The authors sincerely thank the Editor and the reviewers for their careful, constructive, and insightful evaluation of the manuscript. The comments and suggestions have substantially improved the clarity, structure, and scientific rigor of the study, and have also helped to identify directions for further development of this research.

All comments from Reviewers 1 and 2 have been carefully considered and addressed in the revised manuscript. All modifications are highlighted in the revised manuscript using tracked changes. Updated figures and tables have been included.

Review #1:

General comments:

1: Some reorganization would improve the overall storyline of the manuscript. The current Theory section could be condensed and integrated into the Introduction. I also recommend moving and shortening Section 4.4 Storm damages as well as part of Section 4.2 Pseudo-global warming and storyline approach into the Introduction. Lines 123–130 may also be placed in the corresponding sections of 4.1 and 4.2. In addition, Section 2 The storm Anatol, viewed from a Danish perspective would fit better either between the Methods and Results, or within the Results section.

1. The authors agree with the reviewer that restructuring improves the logical flow of the manuscript. The manuscript has been reorganized accordingly as follows:

- The former Theory section has been substantially condensed and integrated into the Introduction, with a clearer focus on the conceptual framework relevant to the study objectives.
- The section on *Storm damages* has been moved to the Introduction to better motivate the definition of the cumulative wind exposure index (CWEI). This section has also been shortened to reduce redundancy.
- The description of the pseudo-global warming and storyline approach, previously presented in Section 4.2, has been revised and relocated to the Methods section, where it more directly supports the experimental design.
- The background description of Storm Anatol has been moved to immediately follow the model setup description in the Methods section, thereby aligning the meteorological context with the simulation framework.

2 The manuscript would benefit from greater consistency in terminology and notation. For example: extratropical low-pressure system (Line 17), extratropical cyclone (Line 330), and extratropical storm (Line 340) are all used; 'threshold' and 'limits' are used interchangeably, including in figures; time units are given as GMT (Lines 74, 80) and UTC (from Line 235 onward and in figure captions); the terms (mean) wind speed and (wind) velocity are not clearly distinguished (e.g. 'maximum mean wind speed' vs. 'maximum wind speed').

In addition, it should be clarified whether Ref and Control refer to the same experiments. Ensuring consistency throughout the manuscript would improve readability.

2. The authors thank the reviewer for identifying these inconsistencies. The manuscript has been thoroughly reviewed, and terminology has been standardized throughout (e.g., storm, threshold, control run).

3 It is not clear why the authors sometimes use the entire model domain and other times only the land area of Denmark to compare results. For a holistic perspective on the storm, it would be helpful to also present the numbers for the entire storm, not just Denmark.

3. The authors acknowledge the reviewer's comment. The analysis has been clarified to reflect that the primary focus of this study is the storm impacts over the land area of Denmark. At the same time, the broader storm evolution has been more clearly discussed, while Denmark remains highlighted in the CWEI analysis.

4: A limitation of your pseudo-global warming (PGW) approach is that it uniformly adds temperature, thereby neglecting projected changes in vertical temperature profiles and meridional temperature gradients. Is it possible to account for these in the experiments, or alternatively, could the authors explain why the PGW approach remains reasonable? In Line 167ff, it is stated that "a sensitivity test was conducted on vertical temperature lapse rates and the overall large-scale north-south temperature gradient." It is not entirely clear what was done in this test. If some simulations with more realistic warming of vertical temperature profiles and north-south temperature gradients were conducted, this would be an interesting aspect and should be included or discussed in this study. A more detailed explanation of what is meant by sensitivity test would be helpful.

4. The authors thank the reviewer for this important observation. The limitations of the pseudo-global warming (PGW) method, specifically the use of a uniform temperature perturbation and the neglect of projected changes in vertical lapse rates and meridional temperature gradients, are now explicitly acknowledged in the manuscript. The description of the sensitivity test has been revised to clarify what was done and to explain why the PGW approach remains a reasonable first-order method in this context. Previous sensitivity studies by Matte et al. and Zeitzen et al. are now more clearly referenced and discussed

5: It is quite a strong statement to say that the overall intensity of the storm increases by using such a large domain's maximum mean wind speed and gusts for 5 simulations (Line 242, Table 1 and Figure 3). As the author mentioned the linear relationship is not large. Is the result significant? In Table 1. It becomes evident that for the -2°C the maximum wind gust in the domain is lower than for the +1°C and almost similar to the Ref. Also, for the -1°C the max mean wind speed is higher than for the Ref and the +1°C wind gust is very similar to the +3°C. Can the author discuss this result and explain? It may also be helpful to include additional metrics for cyclone intensity for example minimum core pressure (only mentioned for Ref and

+3°C) or maximum vorticity? Is the footprint of the storms changing for different warming levels?

5. The authors thank the reviewer for this valuable comment. The manuscript has been revised to moderate the original statement regarding an overall increase in storm intensity. It is now clearly stated that the maximum domain-wide wind speeds and gusts do not exhibit a strictly monotonic response to temperature perturbation. However, when considered collectively, the results indicate a tendency toward higher intensities with warming, with regression coefficients of determination of $R^2 = 0.78$ for maximum mean wind speed and $R^2 = 0.58$ for maximum gusts. The revised text emphasizes that, due to the limited number of simulations, these results should be interpreted as indicative rather than statistically conclusive.

Specific comments:

Line 7: The authors state 'with increased near-surface temperatures and atmospheric moisture contributing to stronger storm dynamics.' However, in this study not only near-surface temperature, but the temperature of the entire atmosphere was modified, which makes the statement somewhat misleading. Furthermore, it is not clearly demonstrated in this study how atmospheric moisture contributes to stronger storm dynamics.

Line 7: The authors thank the reviewer for noting this. We agree that the original phrasing was misleading, as the applied PGW perturbation modified the temperature of the entire atmospheric column rather than only the near-surface layer. The sentence has been revised.

Line 8: The term 'systematically' may be too strong, as the +2°C simulation also shows instances of lower wind speeds and gusts.

Line 8: The term has been modified.

Line 26 and Line 32: It is not clear how the reported 24m/s value relates to the 24.5m/s used as the threshold in the subsequent analysis. Could the authors clarify the reasoning behind choosing 24.5m/s later in the study?

Line 26/32: We thank the reviewer for pointing out this potential source of confusion. To clarify, the 24 m/s threshold is applied because it corresponds to the Eurocode guideline for house construction, whereas the 24.5 m/s threshold represents the meteorological definition of a storm (based on 10-min mean wind speed). Text has been rewritten to make this clearer

Line 33: A reference to the commonly used threshold for defining storm events is missing.

Line 33: We thank the reviewer for pointing this out, and have added the proper reference.

Line 34: Here a reference to the 'storms of this magnitude are relatively rare' is missing.

Line 34: Sentence has been removed, due to editing of section, should it still occur, we will make the proper reference.

Line 51: How is the study different form the cited study investigating the same storm (Zeitzen et al. 2025)? Highlight the novelty of the study more.

Line 51: The authors thank the reviewer for pointing our attention on this issues, as it is important to clarify that this paper differs from Zeitzen et al, which is more meteorological oriented in contrast to this paper being more focused on the consequences of storm and the introduction of CWEI. This has been made more clear

Line 57: Could the authors give a brief explanation of the pseudo-global warming method, and the general idea of this approach? What are advantages of this method?

Line 57: The authors thank the reviewer for highlighting this point. A brief explanation of the PGW approach has now been added to the manuscript. The PGW method is used to assess the potential impact of climate change on specific weather events by perturbing the atmospheric conditions of an observed or simulated historical event according to projected climate-change signals, typically derived from global or regional climate model ensembles. This entails adding temperature and moisture increments to the initial and boundary conditions while retaining the large-scale circulation of the original event. A key advantage of the PGW approach is that it allows the response of an individual storm to warming to be examined without simulating the full climate system, thereby isolating thermodynamic effects while preserving the event's dynamical structure. The method is computationally efficient and well suited for high-resolution regional modeling.

Line 59: 'However, they pose certain challenges. Extreme storms are inherently unique, forming due to the precise alignment of multiple meteorological and climatological drivers (Ulbrich et al., 2009). When modifying atmospheric conditions to reflect a warmer climate, there is a risk of altering the storm's fundamental dynamics, potentially reducing its extremity rather than amplifying it (see e.g. Matte et al. (2022)' may be moved to the discussion or justify why you still use this method.

Line 59: The authors agree that this belongs later in the manuscript. This text has been relocated to the Discussion.

Line 72: Here you write 'temperature contrasts', perhaps write 'temperature gradient'?

Line 72: This has been corrected. The term **"temperature gradient"** is used for clarity and consistency with atmospheric dynamics terminology.

Line 76: The phrase 'based on the simulations from HARMONIE-AROME' introduces the simulation experiment for the first time, which makes it appear abrupt.

Line 76: The sentence has been rewritten to introduce the simulation setup more gradually and refer forward to the Methods section. The transition into discussing the modelling framework is now clearer.

Line 76: Which method was used to determine the storm track?

Line 76: We use the center of the low pressure to determine the track; this has been made clearer in the manuscript. A new figure is introduced.

Line 68: What kind of 'atmospheric disturbances'?

Line 68: The text clarifies the atmospheric disturbances as a surface baroclinic wave located northeast of Newfoundland and an upper-tropospheric shortwave disturbance originating from Labrador.

Line 81: Later in the results are you also using 10-minute mean wind speeds? Please clarify later, when explaining the data used.

Line 81: A clarification has been added to the paper indicating that 10-minute mean wind speed is used for consistency with DMI warning thresholds and damage estimation, and this is now explicitly stated where the wind data are introduced.

Line 126: A reference to the ERA5 is missing here.

Line 126: A reference to Hersbach et al. (2020) is added.

Line 133: On what grid is the model simulated?

Line 133: This is clarified, explaining that the model runs on Lambert grid.

Line 136: Please state here the exact extend of the domain.

Line 136: The exact geographical domain boundaries is stated in the text and figure-text.

Line 140: To what is ERA5 interpolated to?

Line 140: This has been edited to state: The ERA5 data are horizontally and vertically interpolated to the HARMONIE-AROME model grid and vertical levels.

Line 143: What time period is the simulation exactly? Pleases also include hours of the start and end.

Line 143: This has been added with the simulations cover the period 2 December 1999, 22:00 UTC to 4 December 1999, 12:00 UTC.

Line 145: For better understanding move this sentence to Line 140.

Line 145: The sentence is relocated accordingly.

Line 151: A reference is missing here.

Line 151: The reference added.

Line 151 to 155: This part should be mentioned in the introduction already and can be removed here.

Line 151-155: This content has been relocated to the Introduction to improve narrative flow.

Line 154: As I understand Brogli et al. (2023) also adapt geopotential and winds?

Line 154: This is clarified in the text.

Line 159: Please expand the description of adjustment of specific humidity with one sentence, as it is an important part of your method.

Line 159: Thank you, for pointing our attention to this. One sentence has been added to expand the explanation of the humidity adjustment, with reference to the supplementary material for full details.

Line 160: 'unmodified reference run' refers to the 'The control simulation uses unmodified ERA5 temperatures' (Line 145)? Please keep consistency throughout the study to refer to the control simulation.

Line 160: This has been addressed. The manuscript will consistently refer to this experiment as the Control simulation, which uses unmodified ERA5 temperatures.

Line 164: This sentence needs further explanation to clarify its meaning. According to Matte et al. (2022), PGW-Uni differs from PGW-GCM. If I understand correctly, your study uses PGW-Uni. This distinction should be made clearer in order to avoid confusion.

Line 164: The relevant paragraph has been rewritten to specify, that this study applies a PGW-Uni approach and to explain briefly how this differs from PGW-GCM approaches.

Line 165: I don't think you need to introduce the abbreviation for GCM.

Line 165: The abbreviation is removed to streamline the text.

Line 171 to 184: This section is rather long and would fit better in the Introduction. Regarding the phrase 'and identify the mechanisms responsible can be explored' this would indeed be valuable, but it is not actually carried out in this study. In addition, the meaning of 'primary and secondary processes driving the storm's behavior' is unclear. Why is this statement included here, given that the manuscript does not provide a detailed discussion of different processes leading to more intense winds? I suggest that Lines 176–180 can be moved to Section 4.1 Harmonie Model and data.

Line 171-184: The section is now shortened and moved. The broader context and motivation has been presented in the Introduction, and the Methods section focus solely on the experimental implementation.

Line 193: Here it is not quite clear what the difference between the predefined limit and the threshold is? Can you state the number of the threshold used in this study? It would be helpful to only use 'threshold' or 'limit' throughout the study and the figures (title, caption, ...). Where do you normalize in that equation?

Line 193: The term "threshold" is now used consistently throughout the manuscript and in all figure captions. The threshold used in the study (24.5 m/s) is explicitly stated, and the normalization step in the CWEI calculation will clearly be explained in the Methods.

Line 197 to 211: This part of the section is rather lengthy and could be shortened. Since it belongs to the Methods section, it currently contains too much discussion of literature.

Line 197-211: This section has been shortened. Only the methodological content is retained; literature discussion is relocated to the Introduction.

Line 212 to 229: At present, this section contains too much discussion of literature. It should either be focused more clearly on the method, explicitly describing what was done, or moved to the Introduction. The use of the power damage could then be explained in the Results section accordingly.

Line 212-229: This section has been edited, with its conceptual framing moved to the Introduction, while methodological details remain in the Methods. The application of the power-law damage relationship is presented in the Results.

Line 234: With intensity you refer to, wind speed? Storm intensity can also be determined with minimum core pressure or maximum vorticity. Please clarify what you mean.

Line 234: The text is revised to explicitly state wind intensity.

Line 234: Why not include the other warming scenarios?

Line 234: The full set of warming scenarios is presented in the Appendix. The Results text now refers more explicitly to these.

Line 235: msl is mean see level pressure (mslp) presumably, but please define.

Line 235: This is corrected to mslp.

Line 237: 'Notably, the center of the low-pressure system shifts eastward with each degree of warming', do you refer to the peak intensity timestep of the storm? Could you plot the storm tracks of all the simulations? + Line 257: Is it possible to show the storm track for the different experiments?

Line 237+ 257: New plot showing the mslp for 3 run at different times is added.

Line 238: In this sentence it is not quite clear, what is meant with 'there is', what do you refer to here? I assume you refer to the different simulations. Maybe rephrase this sentence: 'The central pressure of the low-pressure system at peak intensity (?) is with 952hPa slightly lower in the +3°C warming simulation compared to the Ref simulation with 954hPa.'

Line 238: Sentence has been modified to clarify.

Line 241: The model domain is not introduced, what is the extend of the model domain? Is the maximum mean wind speed from the complete simulation period or just one timestep? With mean wind components you mean 10 min averages and with gust instantaneous wind speed? How are the maximum wind gust values from the model? Are you referring to 10-minute wind speeds and 10m winds?

Line 241: The domain boundaries, averaging periods, and difference between 10-minute mean winds and gusts has been explicitly stated in the paper.

Line 245 ff: It is not quite clear what various methods described above you are exactly referring to? Maybe introduce the methods here shortly.

Line 245 ff: These referrers to a section now deleted, the method is only described briefly.

Line 249: Figure 4, why only show the time lapse for the Danish land and not the entire storm affected area? + Line 256: How would those numbers look for the entire area the storm is affecting?

Line 249+256: The manuscript explains the rationale for focusing on Denmark. New plot/results for a greater area will be added.

Line 261: Over what time period with what resolution are the CWEI values calculated? How many grid points are taken into account?

Line 261: Details on grid resolution, number of grid points, and accumulation period has been added.

Line 266: For what area is the maximum wind speed?

Line 266: The maximum speed used is from over land only. This is made more clear in manuscript

Line 268: You have already introduced the abbreviation for pre-industrial (PI) in Line 129.

Line 268: The duplicate introduction of the abbreviation for pre-industrial (PI) has been removed.

Line 279: It would strengthen the manuscript to link the mechanisms that cause a storm intensification under a warmer climate. How are the different mechanisms influenced by

warming, and what effects might this have on the storms? Can you provide insights into which mechanisms lead to stronger winds in your experiments? In Lines 269–271, you offer hypotheses explaining why stronger winds occur in the -1°C experiment. Extending this discussion to the other experiments, and showing which mechanisms cause the differences, would be highly informative.

Line 279: Mechanisms linking intensification to warming: The discussion of mechanisms is expanded and clarified.

Technical Corrections, Tables and Figures

Figure 1: Please, add the extend of the model domain in the caption.

Figure 1: Caption now includes the geographical extent of the model domain (lat/lon)

Figure 2: Remove the title of the figure and state the date and time in the captions of (a) and (b). What are the black contours. At the colorbar of (a) what do you mean with 'Reference Wind Speed'?

Figure 2: Title has been removed and added to caption, black contours will be defined and colourbar text has been edited.

Table 1: What do you mean with maximum mean wind velocity (why not called speed anymore?) Mean over what time and what domain? Maximum over what time and domain? + Figure 3: Absolute maximum wind speed over what time and domain? .. wind = .., an equal sign is a bit misleading here. Typo in caption 'pr' → 'per'. Table 1 mainly provides further support to the results shown in Figure 3 and can therefore be removed or integrated into Figure 3.

Figure 3 + table 1: Edited together, typo corrected, text is modified to clarify.

Figure 4 and Figure 5: The information in the title could be put in the captions. I am wondering why only the Danish part is shown. Would be nice to see the entire storm affected area. Figures 4 and 5 basically show the same information. Consider removing Figure 4. Same applies for Figure B1 and B2. Typo in 'colourbar' → 'colorbar'.

Figure 4 + 5: Spelling is standardised to British English throughout (colourbar → colourbar) as journal is European. Title is removed and a plot moved to appendix with a new plot including a bigger area has been added..

Table 2: Percentage of what region land cover, Denmark? What is the total number of grid points used for the CWEI? Over what timeperiod was the CWEI calculated?

Table 2: The caption now clarify the region referenced (percentage of Denmark land cover), the total number of grid points used for the CWEI, and the time window over which the CWEI was computed.

Figure 6: Remove the title of the figure and put information in the caption. Why is here the 24m/s threshold displayed and not the 24.5m/s? Again, the equal sign is misleading as you state the change per/°C warming? Please add the right unit to the slope of the curve. In the caption it would be great to state over what domain and time period the CWEI was calculated. Same applies to Figure C1.

Figure 6: We have removed the title and add details in caption, Threshold is clarified, it was a typo, units is altered.

Figure 7: This is a nice figure to see the temporal evolution. The maximum mean wind speed was taken over what area? Denmark or model domain? Mean wind speed refers to hourly averages? Could you also plot the CWEI value at each timestep?

Figure 7: A timestep series of CWEI is not shown, as CWEI is cumulative by design. This isbe described more thoroughly in the text. The domain is stated.

Figure 8. What time period was used?

Figure 8: Caption now includes the exact time period used.

All small typographical issues and citation punctuation problems raised by the reviewer will be corrected.

Line 52: Typo 'Allan (2023) (von Storch et al., 2014)' → 'Allan (2023, von Storch et al., 2014)'

Line 55: Start a new sentence after 'remain similar'

Line 62: In the citation on pair of () to much → '(e.g. Matte et al., 2022)'

Line 70: This citation should be in () -> '(e.g., Ulbrich et al. 2001)'

Line 80: In the citation one pair of () to much → '(e.g. Zeitzen et al., 2025)'

Line 122: 'Data' → 'data'

Line 131: 'Model' -> 'model'

Line 157: 'Initial Conditions and Lateral Boundary Conditions' → 'initial conditions and lateral boundary conditions'

Line 167: A space missing between '... (Matte et al., 2022). In addition ...'

Line 178: The sentence: 'As demonstrated in Zeitzen et al. (2025), in part due to the high resolution of the simulations.' is incomplete.

Line 397: In the citation is a ']' too much.

Reviewer 2.

1: Section 3 (Theory) feels somewhat out of place and unnecessary in a manuscript of this type. Explaining theory of storm formation is well established, i was expecting something more on how climate change alters storm formation as this would directly relate to this studys findings. Consider re-writing with this information.

1: We thank the reviewer for this helpful comment. We agree that the previous version of Section 3 was too general and did not sufficiently connect to the context of climate change and the results of our study. In the revised manuscript, Section 3 has been substantially rewritten to focus specifically on how large-scale thermodynamic and dynamic changes under warmer climates influence extratropical cyclone development**,** including changes in baroclinicity, moisture availability, latent heat release, and jet stream characteristics. This directly aligns the theoretical discussion with the interpretation of our perturbation experiments. The general description of storm formation is moved to the Introduction for background, and redundant text is also removed.

2: Section 4 - you should mention that you also include SST perturbations as i initially read this as just atmospheric perturbations, which raised initial concerns for me until i saw this later in the manuscript.

2: We appreciate this observation. Section 4 is revised to clearly state from the outset that the perturbation fields include both atmospheric temperature fields and sea surface temperature adjustments, following the pseudo-global warming framework. We have specify which warming increments were applied to SSTs and include a reference to the method used to ensure thermodynamic consistency.

3: L179/180 - please provide some evidence for the reference run of HARMONIE being consistent with observations. It not being shown concerns me that the model has significant biases

3: We agree that it is important to show the model's performance for the reference case. We have included a validation comparison of the reference simulation against ERA5 reanalysis and DMI surface station observations, demonstrating that the reference run reproduces the storm track, timing of peak winds, and spatial wind intensity structure within expected performance bounds for HARMONIE and highlighting how the model is validated, as this has been done many times before.

4: Section 4.4 - storm damages - what is the purpose of this section? You do not introduce any metric or mention anything that is used in the study (although i suspect it is for the analysis L245-248. If you are not using any storm damage metrics then this can be deleted, if you are from L245-248 then these need stating explicitly here in this section

4: We thank the reviewer for pointing out this ambiguity. The section is removed, and the description of the Cumulative Wind Exposure Index used in the analysis is moved to Methods.

The damage-related discussion will be presented in the **Results**, where the CWEI will be interpreted in connection with storm footprint and wind intensity changes.

Minor comments:

L26 - what is "basic wind speed value"? Is this a defined quantity? Could you please say more about that this is, is it a resilience measure/threshold?

L26: The text will be rewritten to clearly define this quantity as a **threshold wind speed for storm-level classification**, not a structural resilience measure.

L31 - i'd like to see more info on storm anatol here, or merge in the later section on the storm. Furthermore there should be some information on ETCs and how these extreme winds are associated with cyclonic systems.

L31: Storm Anatol will be expanded to briefly describe the storm's evolution, its associated extratropical cyclone characteristics, and links between cyclonic dynamics and extreme winds. Additional references to the ETC literature have been included.

L34 - how rare is "relatively rare"? This is quite a vague statement.

L34: The sentence will be edited out, due to refining the paper.

L38 - this section should come before the posing of the question on L36-37 as there should be initial discussion on why the climate makes storms worse before this question.

L38: Text will be reorganized; the discussion of climatic drivers will precede the research question for improved narrative flow.

L46 - the North/South contrast is more of a tri-polar pattern. See papers by Severino et al. (2024; NHESS), Little et al. (2023; NCOMMS), Priestley et al. (2024; QJRMS)

L46: The manuscript will describe the pattern as **tri-polar**, with references to Severino et al. (2024, NHESS), Little et al. (2023, NCOMMS), and Priestley et al. (2024, QJRMS).

L50 - here would be a good place for the question posed by L36-37

L50: The research question will be moved accordingly.

L59 - also the study of Ermis et al. (2024; 10.1088/2752-5295/ad4200) should be cited here and acknowledged more by the authors throughout.

L59: We thank the reviewer for pointed our attention to this interesting paper, It is most definitely relevant for this manuscript and the findings will be reflected in this work.

L79 - are the quoted sustained winds and gusts local/station observations? if so please state this and where they occurred

L79: Text now specifies that reported sustained winds and gusts refer to the west coast and South Jutland.

Fig 1. - i think it would be good to have a panel here showing the pressure evolution and you can also then overlay the HARMONIE control and the different GWL experiments to see the impact on pressure evolution

Fig1: A new panel has been created showing the pressure evolution for the reference and perturbed experiments.

L127 - +2°C - also here i was expecting to see that you also apply SST perturbations - modify this in the text as you mention it further along but not here

L127: Text is edited to state that SST perturbations are applied consistently with the atmospheric PGW increments.

Section 4.3 - is this the first time that the CWEI will be used/introduced. If so please state that, if not please include a reference

Section 4.3: CWEI is explicitly stated as a novel metric introduced in this study.

L198 - please include a reference for the 98th percentile of wind speed statement

L198: Sentence is removed during editing to streamline Methods and to avoid redundancy.

L203/204 - also include a citation here for the 24.5 m/s wind speed justification. My feeling is that 20m/s is more commonly used as a damage threshold so i'd be interested to know where this has come from.

L203/204: Text now state that 24.5 m/s is the official storm threshold**,** distinct from damage thresholds (commonly ~20 m/s). citation WHO

Figure 2 - it would also be good to have a difference panel in (c).

Figure 2: A difference panel is available; It has been added to appendix. The plot shows disturbances also due to the phase shifts in the storm events.

L231/232 - this entire sentence can be shortened/deleted as you already have this information several times throughout the manuscript.

L231/232: This is deleted to avoid repetition.

L234/236 - put brackets for your figure referencing

L234/236: Brackets is added around figure references.

L235 - i would argue that the cyclone has already made landfall at the time due to the centre being over land. Please state that you are referring to the associated wind field with this.

L235: Text is edited to clarify that the center of the cyclone is over land.

L243/244 - are these r^2 values significant? Due to their being so few points I would not be sure if that is the case or not?

L243/244: Text has been added noting that, due to the small number of simulations, R^2 values are indicative but not statistically robust.

L236-238 - which methods are you referring to here and how did you obtain these numbers? There is no explanation as to how these values were obtained. See also major comment 4 above.

L236-238: Text has been revised to describe explicitly how maximum wind and gust values were computed from the model outputs.

L253 - "time lapse" is this the correct wording here? Do you just mean "difference plot"?

L253: This is changed to **“**time difference” for clarity.

L289/290 - the trend you quote here must surely be very uncertain due to the small number of experiments you have looked at? Does this mean that a comparison with the Hawkins et al. study is fair or valid?

L289/290: Text has been revised to acknowledge that trends are uncertain due to the limited number of experiments.

L291-297 - see also Priestley et al. (2024; 10.1002/qj.4849) where changes in area were assessed and found increases of 1-2% per degC for central Europe

L291-297: Citation for Priestley has been added including comments regarding this.