

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

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

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We are grateful to Reviewer for their careful reading of the manuscript and for the valuable comments and recommendations. Their feedback has contributed significantly to strengthening the manuscript. Our detailed responses to each comment are presented below.

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 $z_0 = 2 \cdot 10^{-4} m$. 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 $m \cdot s^{-1}$ for wind speeds of 5-10 $m \cdot s^{-1}$. 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 $1/2 \text{ day}^{-1}$). 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 ($0.99 \times \text{Nyquist}$) 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.

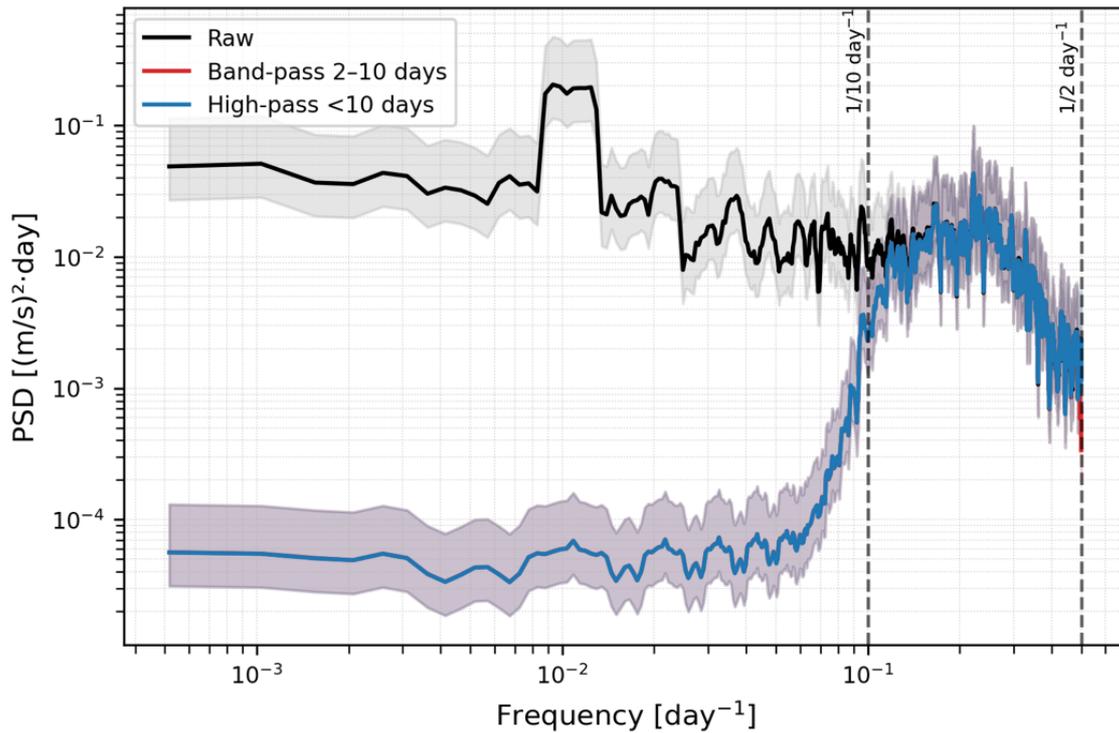


Figure R3: Power spectral density of the meridional wind (V10) at a single basin point (pt2312), JAS 2001–2021: raw signal (black), 2–10-day band-pass filtered signal (red), and <10-day high-pass filtered signal (blue). Shaded areas indicate the 95% confidence intervals.

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., $\frac{^{\circ}\text{C}\cdot\text{day}^{-1}}{(\text{m}\cdot\text{s}^{-1})}$) “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 be 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 be 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 has 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 suggested 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 of 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, Fig 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 U_z and V_z 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 $-0.2^{\circ}\text{C}/\text{day}/(\text{m}/\text{s})$. However, these numbers are extremely high and would require very large fluxes to sustain ($\sim 250 \text{ (W}/\text{m}^2)$ per $1\text{ m}/\text{s}$ 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 $^{\circ}\text{C}/\text{day}/(\text{m}/\text{s})$, 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., $24\text{--}47 \text{ W.m}^{-2}$, for cooling rates of $0.1\text{--}0.2 \text{ }^{\circ}\text{C.day}^{-1}$ 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 $30\text{--}50 \text{ W.m}^{-2}$, 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.

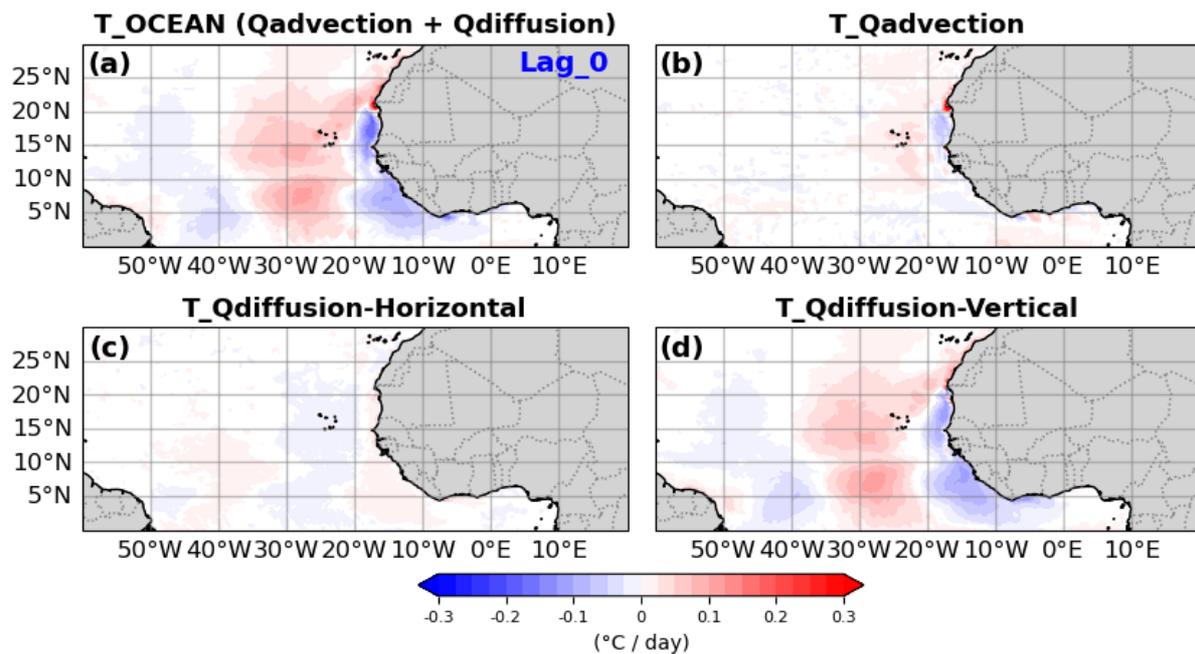


Figure R4: Anomalies (at Lag 0) in the upper-5-m ocean heat budget terms, regressed onto the 2–10-day AEWs index. Panels show: (a) contribution from oceanic processes (T_OCEAN) and its components (b) horizontal and vertical advection (c) horizontal diffusion and (d) vertical diffusion.

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