

Author's response to Reviewer 1
#Reviewers comments in blue, our responses in black

Re-review of Climate Modes Synergistically Influence Marine Heatwaves in the North Sea. Thank you to the authors for addressing the previous comments. I have only a small number of additional comments to make:

Response: Thank you very much. We carefully considered each of your comments and suggestions, and we would like to respond to them in the following content.

- Line 318 – do you mean $p < 0.1$?

Response: Yes, we confirm that $p < 0.1$ here. The purpose is to indicate that the correlations after 2013 are no longer statistically significant, corresponding to $p > 0.1$. To avoid confusion, we have rephrased the sentence as follows:

L317-L318: *"However, this teleconnection substantially weakens after 2013 (Fig. S2), and the correlations are no longer statistically significant ($p > 0.1$), indicating a reduction in Pacific influence on North Sea MHWs."*

- Line 377 – the text says ‘intensification of cyclonic wind conditions’, but it looks more like a weakening to me.

Response: Thank you for the careful reading. Indeed the original wording was incorrect, and we have revised the sentence to:

L376-L377: *"In November, the system enters a transition phase. The region of SST cooling expands northward and eastward, accompanied by a weakening of cyclonic wind anomalies over the mid-latitude North Atlantic (Fig. 5b)."*

- Line 386 – suggest removing ‘of the following year’, or replace ‘following’ with ‘current’, as otherwise it suggests that the plot spans a period greater than a year.

Response: Thank you for the suggestion. We agree that the original wording was potentially confusing. The figure caption has been revised to *"from October to January"*, with the year transition clarified in the accompanying text, to avoid any ambiguity.

- Line 411 – was ‘deepening’ intended instead of ‘shoaling’? In Fig 7b, it looks like the shallowest regions have some deepening of MLD and the shoaling occurs in the deeper seas off the coast of Norway.

Response: We agree that the original wording was inaccurate. The text has been revised to:

L409-L411: *“Consistent with this background state, composite MLD anomalies (Fig. 7b) are negligible across most of the Cluster 1 region, with positive anomalies confined to very shallow shelf regions.”*

- Line 421 – linked to the comment above, is it correct to say in this line that the MLD is shoaling?

Response: Thank you for pointing this out. We agree that the original description was incorrect. Because the MLD anomalies in Fig. 7b are predominantly positive and confined to shallow shelf regions, the previous description based on MLD shoaling and reduced upper-ocean heat capacity is not appropriate. We have revised this paragraph to accurately describe the observed MLD anomalies and to clarify that MLD changes play only a limited role compared with horizontal heat advection and the modulation of vertical mixing in the development of Cluster 1 winter MHWs.

L420-L422: *“Additionally, positive MLD anomalies are confined to very shallow shelf regions and are negligible across most of the Cluster 1 area, suggesting that changes in MLD play only a limited role in the basin-scale evolution of winter MHWs.”*

- Line 494/495 – Fig 10a does not seem to support the text about northeasterly winds and negative SST anomalies over the Caribbean Sea.

Response: Thank you for pointing this out. We agree that the wind anomalies over the Caribbean Sea are not clearly evident in Fig. 10a. We have therefore revised the text by removing the reference to the northeasterly wind anomalies and restricting the description to the SST and precipitation anomalies that are clearly supported by the figure.

L492-L494: *“During the strong positive AMV phase, negative SST anomalies and reduced precipitation develop over the Caribbean Sea (Fig. 10a-b), suggesting a coherent tropical Atlantic response associated with the downstream atmospheric adjustment.”*

Author's response to Reviewer 2
#Reviewers comments in blue, our responses in black

In this new revision the authors implemented most of the requested changes and improved clarity. While I remain not fully convinced of the clustering-based approach, I appreciate other reviewers are not critical, and the additional tests made point in the right direction.

Response: We sincerely thank the reviewer for the encouraging comments and for the constructive feedback provided throughout the review process. We appreciate your thoughtful evaluation of the clustering-based approach and your recognition that the additional analyses have improved the clarity of the manuscript and strengthened the overall presentation of our results.

I realized that many DOIs in the reference list are meaningless, e.g. starting with "https://doi.org/https://doi.org", clearly returning 404 errors. Please check and correct accordingly.

Response: We apologize for the formatting errors in the DOI links. We have carefully reviewed the entire reference list and corrected all duplicated DOI prefixes (e.g., "https://doi.org/https://doi.org"), ensuring that all DOI links are complete and functional.

Comments by line (file ATC2)

264 apparently you are using the same " ϕ " character for both geopotential and latitude, which is misleading. Please use another character for geopotential, e.g. "Z".

Response: Thank you for pointing out this issue. We apologize for the confusion caused by using the same symbol ϕ for both latitude and geopotential height in Eq. (7). We agree that this notation was misleading. We have corrected this error by replacing the symbol of geopotential height with Z in the related descriptions in the manuscript.

L257-L258: " $\psi(=z/f)$ is the geostrophic stream function, z (m) is the geopotential height."

Also explain the implications of using monthly data for this calculation, which is most likely done with sub-daily data in the cited papers.

Response:

We thank the reviewer for this helpful comment. While the original study of Takaya and Nakamura (2001) demonstrated the WAF formulation using sub-daily geopotential

fields for individual blocking events, the formulation itself is not restricted to high-frequency data. In this study, our objective is to diagnose low-frequency stationary Rossby wave propagation associated with seasonal-to-interannual climate variability rather than transient synoptic disturbances. Therefore, following previous teleconnection studies (Hou et al., 2023), we calculate the WAF using monthly mean ERA5 fields. This approach suppresses transient synoptic variability while emphasizing the persistent large-scale Rossby wave pathways relevant to climate teleconnections.

266 please explain what do you mean with "perturbed"

Response: In the original manuscript, the term “perturbed” was not clearly explained. Here, the perturbed stream function (ψ') refers to the anomalous component of the geostrophic stream function relative to the climatological mean state, which is commonly used in diagnosing Rossby wave activity flux. Specifically, ψ' is calculated as $\psi' = \psi - \bar{\psi}$, where $\psi = z/f$ represents the geostrophic stream function, and $\bar{\psi}$ is the climatological mean stream function. We have added this clarification in the revised manuscript.

L258-L259: “ ψ' is the perturbed stream function, calculated as the deviation of the geostrophic stream function from its climatological mean state.”

524 "enhanced geopotential" sounds strange

Response: Thank you for pointing this out. We have revised the wording to “positive geopotential anomalies” to better describe the anomalous circulation response.

L500-L501: “This large-scale circulation response is accompanied by a positive geopotential anomaly over the northern North Sea, where the regression patterns show reduced cloud cover within Cluster 2.”

560 "is converted into" -> "influences" or "shapes" or similar

Response: Thank you for this suggestion. We have revised the sentence by replacing “is converted into” with “influences” to better represent the physical relationship.

L514-L515: “The regional upper-ocean structure further determines how this atmospheric forcing influences MHW characteristics.”

1006 incomplete reference

Response: Thank you for this helpful suggestion. We agree that the previous reference was not essential and has therefore replaced it with a more widely cited and recent study that specifically addresses the influence of ENSO on marine heatwaves in the tropical

Indian Ocean and includes a DOI. The corresponding citation and reference list have been updated accordingly.

L909-L911: “*Saranya, J. S., Roxy, M. K., Dasgupta, P., & Anand, A. (2022). Genesis and Trends in Marine Heatwaves Over the Tropical Indian Ocean and Their Interaction With the Indian Summer Monsoon. Journal of Geophysical Research: Oceans, 127(2), e2021JC017427. <https://doi.org/10.1029/2021JC017427>*”

Hou, J., Fang, Z., & Geng, X. (2023). Recent Strengthening of the ENSO Influence on the Early Winter East Atlantic Pattern. *Atmosphere*, 14(12), 1809. <https://www.mdpi.com/2073-4433/14/12/1809>