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 thank the reviewers for their constructive comments and suggestions, which have significantly improved the quality of the manuscript. Reviewers raised several main concerns. These, along with the minor comments, have been carefully addressed in the revised version. The main points are summarized below and further detailed in the specific responses:

- In response to the comments regarding the index description and the interpretation of the linear regression analysis, we agree that the methodological aspects were insufficiently described in the original version and could have led to misunderstandings or over-interpretation of the results. We have therefore substantially expanded the methodology section and clarified the filtering strategy to improve the robustness and transparency of the results.
- Concerning the independence of the datasets used for model evaluation, we acknowledge that parts of the original text required clarification. While we do not agree with all aspects of the reviewers' interpretation, we have carefully reworded the relevant sections and added significant explanations to better justify our approach and the conclusions drawn from the comparisons.
- We also acknowledge several typographical errors that may have hindered the reviewer's reading process. These have been carefully corrected in the revised manuscript.

Reviewer 1:

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).

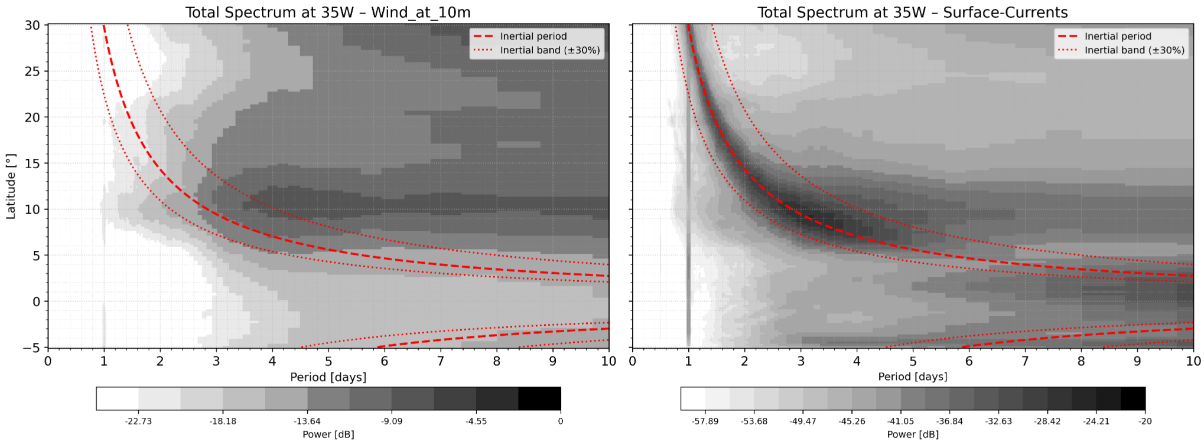

Figure R1: Latitude–period distribution of multi-taper rotational spectra of wind at 10 m (left) and surface currents (right) at 35°W during the dry season (2001–2021). The red dashed curve represents the local inertial frequency, and the red dotted curves delimit the ±30% band around this frequency.

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.

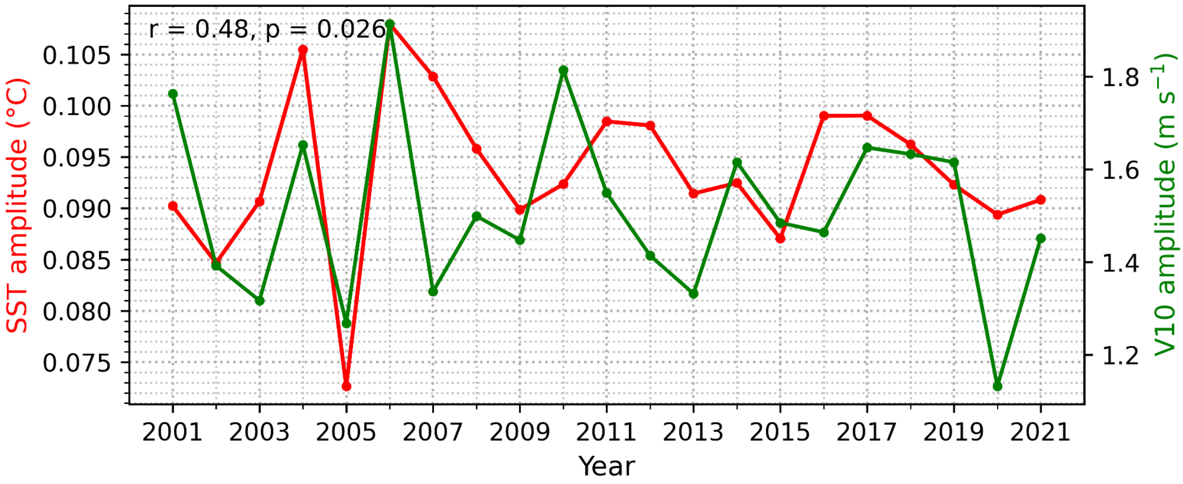

Figure R2 shows the interannual evolution of the synoptic amplitude of SST and V at 10 m in JAS 2001-2021, averaged over a representative AEW-active region of the tropical North Atlantic (9–18°N, 26–20°W), centered near the PIRATA mooring at 12°N–23°W.

Minor points:

- 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 $\text{AEWs}_{3-5\text{day}}$", 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.

Reviewer 2:

The paper addresses relevant scientific questions that fall within the scope of Ocean Sciences. Foltz et al (2025) recently highlighted the needs for improved understanding of upper ocean physical processes and their forcing in the tropical oceans. This study could potentially contribute to a better understanding of the role of high-frequency atmospheric variability, such as African Easterly Waves, in setting sea surface temperature in the tropical North Atlantic. However, the current version of the manuscript contains a large number of errors that make the reported results untrustworthy. E.g., all-time series used in the study are band pass filtered for the 1/2-1/10 day^{-1} although daily time series are used that only resolve periods of 2 days. This filtering reduces variability in the frequency range from 1/4 to 1/2 day^{-1}, even though this variability is considered to be the factor that most strongly impacts sea surface temperature. In the model evaluation section, observations are compared to model output that are falsely interpreted and conclusions drawn from the comparison are invalid. Similarly, the limited explanation of the methods used suggests that the linear regression analysis has been misinterpreted and that the figures presented are unreliable, while a statistical uncertainty analysis is completely lacking.

Below, I am detailing my remarks with reference to the individual line numbers of the manuscript and provide some suggestions for revision.

Detailed remarks:

1- Line 70, quasi-inertial waves: What are these? Please explain this term.

Response: We have clarified this terminology. We now consistently use the term “near-inertial waves” throughout the manuscript, referring to oceanic internal waves with frequencies close to the local Coriolis (inertial) frequency, typically generated by high-frequency atmospheric wind fluctuations.

2- Line 88, air-heat fluxes: Please explain this term.

Response: Thanks. The term “air-heat” was a typographical error and has been corrected to “air-sea” in the revised manuscript.

3- Line 128, use of log wind profile: The coefficients used for the log wind profile and the assumed height of the anemometers of the PIRATA buoys are critical to the model evaluation section. How do you justify the values used and what may be their uncertainty?

Response: We thank the reviewer for raising this important point and have added further clarification on the use of the logarithmic wind profile and the associated uncertainties in Section 2.2, as detailed below.

“Wind measurements from PIRATA buoy are reported at an anemometer height of 4 m ($Wind_{4m}$) and are scaled to 10-m wind speed ($Wind_{10m}$) to ensure consistency with ERA5, ASCAT, and the model diagnostics. The conversion is performed using a neutral logarithmic wind profile ($Wind_z = \frac{u_*}{\kappa} ln\left(\frac{z}{z_0}\right)$), assuming a representative open-ocean roughness length of $z_0=2.10^{-4}m$. This value of surface roughness length (z_0) is consistent with commonly reported values under moderate wind conditions (Charnock, 1955; Dutton, 1995; Fairall et al., 2003; Large and Pond, 1981)."

This gives us the conversion factor: $\frac{U_{10m}}{U_{4m}} = \frac{ln\left(\frac{10}{z_0}\right)}{ln\left(\frac{4}{z_0}\right)} \approx 1.093$

The uncertainty primarily arises from the variability in effective roughness length and atmospheric stability (neutral assumption). "Sensitivity tests show that using plausible open-ocean values of z_0 (order 10^{-4} - 10^{-3}) changes the 4-10 m wind conversion factor by only ~1-2% relative to our reference value, corresponding typically ~0.05-0.15 $m.s^{-1}$ for wind speeds of 5-10 $m.s^{-1}$. In addition, a small uncertainty in the reported anemometer height would produce a similarly modest effect on the conversion factor. These uncertainties remain small compared to the synoptic wind variability considered in this study."

4- Lines 85-109, description of the regional coupled model: I would suggest the authors provide more information on the ocean model used. E.g., which vertical mixing scheme is employed in the ocean model? This is relevant because vertical ocean processes do seem to be important.

Response: Additional information on the ocean model has been added in the revised manuscript. In Section 2.1, we now specify that "vertical mixing in the ocean model is parameterized using the Generic Length scale (GLS) turbulence closure in a k-ε configuration".

This scheme explicitly represents shear and stratification driven turbulence through prognostic equations for turbulent kinetic energy and its dissipation. This choice is appropriate for the present study, as vertical mixing diagnosed primarily through vertical diffusion plays an important role in the SST response to AEWs.

5- Lines 131-132, Butterworth band-pass filter: The authors use daily time series of winds, SST and model output (mixed layer heat budget terms) in this study. This implies that the lowest period that is resolved by the data is 2 days (Nyquist frequency is 1/2 day^{-1}). It makes no sense to band-pass filter this data to "retain variability in the 2-10-day period (line 132)". When doing so, variability in the 2-4-day period range will be damped. Instead, the data simply need to be high-pass filtered with a cut-off period of 10 days to retain high-frequency variability. The use of a band-pass filtered data for the analysis in section 3 to 5 and the model evaluation makes the results questionable, because variance in the 2-4-day period band is lost in all-time series. Apart from this, additional information about the filtering methodology such as one-sided (does not preserve phase) or two-sided (preserves phase) filtering and filter order needs to be added to the section to ensure reproducibility and interpretability.

Response: We agree that, for daily sampled data, the Nyquist frequency is 0.5 day^{-1}, implying that variability exactly at the 2-day period cannot be resolved. In practice, digital filtering requires the cutoff frequency to remain strictly below the Nyquist frequency. We acknowledge that this was not explicitly stated in the original manuscript and have clarified this point in the revised version as follows. "Although we use the "2–10-day" denomination, the upper cutoff frequency of the filter is set slightly below the Nyquist frequency (0.99 × Nyquist) to account for the daily sampling.”

It is important to note that this does not imply a loss of variance across the entire 2–4-day band. The Butterworth filter used here has a continuous frequency response; attenuation is therefore confined to a very narrow frequency band immediately adjacent to the Nyquist frequency. Variability at periods longer than approximately 2.1 days, including the 3–5-day band, is consequently well preserved.

This is visible in Figure R3, which compares the power spectra of the raw data, the band-pass filtered data (2–10 days), and a dataset filtered using a high-pass filter (< 10 days). The spectra are estimated using an adaptive multitaper method, and the shaded envelopes indicate 95% confidence intervals based on the effective degrees of freedom. The band-pass and high-pass spectra closely overlap over the synoptic range, indicating that variance in the ~2-to-4-day band is largely preserved.

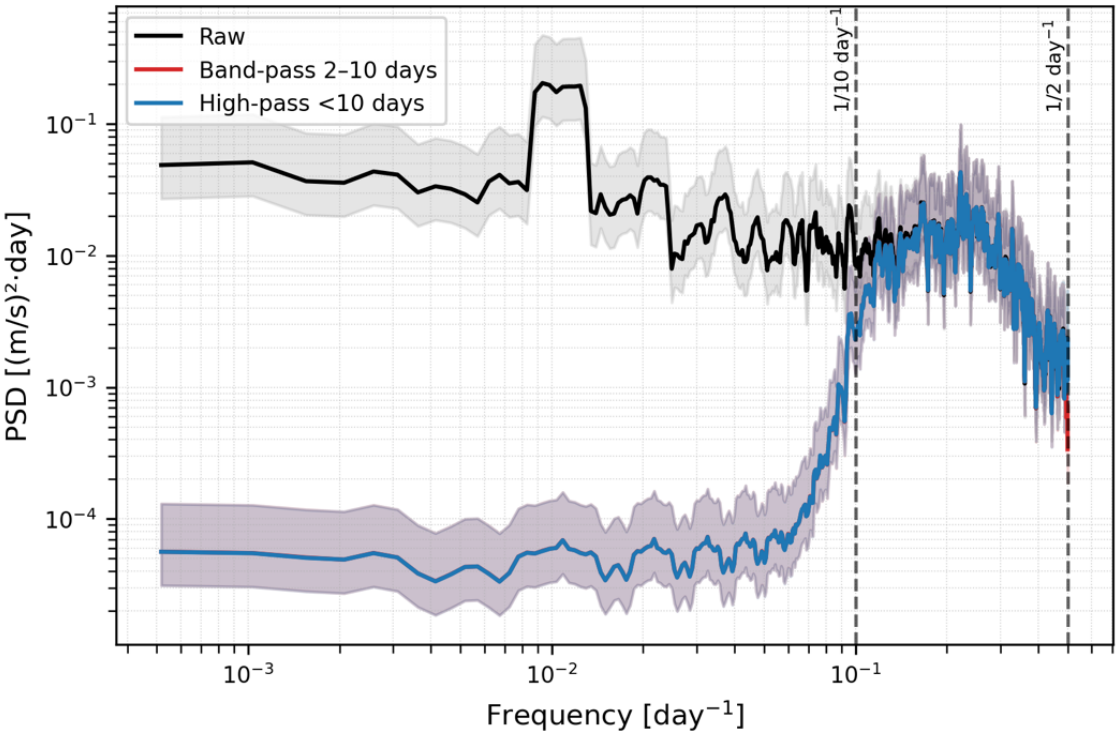

Figure R3: Power spectral density of the meridional wind (V10) at a single basin point (pt2312), JAS 2001–2021: raw signal (black), 2–10-day band-pass filtered signal (red), and <10-day high-pass filtered signal (blue). Shaded areas indicate the 95% confidence intervals.

We acknowledge that the wording “2 to 10 days,” without further methodological details, could be confusing. The text has therefore been revised to clarify that the filter targets synoptic variability at periods shorter than 10 days, with the effective lower bound being slightly above 2 days due to daily sampling.

Additional details on the filtering methodology have also been included (Section 2.3) to ensure reproducibility and interpretability. Specifically, we now state that “we apply a 4th-order zero-phase Butterworth band-pass filter to the time series, retaining variability in the 2–10-day period. The filter is applied using a forward–backward procedure, which removes phase distortion while preserving the amplitude of the signal.”

6- Lines 132-143, identification of AEWs: While I think that this section nicely motivates the use of regression to analyze the effect of EAWs on the oceans’ heat balance, a section should be added that details the use of the regression analysis. Detail should include (1) whether explanatory or predictor or time series were normalized, (2) processing of the time series used as input and (3) a detailed evaluation of statistical significance.

Response: We agree with the reviewer. As suggested, we have expanded the methodological paragraph to include details of the regression analysis used to quantify the oceanic response to AEWs in Section 2.3, as follows: “

To quantify the impact of AEWs on atmospheric and oceanic variables, we employ a linear regression framework to band-pass filtered anomaly time series. An AEW index is defined from the filtered 10m meridional wind over a reference region characterized by strong synoptic variability.

The AEW index is not normalized prior to regression, so that the regression coefficients retain their physical units” (e.g., $\frac{°C.day^{-1}}{(m.s^{-1})}$) “and can be directly interpreted as the response per m/s of 10-m meridional wind anomaly.”

“To facilitate the interpretation of the regression results, the regression coefficients are evaluated for a representative AEW amplitude derived directly from the AEW index (i.e., meridional wind). The local extremes of the filtered index are identified, and only peaks exceeding one standard deviation in absolute value ($|x| > 1\sigma$) are retained, thus isolating robust and well-developed AEW events while excluding weak fluctuations. The representative AEW amplitude is defined as the average magnitude of these peaks.” Lagged regressions are performed by shifting the dependent variable in time relative to the AEW index.

“Statistical significance is assessed independently at each point of the grid using the student's t-test associated with the regression slope, with p-values being obtained directly from the regression analysis. Only regression coefficients that are significant at the 95% confidence level ($p < 0.05$) are retained and displayed in the figures. Given that temporal filtering introduces autocorrelation and reduces the effective number of degrees of freedom, this significance threshold should be considered conservative. The robustness of the results is therefore mainly assessed through the spatial consistency of the regression models and their consistency between variables and time lags.”

7- Lines 154-155, Southeast of the Equator, the Atlantic cold tongue …: Why is the cold tongue located southeast of the Equator ?

Response: The Atlantic cold tongue is preferentially located southeast of the equator because of the asymmetric wind forcing and ocean circulation in the tropical Atlantic. During the boreal summer, southeasterly trade winds cross the equator and induce Ekman divergence, causing greater upwelling south of the equator, particularly in the eastern basin (Cromwell, 1953; Stommel, 1959). This cooling is accentuated by turbulent vertical mixing and upwelling from the thermocline in the southeastern tropical Atlantic (Jouanno et al., 2011; Wade et al., 2011).

8- Line 166-168, comparison of SST variability: Here, you are comparing daily averages of model and ERA5 SST with a satellite SST product (OISST) that is not a daily average, but measures SST whenever there is a satellite over that specific region. OISST thus retains diurnal variability in their data set. To me, the comparison of SST variability presented here does not make sense. You are comparing daily averaged with SST taken at specific times during a day. To compare model output to OISST, the SST from the time of day of satellite overpasses (probably less than a minute) need to extracted from the model and compared.

Response: To avoid any misinterpretation, we have revised the manuscript to clarify the nature of OISST and the scope of the comparison. In this revised version, we specify that OISST is a daily gridded optimal interpolation product that blends satellite retrievals with in situ observations from ships and buoys, including PIRATA, and therefore "represents a blended bulk SST product" rather than a direct skin SST measurement. Differences in synoptic SST variability between datasets are the result of methodological characteristics.

More specifically, "differences in synoptic SST variability among OISST, ERA5, and the coupled model likely arise from a combination of factors, including analysis methodology, spatial smoothing, and effective temporal sampling, as documented in previous intercomparisons of SST products (e.g., Huang et al., 2021; Reynolds et al., 2007)."

It should be noted that some satellite data used in OISST (e.g., AVHRR) may retain a contribution from diurnal variability (Huang et al., 2021), despite the implementation of dedicated procedures in the OISST analysis system to limit this effect. However, this characteristic does not affect the scope of our analysis, which focuses on synoptic variability at daily resolution.

Finally, to assess the realism of synoptic SST variability, "we place particular emphasis on comparisons with in situ observations from PIRATA moorings, which provide SST measurements at a depth of approximately 1 m and are consistent with the vertical resolution and daily averaging of the coupled model."

9- Line 168-174, discussion about skin temperature measured by satellites: I find this section highly speculative. The OISST product, as you state in the lines 120-122, is a product from satellite data, ship and buoy data (Huang et al., 2021). It uses satellite skin measurements but adjusts and blends them with in situ data, including PIRATA temperature measurements, so that the final product represents a bulk SST field rather than true skin temperature. This should be clearly stated here. There have been numerous studies comparing OISST with independent data sets. Can any of these previous studies support this discussion, here? As noted above, OISSTs are not daily averages.

Response: We agree with the reviewer on this point. OISST is indeed a composite global SST product that combines satellite data with measurements from ships and buoys, including PIRATA data. Therefore, it represents a blended bulk SST product rather than a direct skin SST measurement.

We acknowledge that the previous wording of this section (3.1) could be misleading. We have therefore removed the speculative discussion attributing the increased variability of OISST to surface temperature effects and revised the text to indicate the global and composite nature of OISST clearly. The revised discussion (section 3.1) now emphasizes that “differences in synoptic SST variability among OISST, ERA5, and the coupled model likely arise from a combination of factors, including analysis methodology, spatial smoothing, and effective temporal sampling, as documented in previous intercomparisons of SST products (e.g., Huang et al., 2021; Reynolds et al., 2007)”, rather than from skin-temperature effects.

10- Lines 193-196, biases in satellite measurements: I find this hard to follow. Before, in line 168-174, it is argued that skin temperature is causing elevated variability. Here, it is argued that biases in the interpolated data are causing the elevated variability. How is any of this justified? OISST uses temperature measurements from PIRATA buoy data in their data sets. Why is OISST so much different from the data it uses? Again, PIRATA temperatures from the time of day of satellite overpasses need to extracted and compared to OISST data.

Response: We agree with the reviewer that the original wording could be perceived as inconsistent. In the revised version of the manuscript, we no longer attribute the increased variability in OISST data to surface temperature effects or satellite measurement biases. Instead, we interpret the discrepancies between OISST, ERA5, and the coupled model data at synoptic time scales as reflecting differences in analysis methodology, spatial smoothing, and effective temporal sampling, consistent with previous assessments of SST products (e.g., Huang et al., 2021; Reynolds et al., 2007).

Although OISST incorporates PIRATA observations, it is not expected to correspond exactly to PIRATA's short-term point measurements. For this reason, and to avoid any ambiguity, the assessment of synoptic variability in SST in this study is based primarily on a direct comparison between the coupled model and global PIRATA SST, which is the most relevant reference for the model at these time scales, rather than on OISST data.

11- Lines 199-232 and Figures 3 and 4, model evaluation of 10-meter wind: Again, I have difficulties in believing any of the analysis presented here. In Figure 3 and 4, it is shown that PIRATA winds are very different from ERA5 winds, on average and in the magnitude of 2-20-day variability. However, ERA5 uses the data assimilated and processed in the ECMWF's Integrated Forecast System (IFS) (Hersbach et al., 2020). The IFS in turn draws data from the GTS, including 6 hourly PIRATA buoy wind data (e.g. see ECMWF global data monitoring reports). As stated in Johns et al. (2021), PIRATA wind have much larger weight for the ECMWF forecast compared to other data such as satellite retrievals. So, I do not understand why PIRATA wind data should be different from ERA5 wind data at the mooring position as suggest in the analysis presented in this section. Could the differences shown here arise from the log wind profile scaling used in this study for the PIRATA wind data that is different from what ECMWF uses? To me, there should be no difference between ERA5 winds and PIRATA winds as they are fully assimilated in the reanalysis and heavily weighted. I would even go as far as saying that the comparison here is unnecessary, as you are comparing the same data. However, why should there be differences in the order of 1 m/s (which is in the order of 15-20% of the wind magnitude) in average winds during July through September between the two data sets as shown in Figure 3? Were the same time intervals used for ERA5 and PIRATA wind averages, i.e. were the periods when no data was available from PIRATA excluded from ERA5 data averaging? I would think that this is most likely the cause the apparent differences in the two data sets. In general, the disagreement between ERA5 winds and PIRATA winds reported in this manuscript challenges the validity of the methods used in the model evaluation. Details of the data treatment for the comparison should be presented here, along with a detailed explanation of why the two data sets should be different. Last not least, ASCAT winds are again satellite measurements done at a certain time during the day. Short-term (e.g. diurnal) variability of these measurements should be much larger than daily averages from ERA5 and the model. I wonder, why ASCAT winds compare relatively well with daily model averages?

Response: We thank the reviewer for this comment but we would like to explain why we believe that our analysis is still relevant although PIRATA wind observations are assimilated into ERA5 through the ECMWF Integrated Forecast System (Hersbach et al., 2020). The differences cannot be attributed to the logarithmic wind profile scaling (see previous response), nor to data gaps in the PIRATA observations, since ERA5 and model winds are sampled at the same times as the PIRATA measurements to avoid sampling biases. However, other intrinsic factors related to data assimilation can explain these differences. In general, comparisons are not independent, as data assimilation systems necessarily rely on the observations they ingest and therefore cannot be routinely assessed using independent observations. We added the following paragraph.

It should be noted that data assimilation does not imply that the reanalysis reproduces buoy observations exactly. ERA5 represents a dynamically balanced atmospheric analysis that optimally combines the model background with all available observations, rather than a restitution of individual measurements (Kalnay, 2003; Lorenc, 1986). Even at buoy locations, differences between PIRATA and ERA5 winds are to be expected since some processes sampled by the observations are not simulated in the model and cannot be constrained by data assimilation, in particular due to horizontal resolution. Indeed, PIRATA provides point measurements, while ERA5 represents the mean winds of grid cells at a horizontal resolution of approximately 0.25°, resulting in representativeness errors inherent to data assimilation systems (Janjić et al., 2018). Finally, ERA5 assimilates PIRATA winds as well as a wide range of other in situ and satellite observations, including winds measured by scatterometers. The resulting analysis reflects an optimal compromise between all data sources and model dynamics, rather than an exact match with individual observations (Ingleby and Lorenc, 1993; Lorenc, 1986).

Previous assessments have also highlighted residual differences between ERA5 surface winds and in situ observations, despite data assimilation (e.g., Bentamy and Fillon, 2012; Ramon et al., 2019).

Finally, although ASCAT winds are derived from satellite passes at specific times, they are spatially averaged over areas spanning several tens of kilometers and exhibit relatively low diurnal variability. Therefore, ASCAT winds are reasonably comparable to the daily mean winds of ERA5 and the coupled model, as ASCAT has been extensively validated against observation (Bentamy et al., 2008). This point has been clarified in the revised version of the manuscript.

12- Line 251, observations: Which observations are you referring to?

Response: Here, the term "observations" refers to the features identified in the figures (i.e., the spatial patterns seen in the analyzed fields), and not to the observational datasets. We have modified the text (section 3.3) to clarify this point.

13- Line 260, Figu 5: The figure contains non-english text which should be removed. Furthermore, I can not make out mean zonal winds in the figures. I would suggest to add a separate subplot showing average winds. What does Uz and Vz stand for? Is vertical shear of horizontal velocity shown here? In the caption it says that velocity is shown.

Response: All non-English text has been removed from Figure 5 in the revised version. The mean zonal winds are represented by contour lines in Figure 5, as indicated in the legend, to highlight the zonal jet streams associated with the AEWs. We acknowledge that this was not sufficiently clear and have revised the caption to make this explicit. In Figure 5, U_z and V_z(now, U and V) denote the zonal and meridional components of the wind, respectively, and do not represent vertical wind shear. The shaded areas correspond to the standard deviation of the band-pass filtered meridional wind anomalies, while the contour lines represent the mean zonal wind. The legend and color scale labels have been modified accordingly to remove any ambiguity.

14- Line 274, North tropical Atlantic: It is either tropical North Atlantic or northern tropical Atlantic.

Response: The correct term is ‘tropical North Atlantic’, which is now used consistently throughout the manuscript.

15- Line 281, mean meridional wind: What is meant by mean? What is averaged?

Response: It was indeed a wording error. The analysis does not focus on the mean meridional wind, but rather on the standard deviation of band-pass filtered meridional wind anomalies. We have corrected the text accordingly to avoid any confusion.

16- Lines 283-285, location of index: While I agree that location of the reference index is somewhat arbitrary in the sense that the pattern will look similar, I would expect statistically significant regression patterns to appear at different locations in Figure 8, 9 and 10 if the location of the index was altered, e.g. placed further offshore. If this is the case, the sentence written here is misleading and should be altered to facilitate understanding of the methodology.

Response: We agree that the precise location of the reference index is not strictly arbitrary and that its displacement may affect the local amplitude and statistical significance of the regression model. In Section 4.1, we specify that "this site is therefore representative of the region of high surface wind variability associated with the AEW", without implying spatial invariance of the regression results. "Sensitivity tests carried out with other index sites", notably that 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". The manuscript has been revised to facilitate understanding of this section.

17- Line 293-295, … a notable degree of correspondence …: I find this formulation rather unprecise. Are these two timeseries significantly correlated or not? By eye, I would think they are not significantly correlated.

Response: We agree that the previous wording could be interpreted as implying a direct or statistically significant linear correlation. Figure 7 combines the raw and synoptic signals to provide an illustrative point-based view of the covariance between wind and SST. While the synoptic panel highlights the synoptic component, the synoptic-scale SST response is intermittent, lagged, and nonlinear; therefore, a strong, point-based linear correlation is not expected. We have revised the text accordingly to clarify that Figure 7 is intended as an illustrative example rather than a quantitative assessment of synoptic coupling. The latter is addressed using regression analyses on the entire dataset in the following sections.

18- Line 297-299, regression analysis: As stated above, the regression analysis should be detailed in section 2.

Response: We agree that the regression methodology should be described in full. As described in question 6, the regression analysis performed here is detailed in the revised manuscript (section 2.3, lines 185–233).

19- Line 307, Figure 8: It is unclear to me how to quantitatively interpret the results of the linear regression analysis presented in the figure. This is due to the fact that details of the calculation are lacking. E.g. where the explanatory time series normalized? How was that done? What is actually shown in the plots, the linear regression slopes or the full linear regression of the filtered time series? How are 95% significant regressions indicated? Why are there units mentioned in the top panels. The units of a regression analysis should be different from those mentioned in the upper panels.

Response: We thank the reviewer for this comment. The detailed description of the regression methodology (choice of normalization, temporal filtering, definition of the AEW index, scaling of regression coefficients, and statistical significance tests) has been fully taken into account following comment 6 and now appears in section 2.3 of the revised manuscript. We would like to clarify some points regarding Figure 8 that were not explicitly addressed previously. The fields shown in Figure 8 (as well as Figures 9 and 10) do not represent the raw regression slopes, but rather the regression response scaled by the AEW, evaluated for a representative

AEW amplitude. Therefore, the fields displayed retain the physical units of the regression variables (e.g., °C SST, m s^{-1} for winds), which explains the units shown in the upper panels.

20- Line 319, +/- 0.5°C: How was this number determined?

Response: The threshold of ±0.3 °C (±0.5 °C) was determined based on the amplitude of SST anomalies regressed on the AEW index associated with average events (strongest events). It corresponds to the strongest SST signals associated with AEWs, represented by the darkest area in the regressed SST diagnostics (Fig. 8b). This threshold is therefore not arbitrary; it represents the most significant average SST anomalies linked to AEW activity.

21- Line 328, "… where evaluated in the model": What does that mean?

Response: With this wording, we wanted to clarify that all terms of the heat balance of the mixing layer are calculated "on-line" in the coupled model. The text has been modified accordingly to remove any ambiguity.

22- Line 330: All variables used in the equations must be introduced in the text.

Response: We have revised the text to clearly define each variable used in the surface layer heat balance equation, including physical fields, parameters, and constants.

23- Line 344, OLR: Why is outgoing longwave radiation (OLR) used? For the heat budget, net longwave radiation, i.e. the difference between outgoing and incoming longwave radiation would be much more meaningful.

Response: Indeed, OLR is not used directly in the heat balance of the mixing layer. This balance is explicitly based on net infrared radiation, i.e. the difference between downward and upward infrared radiation at the surface, which is included in the non-solar surface heat flux term.

OLR is used here solely as an indicator of deep convection and cloudiness, two key atmospheric features associated with AEWs. Variations in OLR provide information on convective activity, which is dynamically linked to AEWs, but they do not directly enter into the ocean heat balance. This distinction has been clarified in the revised version of the manuscript.

24- Lines 356-357, cooling rate and OLR regression slopes: How do you interpret the numbers you have exemplarily selected here? When the mixed layer cools, there is less heat loss due to outgoing longwave radiation? What does this tell us? Furthermore, the units of the regression slopes should be different. E.g. cooling rate should be -0.2°C/day/(m/s). However, these numbers are extremely high and would require very large fluxes to sustain (~-250 (W/m^2) per 1m/s of wind change). So, I again wonder how the EAW index was treated.

Response: We agree that the interpretation of the values quoted in these lines required clarification. The values cited are intended to illustrate the order of magnitude of the SST trend associated with a typical AEW event, not a trend per unit of wind variation. Therefore, although the regression slope is expressed in °C/day/(m/s), the values presented in the text correspond to the AEW-related response evaluated for a representative AEW amplitude, and not to the raw regression slope itself.

As specified in the revised version of the manuscript (section 2.3), "The AEW index is not normalized prior to regression" and "the regression coefficients are evaluated for a representative AEW amplitude derived directly from the AEW index (i.e., meridional wind). The local extremes of the filtered index are identified, and only peaks exceeding one standard deviation in absolute value ($|x| > 1\sigma$) are retained, thus isolating robust and well-developed AEW events while excluding weak fluctuations. The representative AEW amplitude is defined as the average magnitude of these peaks.

This threshold corresponds to synoptic-scale meridional wind anomalies with typical amplitudes of 4.45 m/s."

Thus, the reported cooling rates (e.g., ~−0.2 $°C.day^{-1}$) represent the temperature trend associated with AEWs and do not imply excessively high heat fluxes per unit wind speed.

As for OLR, we specify that this does not refer to the long-wave heat flux emitted by the ocean surface. It is presented here as an indicator of convective activity and cloudiness associated with AEWs. Its regression highlights the consistent adjustment of convection and cloudiness during the passage of AEWs and their link with short-wave radiation variability, rather than direct radiative forcing of the heat balance of the mixing layer.

25- Line 360, Figure 9: Again, I do not agree with the units shown for the regression or the regression slope (whatever is shown here). It should be (°C/day)/(m/s) unless the AEW index was normalized somehow. Using the correct units would also make it easier to interpret the results of the regression analysis. How are statistically significant correlation slopes indicated? Also, I find it hard to identify clear patterns in the plot because they are so busy and small. The figure should be revised to fix this.

Response: The issue of regression units and the treatment of the AEW index is addressed in detail in our response to the previous comment (24- lines 356-357...). As previously stated, the regression slope is expressed in $(°C.day^{-1})/(m.s^{-1})$, but the values presented and analyzed correspond to the AEW-normalized response, evaluated for a representative AEW amplitude, and not to the raw slope per unit of wind speed. With regard to statistical significance, we specified in section 2.3 that it "is assessed independently at each grid point using the student's t-test for the regression slope, with p-values obtained directly from the regression analysis. Only regression coefficients that are significant at the 95% confidence level ($p < 0.05$) are retained and displayed in the figures." Finally, we recognize that Figure 9 was visually dense. It has been revised to improve its readability.

26- Lines 367-380: Again, the units of the parameters (lines 375-376) discussed in this section are wrong and the magnitude of the numbers presented seem to be too high. Results of the regression analysis should also be quantitatively compared to observations such as in Hummels et al. (2020) and Foltz et al. (2020).

Response: We thank the reviewer for this comment. Questions relating to the regression units and the order of magnitude of the reported values are addressed in detail in our responses to previous comments (24-lines 356-357. and 25- Line 360.). Indeed, we did not sufficiently explain the regression unit, but the reported values correspond to the responses evaluated for a representative AEW amplitude (meridional wind anomaly of 4.2 m/s) and are not a function of the wind unit.

Regarding the requested comparison, we agree that it is useful to compare the amplitudes obtained in this study to available observational estimates of turbulent cooling in the tropical North Atlantic, such as those reported by Hummels et al. (2020) and Foltz et al. (2020).

But it should be noted that these studies provide observational estimates of turbulent cooling of the mixing layer, focusing primarily on seasonal variability and event-scale analyses rather than synoptic composites based on regression.

For example, (Hummels et al., 2020) report a vertical diffusion heat loss in the mixing layer of approximately 244 W.m^{-2} during a particularly energetic quasi-inertial event near 11.5°N-23°W, corresponding to intense mixing over a continuous observation period (~20 h). This value corresponds to an intense event and therefore exceeds the composite synoptic response derived from the regression analysis associated with typical AEW events (i.e., 24–47 W.m^{-2}, for cooling rates of 0.1–0.2 °C.day^{-1} over the upper 5 m).

This latter value most closely approximates the one-dimensional mixing diagnostics presented in (Foltz et al., 2020) indicate turbulent cooling typically on the order of 30 to 50 W.m^{-2}, with seasonal peaks sometimes reaching approximately 60 W.m^{-2} around 4°N and 15°N.

Thus, the vertical diffusion cooling associated with AEWs identified in this study (generally on the order of 30–50 W.m^{-2}, and occasionally exceeding 70 W.m^{-2} on average for more energetic AEW events) falls within the range of turbulent cooling observed in the tropical North Atlantic.

27- Line 369, Vertical mixing primarily controls …: I would appreciate if this statement would be supported by a thorough discussion and a plot showing that.

Response: We agree with the reviewer's suggestion. To support the claim in line 369, we have produced an additional diagnostic (Figure R4) for the reviewer using the same regression framework as Figure 9. It compares the regressions linked to the AEW of T_OCEAN, the advection term and the diffusion term, particularly vertical diffusion. In the different iterations shown here in the figure, we see that T_OCEAN is almost entirely dominated by vertical diffusion, while advection and horizontal diffusion are negligible in comparison. As this figure is only intended to serve as supporting evidence, we have not included it in the manuscript.

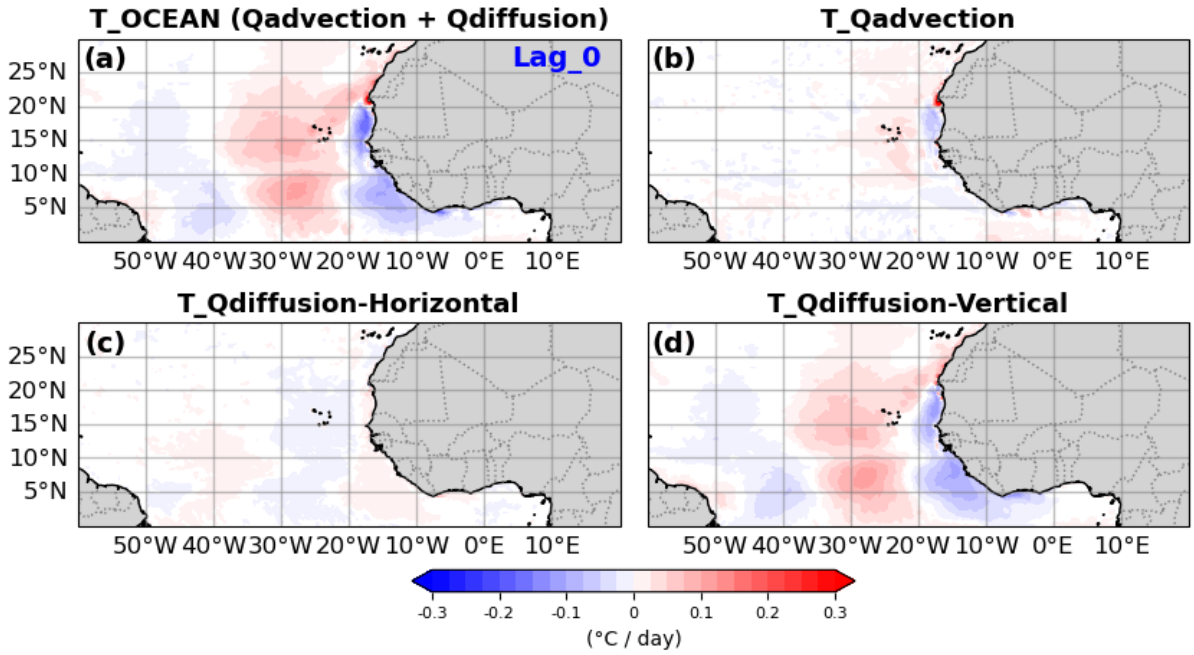

Figure R4: Anomalies (at Lag 0) in the upper-5-m ocean heat budget terms, regressed onto the 2–10-day AEWs index. Panels show: (a) contribution from oceanic processes (T_OCEAN) and its components (b) horizontal and vertical advection (c) horizontal diffusion and (d) vertical diffusion.

References:

Bentamy, A. and Fillon, D. C.: Gridded surface wind fields from Metop/ASCAT measurements, International Journal of Remote Sensing, 33, 1729–1754, https://doi.org/10.1080/01431161.2011.600348, 2012.

Bentamy, A., Croize-Fillon, D., and Perigaud, C.: Characterization of ASCAT measurements based on buoy and QuikSCAT wind vector observations, Ocean Sci., 4, 265–274, https://doi.org/10.5194/os-4-265-2008, 2008.

Bourlès, B., Araujo, M., McPhaden, M. J., Brandt, P., Foltz, G. R., Lumpkin, R., Giordani, H., Hernandez, F., Lefèvre, N., Nobre, P., Campos, E., Saravanan, R., Trotte-Duhà, J., Dengler, M., Hahn, J., Hummels, R., Lübbecke, J. F., Rouault, M., Cotrim, L., Sutton, A., Jochum, M., and Perez, R. C.: PIRATA: A Sustained Observing System for Tropical Atlantic Climate Research and Forecasting, Earth and Space Science, 6, 577–616, https://doi.org/https://doi.org/10.1029/2018EA000428, 2019.

Charnock, H.: Wind stress on a water surface, Quarterly Journal of the Royal Meteorological Society, 81, 639–640, 1955.

Cromwell, T.: Circulation in a meridional plane in the central equatorial Pacific, 1953.

D'Asaro, E. A.: The energy flux from the wind to near-inertial motions in the surface mixed layer, Journal of Physical Oceanography, 15, 1043–1059, 1985.

Deppenmeier, A.-L., Haarsma, R. J., Heerwaarden, C. van, and Hazeleger, W.: The Southeastern Tropical Atlantic SST Bias Investigated with a Coupled Atmosphere–Ocean Single-Column Model at a PIRATA Mooring Site, Journal of Climate, 33, 6255–6271, https://doi.org/10.1175/JCLI-D-19-0608.1, 2020.

Diedhiou, A., Janicot, S., Viltard, A., and de Felice, P.: Evidence of two regimes of easterly waves over West Africa and the tropical Atlantic, Geophysical Research Letters, 25, 2805–2808, 1998.

Diedhiou, A., Janicot, S., Viltard, A., de Felice, P., and Laurent, H.: Easterly wave regimes and associated convection over West Africa and tropical Atlantic: results from the NCEP/NCAR and ECMWF reanalyses, Climate Dynamics, 15, 795–822, https://doi.org/10.1007/s003820050316, 1999.

Dutton, J. A.: Dynamics of Atmospheric Motion, Dover Publications, 1995.

Fairall, C. W., Bradley, E. F., Hare, J. E., Grachev, A. A., and Edson, J. B.: Bulk Parameterization of Air–Sea Fluxes: Updates and Verification for the COARE Algorithm, Journal of Climate, 16, 571–591, https://doi.org/10.1175/1520-0442(2003)016%3C0571:BPOASF%3E2.0.CO;2, 2003.

Foltz, G. R., Grodsky, S. A., Carton, J. A., and McPhaden, M. J.: Seasonal mixed layer heat budget of the tropical Atlantic Ocean, Journal of Geophysical Research: Oceans, 108, https://doi.org/https://doi.org/10.1029/2002JC001584, 2003.

Foltz, G. R., Hummels, R., Dengler, M., Perez, R. C., and Araujo, M.: Vertical Turbulent Cooling of the Mixed Layer in the Atlantic ITCZ and Trade Wind Regions, Journal of Geophysical Research: Oceans, 125, e2019JC015529, https://doi.org/https://doi.org/10.1029/2019JC015529, 2020.

Grist, J. P. and Nicholson, S. E.: A Study of the Dynamic Factors Influencing the Rainfall Variability in the West African Sahel, Journal of Climate, 14, 1337–1359, https://doi.org/10.1175/1520-0442(2001)014%3C1337:ASOTDF%3E2.0.CO;2, 2001.

Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., and Schepers, D.: The ERA5 global reanalysis, Quarterly Journal of the Royal Meteorological Society, 146, 1999–2049, 2020a.

Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., and Schepers, D.: The ERA5 global reanalysis, Quarterly Journal of the Royal Meteorological Society, 146, 1999–2049, 2020b.

Hsieh, J.-S. and Cook, K. H.: A Study of the Energetics of African Easterly Waves Using a Regional Climate Model, Journal of the Atmospheric Sciences, 64, 421–440, https://doi.org/10.1175/JAS3851.1, 2007.

Huang, B., Liu, C., Freeman, E., Graham, G., Smith, T., and Zhang, H.-M.: Assessment and Intercomparison of NOAA Daily Optimum Interpolation Sea Surface Temperature (DOISST) Version 2.1, Journal of Climate, 34, 7421–7441, https://doi.org/10.1175/JCLI-D-21-0001.1, 2021.

Hummels, R., Dengler, M., Brandt, P., and Schlundt, M.: Diapycnal heat flux and mixed layer heat budget within the Atlantic Cold Tongue, Climate Dynamics, 43, 3179–3199, https://doi.org/10.1007/s00382-014-2339-6, 2014.

Hummels, R., Dengler, M., Rath, W., Foltz, G. R., Schütte, F., Fischer, T., and Brandt, P.: Surface cooling caused by rare but intense near-inertial wave induced mixing in the tropical Atlantic, Nature Communications, 11, 3829, https://doi.org/10.1038/s41467-020-17601-x, 2020.

Ingleby, N. B. and Lorenc, A. C.: Bayesian quality control using multivariate normal distributions, Quarterly Journal of the Royal Meteorological Society, 119, 1195–1225, 1993.

Janjić, T., Bormann, N., Bocquet, M., Carton, J. A., Cohn, S. E., Dance, S. L., Losa, S. N., Nichols, N. K., Potthast, R., Waller, J. A., and Weston, P.: On the representation error in data assimilation, Quarterly Journal of the Royal Meteorological Society, 144, 1257–1278, https://doi.org/https://doi.org/10.1002/qj.3130, 2018.

Jouanno, J., Marin, F., Penhoat, Y. du, Molines, J. M., and Sheinbaum, J.: Seasonal Modes of Surface Cooling in the Gulf of Guinea, Journal of Physical Oceanography, 41, 1408–1416, https://doi.org/10.1175/JPO-D-11-031.1, 2011.

Kalnay, E.: Atmospheric modeling, data assimilation and predictability, Cambridge university press, 2003.

Kiladis, G. N., Thorncroft, C. D., and Hall, N. M. J.: Three-Dimensional Structure and Dynamics of African Easterly Waves. Part I: Observations, Journal of the Atmospheric Sciences, 63, 2212–2230, https://doi.org/10.1175/JAS3741.1, 2006.

Lamb, P. J.: Sub-saharan rainfall update for 1982; continued drought, Journal of climatology, 3, 419–422, 1983.

Large, W. G. and Pond, S.: Open Ocean Momentum Flux Measurements in Moderate to Strong Winds, Journal of Physical Oceanography, 11, 324–336, https://doi.org/10.1175/1520-0485(1981)011%3C0324:OOMFMI%3E2.0.CO;2, 1981.

Leroux, S. and Hall, N. M. J.: On the Relationship between African Easterly Waves and the African Easterly Jet, Journal of the Atmospheric Sciences, 66, 2303–2316, https://doi.org/10.1175/2009JAS2988.1, 2009.

Liu, W., Cook, K. H., and Vizy, E. K.: Role of the West African westerly jet in the seasonal and diurnal cycles of precipitation over West Africa, Climate Dynamics, 54, 843–861, https://doi.org/10.1007/s00382-019-05035-1, 2020.

Lorenc, A. C.: Analysis methods for numerical weather prediction, Quarterly Journal of the Royal Meteorological Society, 112, 1177–1194, https://doi.org/https://doi.org/10.1002/qj.49711247414, 1986.

Plueddemann, A. J. and Farrar, J. T.: Observations and models of the energy flux from the wind to mixed-layer inertial currents, Deep Sea Research Part II: Topical Studies in Oceanography, 53, 5–30, https://doi.org/https://doi.org/10.1016/j.dsr2.2005.10.017, 2006.

Ramon, J., Lledó, L., Torralba, V., Soret, A., and Doblas-Reyes, F. J.: What global reanalysis best represents near-surface winds?, Quarterly Journal of the Royal Meteorological Society, 145, 3236–3251, https://doi.org/https://doi.org/10.1002/qj.3616, 2019.

Reynolds, R. W., Smith, T. M., Liu, C., Chelton, D. B., Casey, K. S., and Schlax, M. G.: Daily High-Resolution-Blended Analyses for Sea Surface Temperature, Journal of Climate, 20, 5473–5496, https://doi.org/10.1175/2007JCLI1824.1, 2007.

Shi, Y., Huang, W., Wang, B., Yang, Z., He, X., and Qiu, T.: Origin of Warm SST Bias over the Atlantic Cold Tongue in the Coupled Climate Model FGOALS-g2, Atmosphere, 9, https://doi.org/10.3390/atmos9070275, 2018.

Stommel, H.: Wind-drift near the equator, Deep Sea Research (1953), 6, 298–302, https://doi.org/https://doi.org/10.1016/0146-6313(59)90088-7, 1959.

Voldoire, A., Exarchou, E., Sanchez-Gomez, E., Demissie, T., Deppenmeier, A.-L., Frauen, C., Goubanova, K., Hazeleger, W., Keenlyside, N., and Koseki, S.: Role of wind stress in driving SST biases in the Tropical Atlantic, Climate Dynamics, 53, 3481–3504, 2019.

Wade, M., Caniaux, G., and Du Penhoat, Y.: Variability of the mixed layer heat budget in the eastern equatorial Atlantic during 2005–2007 as inferred using Argo floats, Journal of Geophysical Research: Oceans, 116, 2011.

Wu, M.-L. C., Reale, O., and Schubert, S. D.: A Characterization of African Easterly Waves on 2.5–6-Day and 6–9-Day Time Scales, Journal of Climate, 26, 6750–6774, https://doi.org/10.1175/JCLI-D-12-00336.1, 2013.