Referee's remarks on manuscript #egusphere-2025-4429:

Intraseasonal modulation of Sea Surface Temperatures in the Tropical North Atlantic by African Easterly Waves

Marc K. Mendy [1,2], Florent Gasparin [2], Manon Gévaudan [3], Moussa Diakhaté [2], Issa Sakho [2], Julien Jouanno [1]

[1] Université de Toulouse, LEGOS (IRD/UT3/CNES/CNRS), Toulouse, France,
[2] Université Amadou Mahtar Mbow, Diamniadio, Sénégal,
[3] Centre National de Recherches Météorologiques, CNRM-CNRS, Météo-France, Toulouse, France

Correspondence to: Marc K. Mendy (marckakantemendy@gmail.com)

We sincerely thank Reviewer for their thorough evaluation of our manuscript and for the constructive and insightful comments provided. The suggestions have been very helpful in improving the clarity and robustness of the study. Below, we provide a detailed, point-by-point response to all comments.

In their study "Intraseasonal modulation of Sea Surface Temperatures in the North Tropical Atlantic by African Easterly Waves ", the authors investigate how meridional wind anomalies at time scales of 2-10 days in the Mauritania upwelling region impact the surface ocean and mixed layer heat budget in the tropical North Atlantic. They find that these wind fluctuations that are associated with African Easterly Waves (AEWs) modulate sea surface temperatures through changes in atmospheric fluxes (in particular, latent heat flux and shortwave radiation) as well as through changes in vertical mixing. The study is primarily based on model output, but a substantial part of the manuscript focuses on the validation of the model output with in-situ and satellite observations as well as reanalysis data. Overall, the study shows some interesting results and, in my opinion, could be a useful contribution toward a better understanding of air-sea interactions in the Mauritania upwelling region. However, I find several major concerns which should be considered before publication.

Major concerns:

1. The validation of the model output makes up a substantial part of the results and half of the shown figures (Figs. 1-5). While I agree that it is important to examine how well the model reproduces SST, horizontal surface winds, and vertical profiles of horizontal winds, this takes up space that could be used for more in-depth analysis (see below for some suggestions).

Response: This is the first time we use this coupled model to tackle high frequency dynamics so it is difficult not to produce a thorough validation of them. Since the revised version of the paper, including your suggestions, remains with a reasonable number of figures, we chose to keep them in the main manuscript.

2. It would be useful to include a figure showing a map with the inertial period at each grid point and the period of peak wind variability to assess how close the observed wind variability is to the inertial period and in which regions this relation is most prominent. This could strengthen the author's claim of near-inertial currents having an influence on mixed layer dynamics which is an interesting question that could be examined further this way.

Response: We agree that it would be relevant to compare the local inertial period and the dominant period of wind variability to further study the role of near-inertial waves (NIWs) in the modulation of sea surface temperature.

In this study, the contribution of near-inertial processes is inferred rather than explicitly diagnosed. Our interpretation is based on the temporal consistency between wind fluctuations, the deepening of the mixing layer, and the intensification of vertical mixing due to AEWs, as well as on the comparable magnitude of atmospheric and oceanic contributions to surface temperature trends. It is consistent with previous observational studies that highlight the role of wind-induced near-inertial motions in intensifying surface mixing (e.g., D'Asaro, 1985; Hummels et al., 2020; Plueddemann and Farrar, 2006).
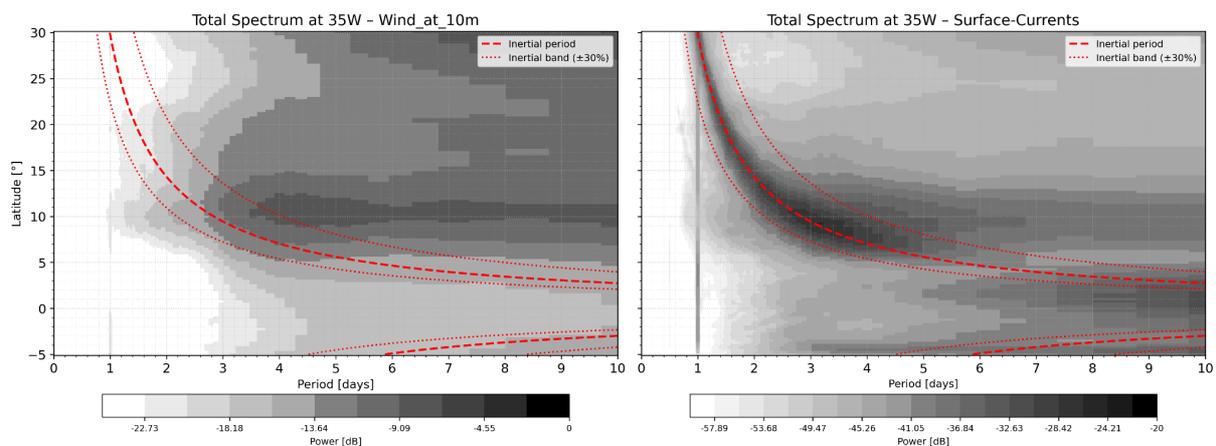
Figure R1: Latitude–period distribution of multi-taper rotational spectra of wind at 10 m (left) and surface currents (right) at 35°W during the dry season (2001–2021). The red dashed curve represents the local inertial frequency, and the red dotted curves delimit the ±30% band around this frequency.

In response to the reviewer's comment, we examined additional diagnostics provided in Figure R1 - for illustrative purposes only, comparing the latitudinal variation of the local inertial period with the spectra of surface currents and winds along 35°W. They indicate that between 5°N and 20°N, AEW-related wind variability is pronounced near the local inertial frequency and is associated with increased quasi-inertial energy (around the local inertial frequency) in surface currents. This confirms the plausibility of an AEW-quasi-inertial pathway contributing to the observed mixing signal.

We would like to emphasize that this figure is not included in the manuscript. It is intended solely to illustrate a natural extension of this work and to provide a glimpse of the follow-up study that we are currently conducting, as mentioned in the conclusion.

3. The description of the AEW index in section 4 is not quite clear to me. It would be more useful to include a formula to clearly describe which variable and which region is used.

Response: We agree that the description of the AEW index in Section 4 lacked clarity. In the revised version, the regression methodology and statistical treatment are detailed in Section 2.3, and the AEW index is now explicitly defined in section 4, clearly specifying the atmospheric variable used and the spatial domain over which it is computed.

4. In my opinion, Fig. 7 is not very convincing. Why did the authors choose only one year to show the relationship between SST and meridional winds? Why are 2–10-day meridional wind fluctuations so similar to the original meridional wind time series while SST shows substantial differences when bandpass-filtered? More discussion would be helpful here.

Response: We agree that Figure 7 would benefit from further clarification and discussion. Therefore, Section 4.1 of the manuscript has been revised accordingly.

Figure 7 illustrates the raw and synoptic evolution of the SST and the 10-m meridional wind at a single point during the 2001 boreal summer. The text now mentions that "this year was chosen as an illustrative example of the relationship between these two parameters", rather than to provide a statistical analysis.

The apparent similarity between the original and filtered (2- to 10-day) meridian wind time series is explained by the fact that, during the boreal summer, much of the variance in the meridional wind is concentrated at the synoptic scale. In contrast, SST incorporates atmospheric forcing over longer periods; bandpass filtering therefore eliminates a significant portion of low-frequency variability, resulting in more pronounced differences between filtered and unfiltered SST time series. We added these two points in the revised version.

It is also specified in the revised manuscript that, "at synoptic timescales, the SST response to wind forcing is intermittent, characterized by variable time lags of about 1-2 days and a non-linear, integrative behavior, so that a strong pointwise linear correspondence is not expected. Nevertheless, several intense southward wind events, particularly in July, are followed by surface cooling, illustrating the influence of synoptic wind variability on SST. Figure 7 is therefore intended as an illustrative example rather than a quantitative

assessment of synoptic air-sea coupling, which is addressed in the following sections using regression analyses."

5. In Figs. 8 and 9, a very locally defined AEW index (basically at the mooring location) is used to examine large-scale changes in winds and SST over the entire tropical North Atlantic. How useful is this regression in case of such a rapidly changing index?

Response: Using a local index for identifying AEWs is a common practice (e.g., Diedhiou et al., 1999; Kiladis et al., 2006). As explained in Section 4.1, it was chosen because the meridional wind at this location lies within the core AEW activity region and provides a clear and robust phase reference for AEW passage during boreal summer. The regression shown in Figures 8 and 9 allows us to capture the propagating nature and typical patterns of AEWs. The main conclusions remain robust to different index definitions - which confirms the relevance of the approach. The following sentence has been added: "Note that sensitivity tests carried out with other index sites, notably the one located at 17.5°W-15°N proposed by Kiladis et al. (2006), indicate that, despite small variations in local amplitude and statistical significance of the regressions, the large-scale spatial structures and physical interpretation remain similar (not shown) ".

6. It seems trivial that the 3–5-day fluctuations have a stronger impact on ocean surface variables because of the vertical pattern of the wind fluctuations (i.e., closer to the surface; Fig. 5) compared to the 6-9-day fluctuations?

Response: We agree that the stronger impact of 3-to-5-day atmospheric oscillations on ocean surface variables is, to some extent, consistent with their more pronounced surface wind signature compared to 6-to-9-day variability, as shown in Fig. 5. In this sense, this result seems physically expected.

However, direct wind impact is not the only mechanism that can affect SST. Modulation of solar radiation and activation of inertial waves are processes that are also involved and are not necessarily linked to the intensity of surface wind anomalies. Furthermore, the amplitude and spatial organization of the SST response are not trivial a priori, given the integrative and non-linear nature of the ocean mixing layer response.

The comparison between the two frequency bands therefore allows us to isolate the dynamic mechanisms by which AEWs influence the ocean surface and explain why only AEWs with a period of 3 to 5 days leave a clear imprint on SST.

We have revised section 5.2 to explicitly clarify that of "both types of AEWs, the results indicate that the SST response arises from a combination of air-sea fluxes and vertical mixing, which contribute in comparable proportions, although the overall amplitude of the response is significantly weaker for the 6–9-day band."

7. The authors should provide a more thorough discussion of the mixed layer heat budget based on the model output and available studies using in-situ measurements (Foltz et al., 2003; Hummels et al., 2014). This could strengthen the claim that vertical mixing plays a role.

Response: Previous studies of the mixed-layer heat budget, such as those discussed by Foltz et al. (2003) and Hummels et al. (2014), focused on seasonal variability rather than high-frequency variability. Therefore, there is no observational baseline study with which to compare, except Hummels et al. (2020) as mentioned in the following sentence : "Hummels et al. (2020) put forward the hypothesis that, in the TNA, AEWs would contribute to cooling the ocean surface, through the associated latent heat fluxes, and the strong vertical mixing at the base of the mixed layer induced by the near-inertial waves they would generate."

However, to strengthen this point, we have reformulated a sentence in the discussion: Although this mechanism is not explicitly diagnosed here, "the mixing component is consistent with a possible influence of near-inertial motions, which have been shown to enhance upper-ocean mixing in previous studies over extensive regions of the eastern TNA (D'Asaro, 1985; Hummels et al., 2020; Plueddemann and Farrar, 2006). Further investigation is therefore required to quantify the role of near-inertial activity in the mixing contribution."

8. What is the temporal variability of the relation between AEWs and SST over the examined time period? How large are year-to-year changes that make it hard to quantify a more distinct relation between AEWs and SST?

Response: We thank the reviewer for this question. The intensity of AEWs and intraseasonal variance of SST both show interannual variability over the period studied (JAS, 2001–2021), as can be seen in Figure R2. While some years show a clear correspondence between synoptic amplitudes, others show less consistent responses. So, it seems there is not a straightforward link between AEWs intensity and SST anomalies. These interannual differences could reflect variations in background conditions, such as heat content, mixed layer depth, or ITCZ position. However, we have not succeeded in understanding what could explain this non-linear response of the SSTs.
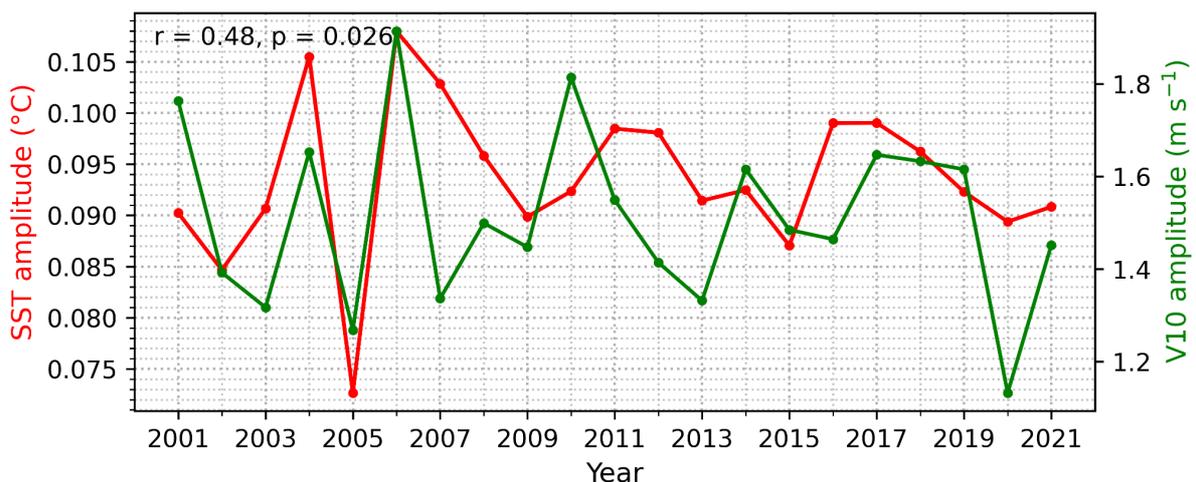

Figure R2 shows the interannual evolution of the synoptic amplitude of SST and V at 10 m in JAS 2001-2021, averaged over a representative AEW-active region of the tropical North Atlantic (9–18°N, 26–20°W), centered near the PIRATA mooring at 12°N–23°W.

<u>Minor points:</u>

- The region in the tropical Atlantic north of the equator has been named tropical North Atlantic (TNA) in most publications. I think this should be changed in the manuscript which often uses "North Tropical Atlantic". In fact, even the authors sometimes refer to this region as tropical North Atlantic (e.g., line 125). It should be uniform throughout the manuscript.

Response: We agree with the reviewer comment and we have now corrected all instances of 'North Tropical Atlantic' to 'Tropical North Atlantic (TNA)'.

- Lines 17-19: In which region are AEWs close to the inertial periods? This could be shown. See major concern 2.

Response: As indicated in our response to major comment 2, AEW time scales are closest to the local inertial period mainly between approximately 5°N and 20°N in the TNA. In this region, the inertial period is of the same order of magnitude as the synoptic variability associated with AEWs, thus creating favorable conditions for interactions between AEW-related wind forcing and quasi-inertial motions.

- Lines 23-26: I don't think key points are required for Ocean Science? Ignore this comment if this has changed.

Response: We agree that the key points are not required for Ocean Science, therefore they have been removed in the revised version.

- Lines 111-113: I wonder how useful it is to validate the model output with a reanalysis product (ERA5) that is used to initialize the atmospheric model? Wouldn't a comparison with independent data be more useful?

Response: We agree that the use of independent observations is essential for an objective model evaluation. ERA5 is primarily used here as a reference framework to assess large-scale atmospheric structure and variability, rather than as a fully independent validation dataset, since it provides the initial and lateral boundary conditions for the atmospheric model. Nevertheless, we consider it informative to show how ERA5 behaves within the dynamical range discussed in this study.

For this reason, the model evaluation is not based solely on ERA5. The simulation is also evaluated against independent observational datasets, including ASCAT surface winds and in situ measurements from the PIRATA network. These comparisons provide an independent assessment of the model's ability to reproduce both the mean state and the high-frequency variability relevant to AEWs.

- Line 234: Which region is meant here? This should be defined clearly. The way it is written here is too vague.

Response: We thank the reviewer for pointing this out. We have amended the text to explicitly refer to the "tropical North Atlantic".

- Figure 5e: How to distinguish between near-surface wind variability in the 3–5-day band from AEWs and the African westerly jet? Or is there interaction between these?

Response: This is an important question. Previous studies have shown that 3- to 5-day AEWs have a detectable signature near the surface and in the lower troposphere, characterized by coherent meridional wind anomalies extending from the surface to the East African jet level (e.g., Diedhiou et al., 1998; Kiladis et al., 2006; Wu et al., 2013).

"The 3–5-day filtering (Figure 5e) eliminates the low-frequency background circulation associated with the WAWJ" (West African Westerly Jet), known to be the driver of moisture This lower tropospheric dynamic is known to be the driver of moisture supply (coming from the ocean) to the West African rain system (Grist and Nicholson, 2001; Lamb, 1983; Liu et al., 2020). At these latitudes, the variability highlighted therefore "mainly reflects transient synoptic disturbances consistent with the documented signature of AEWs. However, interactions between AEWs and the WAWJ cannot be ruled out. AEW-related anomalies can locally and temporarily modulate low-level westerly winds, leading to apparent spatial overlap between the WAWJ region and the AEW signal in surface wind diagnostics. This AEW-WAWJ coupling and its influence on low-level convergence and moisture transport have been addressed in previous studies (e.g., Hsieh and Cook, 2007; Leroux and Hall, 2009)". A quantitative separation of these mechanisms would require specific diagnostics and is beyond the scope of this study. "We therefore interpret Figure 5e primarily as the surface footprint of $AEWs_{3-5day}$", while acknowledging the potential modulating role of the WAWJ reported in the literature.

Specific comments:

Abstract:

- Line 14: Please define PIRATA or keep it more general in the abstract. For instance, by saying "moored surface buoys".

Response: In the abstract, PIRATA has been replaced by a more general description referring to "moored surface buoy air-sea observations."

1 Introduction:

- Lines 42-44: African Easterly Waves propagate from east to west.

Response: We thank the reviewer for pointing this out. The text has been corrected by replacing "eastward-propagating" with "westward-propagating".

- Line 51: I assume the authors mean "zonal wavelengths"?

Response: Yes, this refers to the zonal wavelengths. We have clarified this in the manuscript.

2 Data and methodological approach:

- Line 88: Do the authors mean "air-sea" instead of "air-heat"?

Response: Yes indeed… We have modified this in the manuscript.

- Line 124: Here the surface air temperature at 1m above sea level is meant, correct?

Response: The temperature referred to here corresponds to the ocean temperature measured at 1m depth by the PIRATA mooring. The text has been clarified accordingly to avoid any ambiguity.

- Line 125: Please also mention the more up to date reference for the PIRATA buoy network: Bourlès et al. (2019).

Response: Thank you for this suggestion. The reference has been updated to include the most recent PIRATA description (Bourlès et al., 2019).

3 Evaluation of the coupled model:

- Line 154: Typically, this upwelling region is referred to as "Mauritania upwelling".

Response: We thank the reviewer. The upwelling system between 21°N and 25°N is now referred to as "the Mauritanian upwelling" in the revised version.

- Lines 157-159: It seems that the model also underestimates the magnitude of the Atlantic Cold Tongue. Here, a more quantitative comparison could be useful to validate the model output.

Response: We agree that the model slightly underestimates the amplitude of the Atlantic cold tongue. This warm bias has been documented quantitatively in several studies (Deppenmeier et al., 2020; Shi et al., 2018; Voldoire et al., 2019) and is a well-known feature of many coupled climate models. The magnitude reported in these studies is consistent with the warm bias seen in the mean SST fields of our simulation, as illustrated in Figure 1 and now explicitly mentioned in the revised manuscript.

- Figure 1: Why using the time period 2007-2021 and not the full time series since 2001? I don't see an explanation for using the shorter time period. In Figure 2 the full time series is used.

Response: The period 2007–2021 was selected because several PIRATA moorings exhibit missing SST data during some boreal summers before 2007. Starting the analysis in 2007 allows us to use a temporally consistent dataset and to harmonize the comparison across all PIRATA sites.

- Lines 172-174: But satellite SST data are provided as daily averages (i.e. some of the high-frequency variability is averaged out), whereas ERA5 and PIRATA data are available at higher frequencies (3-hourly and hourly). It would be interesting to look at an exemplary season and compare the time series of ERA5, PIRATA, and model output. Because even the PIRATA buoy

north of the Cape Verde islands which is closest to the high SST STD off Africa shows reduced variability compared to regions outside of the high variability area.

Response: We agree that the different temporal resolutions of sea surface temperature (SST) products (daily for satellite SST, hourly for PIRATA, and sub-daily for ERA5 and the model) may affect the representation of high-frequency variability. Daily averaging of satellite SST can indeed attenuate some of this variability.

In this section, our objective is to consistently compare the spatial configurations and relative amplitude of synoptic SST variability between different datasets, rather than to assess the total variance at sub-daily time scales at specific locations. To this end, all datasets were processed using comparable filtering and temporal (daily) averaging, thus ensuring the internal consistency of the diagnostics presented in Figures 1 and 2.

- Figure 2: It is interesting that ERA5 and OISST produce the same climatology but very different standard deviation (as a function of calendar month). What could be the reason(s) for this? Larger swings in OISST around the same mean values in both products?

Response: We agree, these differences between ERA5 and OISST are interesting. Differences in spatial resolution, interpolation procedures and temporal averaging between the two products may contribute to the differences observed in the standard deviation of SST. In particular, OISST is a daily optimal interpolation product combining heterogeneous satellite and in situ observations, including data potentially contaminated by diurnal variability (e.g. AVHRR), as indicated by Huang et al. (2021). Conversely, ERA5 SST is produced as part of a coupled reanalysis, which leads to a smoother temporal evolution and may explain reduced variability at these time scales.

- Lines 193-194: I don't follow this. Why does higher SST STD in OI-SST imply biases from satellite measurements? It seems that most of the model output validation simply depends on whether the comparisons are really between comparable variables (skin temperature vs. SST)?

Response: We thank the reviewer for this comment and acknowledge that the wording in lines 193–194 could be ambiguous. A higher standard deviation of TSM in OI-SST does not imply bias in satellite measurements. Rather, it reflects differences in the definition of the temperature variable and its temporal representation across datasets. Note that the other reviewer pointed out that OISST is in fact not a measure of skin temperature, but rather of SST. Indeed, satellites do measure skin temperature but were adjusted to be blended with in-situ observations measuring SST at 0.2 m to several meter depths (Huang et al., 2021).

In the revised version, we clarify that the differences in SST variability between OISST, ERA5 and the coupled model mainly reflect differences in analysis methodology, spatial smoothing and effective temporal sampling, rather than measurement biases. OISST represents a composite global SST product, combining satellite and in situ observations, while ERA5 and the coupled model provide SST estimates constrained by the physics of the ocean-atmosphere model, which may induce smoother temporal variability.

To avoid any ambiguity, we also specify that the assessment of SST variability at the synoptic scale in this study is primarily based on PIRATA in situ observations, which are the most physically comparable reference for the model at these time scales. The manuscript has been revised accordingly to ensure consistency in this section (3.1).

- Lines 208-209: The winds north of the equator (10°N-15°N) do not cross the equator. The southeasterly trade winds cross the equator and are deflected to the right north of the equator. Please clarify this sentence.

Response: We agree and we have clarified the sentence to indicate that "South of the equator, the southeasterly trade winds cross the equator and are then deflected to the right by the Coriolis force, giving rise to south-westerly winds north of the equator."

- Figure 4: It should be noted (and discussed why) that the model exhibits the highest deviations from all other products during July to September (Fig. 4a) which is the time of the year when AEWs are investigated.

Response: Indeed, we have observed that the main differences between the coupled model and other products occur between August and September, which coincides with the peak period of AEW activity and a strengthening of monsoon flows.

These differences, although noticeable, remain moderate in magnitude and are not associated with major discrepancies in the spatial structure of the wind field. In particular, the main synoptic variability patterns relevant to AEWs are very well reproduced by the coupled model.

The origin of these summer differences cannot be easily attributed to a single factor. They probably reflect a combination of more intense synoptic atmospheric forcing during this period and differences in model formulation and temporal smoothing between the coupled simulation, reanalysis products, and observational datasets. It is important to note that, despite these seasonal variations, the coupled model reproduces the timing, spatial organization, and dominant modes of wind variability associated with summer waves, which are the main focus of this study.

4 Ocean surface response to AEWs:

- Figure 7b: I believe "2-10jrs" is the French version of "2-10 days". Please replace.

Response: The label "2–10 jrs" has been replaced by "2–10 days" in Figure 7b.

- Lines 289-290: It says 2015 in the text, but 2001 on the x axis in Figure 7. Please clarify which period is shown here.

Response: Figure 7 shows the boreal summer of 2001; the reference to 2015 in the text was a typographical error.

5 The ocean mixed layer heat balance:

- Lines 373-375: Shouldn't a deepening of the mixed layer depth imply warming and not cooling of the mixed layer?

Response: It depends on the cause of the deepening of the mixed layer (ML). When the heat fluxes are strongly positive, the ML can indeed deepen due to an increased heat content (a 'thermal' deepening): the isothermal layer deepens, as does the ML, and this situation is generally associated with a warming.

In this case, however, the ML is deepening due to wind action. This is a 'mechanical' deepening, which causes the entrainment of colder water from under the ML and therefore cools the ML. Moreover, an increase in wind also causes an increase in latent heat loss, which cools the ML even further.

- Lines 386-387: Is this really significant from a statistical point of view? Otherwise, the authors should be careful with using the phrase "significant".

Response: We agree that the term 'significant' can be confusing if interpreted strictly. In this context, it was intended to describe a more intense physical response, not a formally tested statistical significance. To avoid confusion, we have revised the manuscript and replaced 'significant' with 'stronger' (or 'more pronounced') when comparing the SST response associated with the 3-to-5-day and 6-to-9-day AEW bands.

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