceedance of local climatological extreme wind gust values. In addition, it is not as strongly correlated with the other intensity measures and is not present in any of the Sparse PCA components (Cornér et al., 2025). Therefore, it should be fairly a independent measure compared to the other wind-based intensity measures and in terms of the intensity grouping. Based on the sensitivity patterns in section 3.1 it behaves similarly to the other wind-based intensity measures. In section 3.2 its sensitivity signals for the intensity groups are largely nonsignificant so it is difficult to comment on this.

Based on the consistent sensitivity signals we observe for PRECIP both in the full dataset and among the intensity groups, we could expect similar behaviour for precipitation distributions relative to local climatology. Also, theory guides us to expect that precipitation, extreme or average, is largely dependent on temperature and moisture. This is true especially in northern latitudes where precipitation is limited by moisture availability (Pfahl and Sprenger, 2016).

Other comments:

Comment 1

Did you consider reproducing figures 7 through 10 but with ensemble sensitivities that could complement the composites? This may reveal clearer differences across the cyclone groups.

Response:

This is something we considered and actually did for certain time offsets. The sensitivity signals do show larger values and clearer patterns for e.g. 24 h before maximum intensity. This approach however does not align with the aims of quantifying the influence of specifically the background environment while minimizing the effect of the ETC's own circulation. See also Specific Comment 3 by Reviewer 1.

Comment 2

Seasonality. Do the groups of cyclones occur preferentially in different periods during the extended winter? Do their sensitivities also change across the season?

Response:

There is some variation among the groups in the seasonality of cyclone occurrence as shown in Fig. R3. For example, Rainy+Calm ETCs are more common in the beginning of the extended winter and decrease in occurrence towards late winter whereas Dry+Windy exhibit the opposite behaviour.

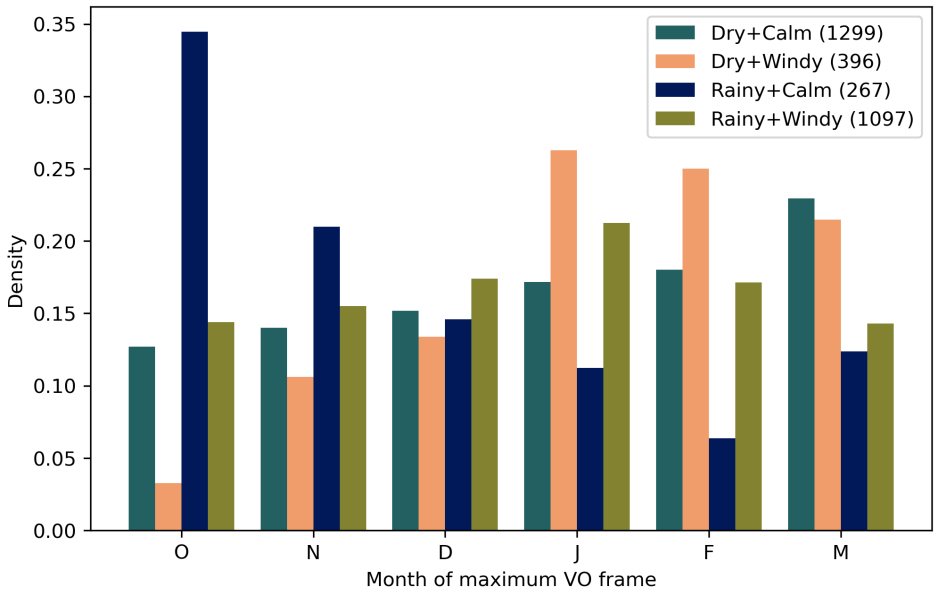

Figure R3: Seasonality of occurrence of ETCs in the groups as proportions of the total number in each group (which is shown in parentheses in the legend).

However, if we look at the proportions of tracks in each group during ONDJFM vs. DJF as shown in the table below, there are slightly more Windy than Calm tracks during DJF, but the difference is small. Based on this, we would expect to see little change in the sensitivity signals throughout the season.

	ONDJFM	DJF
Dry+Calm	42.46 %	40.67 %
Dry+Windy	12.95 %	15.92 %
Rainy+Calm	8.73 %	5.35 %
Rainy+Windy	35.86 %	38.06 %

Comment 3

I wonder if figure 5 would be easier to interpret if you split the panels by cyclone group rather than intensity metric? I.e. remake figure 4 for each of the cyclone groups? Also, why do the numbers shown in the tables change from percentages in figure 4 to absolute values in figure 5? This also makes it harder for comparison.

Response:

This is something we considered while composing the manuscript but in the end decided to use to format shown in the preprint for the following reasons. The numbers in Fig. 4 are shown as percentages because the units of the sensitivities are measure-specific, i.e. the unit of the given intensity measure. Therefore, they are not comparable as absolute values and not representable when using a single colourbar in a single figure. It would be possible to remake Fig. 5 in similar manner but then one would lose the ability to compare the sensitivity values for a given intensity measure between the groups (now in a single panel as absolute values).

Minor comments:

Comment 1

L132-135: Are the results sensitive to the choice of pressure levels here?

Response:

Temperature fields are investigated at two pressure levels and the surface. For the temperature fields the results are fairly similar and no added benefit or insight is foreseen to be gained from other levels. For PV, the answer is covered in General Comment 2.

The only field on a pressure level which was used on only one level is wind speed at 300 hPa. This field represents the jet stream which can be considered to be a feature of large

enough scale that the selection of another suitable pressure level is unlikely to affect the results (e.g. 250 hPa).

Comment 2

L160-165: how is the direction of propagation calculated? Is it an average across the whole storm track or does it change between each tracked point?

Response:

The following text has been added to clarify the way the direction of propagation is calculated:

> The direction of propagation is calculated separately for each point of interest. For a genesis (last) point this is just the angle between the first and second (second-to-last and last) point in the track. For other points it is the average of the angles between the point of interest and adjacent points, i.e. point before and point after.

Comment 3

L207: what do you mean by a "statistically consistent time period? Please clarify.

Response:

We have added the following sentence to clarify:

> This means that – assuming that there is a meaningful relationship between the genesis environment and the eventual maximum value of an intensity measure – we are comparing the same features against each other for each ETC in the sample.

Comment 4

L290-295: Does this have climate change implications? I.e. in a world that is generally warmer and moister ETC impacts are stronger? [I see you highlight this in the conclusions but might be worth mentioning here.]

Response:

For reasons of brevity, we retain the discussion on climate change implications to the conclusions and do not add any comments here.

Comment 5

Fig6: When discussing figure 6, a lot of detail is included comparing the different groups when the differences are often small and well within the spread and therefore may not be robust. Consider removing/rephrasing some text.

Response:

The text has been revised and parts discussing differences within the spread have been either removed or clarified with a mention of this caveat.

Technical corrections:

Comment 1

Fig. 3 caption: I'd suggest highlighting that the sensitivities are for the cyclones at the time of maximum intensity.

Response:

Thank you for the suggestion. This information has been added to the caption.

Comment 2

L474: maybe change larger to more strongly negative as larger might be confused with more positive?

Response:

The sentence has been removed completely due to other revisions in the section.

Comment 3

L493: “have the largest deepening rates” —> deepen fastest?

Response:

We think that the original wording is more suitable since deepening rate is the metric which the paper cited in the sentence investigated.

Comment 4

L663: Mediterranean are —> Mediterranean area

Response:

Thank you for noticing the typo. It has been fixed accordingly.

References

J. Cornér, C. Bouvier, B. Doiteau, F. Pantillon, and V. A. Sinclair. Classification of North Atlantic and European extratropical cyclones using multiple measures of intensity. *Nat. Hazards Earth Syst. Sci.*, 25(1):207–229, 2025. doi: 10.5194/nhess-25-207-2025.

S. Pfahl and M. Sprenger. On the relationship between extratropical cyclone precipitation and intensity. *Geophysical Research Letters*, 43(4):1752–1758, 2016. doi: 10.1002/2016GL068018.