Dear reviewer,

We are very grateful for the constructive and relevant comments that allowed us improving this work.

Please find below our detailed responses to the comments.

Thank you for this opportunity to review this paper. This study investigates the impact of internal waves on a subsurface chlorophyll structure observed during a 26-day log glider deployment, complemented by satellite data. The manuscript presents a very interesting dataset and a compelling effort to explore the relationship between Chl-a concentrations and internal tides. However, several key elements in the methodology and interpretation of the results required further investigation and clarification. In particular:

1. Definition and identification of ISWs: I believe the introductions need more context and explanation of what internal solitary waves (ISWs) are and how they differ from internal tides. Mostly because ISW is a big part of the results and I believe there is some lack of clarity on how they are identified in the glider data. Do they have a different mixing diffusivity value compared to the tides? How do they relate to the separation of high tide vs Low tide analysis? In the results, the identification of ISWs—particularly in glider and satellite data—is unclear and inconsistent.

Resp.: Following the suggestion, we have added a paragraph in the introduction (L48-60) clarifying the definition of Internal Solitary Waves (ISWs) and their relationship to Internal Tides (ITs). ISWs are nonlinear internal waves, shorter and more stable than ITs, which in our study region form primarily through the disintegration and dispersion of baroclinic internal tides. Unlike ITs, ISWs exhibit clear surface signatures, detectable in satellite imagery (MODIS sunglint or SAR), allowing us to identify their occurrence periods. In our study, ISWs are not directly included in the HT/LT analysis, which is based first on the semi-diurnal modulation of ITs identified from temperature spectra, and second on the classification into spring and neap tides. The ISW observations serve only as additional indicators of the presence and propagation of ITs in the region, as well as of the dominant propagation modes. Regarding their role in vertical mixing, ISWs, owing to their stability and ability to propagate over long distances, are generally less dissipative than ITs near their generation site. They can, however, contribute locally to mixing when they break, but this contribution was not quantified in our study.

2. Assumptions about mixing and chlorophyll: A central conclusion of the paper is that differences in chlorophyll concentrations between high tide and low tide are due to physical mixing, but this assumption is not entirely justified in the methods and excludes potential biological processes within the DMC. I think the paper still has good results, but without turbulence or mixing data, the inferred mechanisms require stronger connection to prior work or clearer acknowledgment of uncertainty.

Resp.: We acknowledge the need to better justify the assumption that differences in chlorophyll between high-tide (HT) and low-tide (LT) conditions are due to physical mixing. The limitations inherent to an observation-based approach compel us to make explicit assumptions in the interpretation of our results. Our reasoning follows the conceptual framework described by Ma et al. (2023) and earlier studies in the South China Sea (B. Chen et al., 2013) and equatorial Pacific (Landry et al., 2011), in which the Deep Chlorophyll Maximum Layer (DCML) represents a transition zone where light and nutrients jointly limit photosynthetic rates, and where phytoplankton growth and loss rates are in dynamic equilibrium. Within such an equilibrium state, the only physical mechanism capable of modifying chlorophyll concentrations within an isopycnal layer is turbulent mixing. While direct turbulence measurements are not available in our dataset, our analysis quantifies the contribution of this mixing to the observed differences in chlorophyll between HT and LT.

3. Glider data processing and resolution: The methods section lacks detail on how glider data were interpolated, gridded, or treated before spectral analysis. Details about dive depth, vertical resolution, and time-series construction are critical to evaluating the strength of the results. This is particularly important for the spectral analysis

Resp.: All scientific and navigation data were linearly interpolated to 1-second intervals to align science variables with the glider's main processor clock. This step introduces minimal additional noise, as the vertical displacement of the glider over one second is typically < 0.2 m. In the vertical, data from each dive profile (yo) were binned and averaged into 1 dbar intervals, and then linearly interpolated to produce uniformly gridded vertical profiles. These gridded profiles were used in all subsequent analyses, including stratification diagnostics and vertical chlorophyll characterization. After the standard GEOMAR Toolbox gridding procedure (1 dbar vertical binning and timestamp alignment), we applied a second linear interpolation in time to project the variables onto a regular temporal grid. This interpolation was performed independently at each depth level using valid (non-NaN) observations, ensuring a complete and consistent depth–time matrix for variability analyses. Importantly, spectral analyses were performed using the original gridded data prior to this second temporal interpolation to avoid any potential alteration of the spectral signal.

4. Justification of assumptions and definitions: Further justification and clarification of how key periods, depths, structures, and thresholds are defined throughout the study is needed to strengthen the interpretation of the results.

Resp.: We thank the reviewer for highlighting the need for clearer justification of the definitions used for periods, depths, structures, and thresholds. We have revised the Methods section to explicitly detail how hydrographic periods were identified (based on consistent T, S, and σ_0 structures and visually discernible transitions in T–S diagrams), how transitional zones were treated (excluded from primary comparisons to ensure water-mass homogeneity), and how high/low tidal forcing phases were defined relative to local spring–neap variability and isopycnal displacement amplitude. We have also clarified the rationale for the selected depth ranges and vertical thresholds (e.g., 0.2 mg m^{-3} criterion for DCM thickness) and indicated where alternative definitions were tested and yielded consistent results (e.g., proportional vs. fixed threshold criteria). These methodological clarifications, combined with the changes described in our line-by-line responses, ensure reproducibility and transparency while maintaining focus on the main scope of the paper

Overall, I think this work has great potential to contribute to the literature of the region, but it needs major revisions to improve its readability and impact of its results. Below I describe in detail major comments and minor comments:

Major Comments:

Lines 47-52: The introduction of the ISW theory might need some work. The acronym is used before explaining what it is, and these sentences appear out of order. SWs are mentioned frequently throughout the paper, so it would be helpful to include more background here—how they are generated and how they differ from internal tide

Resp.: In the revised manuscript (L48 - 60), we have reorganized the introduction to define internal solitary waves (ISWs) before using the acronym and to clearly distinguish them from internal tides (ITs). We now provide additional background on ISW generation mechanisms, including their formation from the nonlinear transformation of ITs as well.

Lines 115-124: Throughout the study, there were different ways of using the glider data (surface comparison with the satellite, spectral analysis etc), which I think is excellent, but it's not clear from the methods how the data were interpolated (if it was) or gridded. Also, what was the maximum dive depth? Later, it's mentioned the glider does 12 profiles per day, with 2 hours per profile (Line 203), suggesting it's not reaching 1000 m. More detail on glider operations would help readers understand the interpretation of the data analysis

Resp.: Done L117-140 however The glider performed dives reaching a maximum depth of ~950 m, completing on average 12 profiles per day (~2 h per profile).

Line 233: The assumption that differences in chlorophyll a between high and low tide are due solely to mixing needs more support. What are the limitations of this assumption? Does this imply ΔSMS_dcm = 0? Since turbulent mixing was not measured, it would strengthen the argument to connect with prior work from the region that documented internal wave-driven mixing or estimated diffusivities consistent with your interpretation. Including possible mechanism (shear-driven turbulence?)

Resp.: cf the comment 2 For the case $\Delta SMS_{dcm} = 0$ (see Comment 2).Although no direct turbulent mixing measurements were collected in our study, our interpretation is consistent with recent observations from the region. Kouogang et al. (2025) documented, in the area corresponding to our Region A, that internal tides (ITs) dominate vertical mixing off the Amazon shelf break, with dissipation rates reaching 10^{-6} W/ kg near IT generation sites and still substantial values (~10^{-8} W/ kg) hundreds of kilometers offshore. Microstructure analyses revealed that IT shear contributed up to 60 % of total shear-driven turbulence, and that elevated dissipation in the far field was often associated with large-amplitude internal solitary waves (ISWs) generated by constructive interference of IT rays. These results support our interpretation that the vertical chlorophyll redistribution we observe in Region A is primarily driven by IT-induced shear-driven turbulence, with ISWs playing a secondary but locally significant role. We have now included a reference to Kouogang et al. (2025) in Lines 270–272 to support our interpretation.

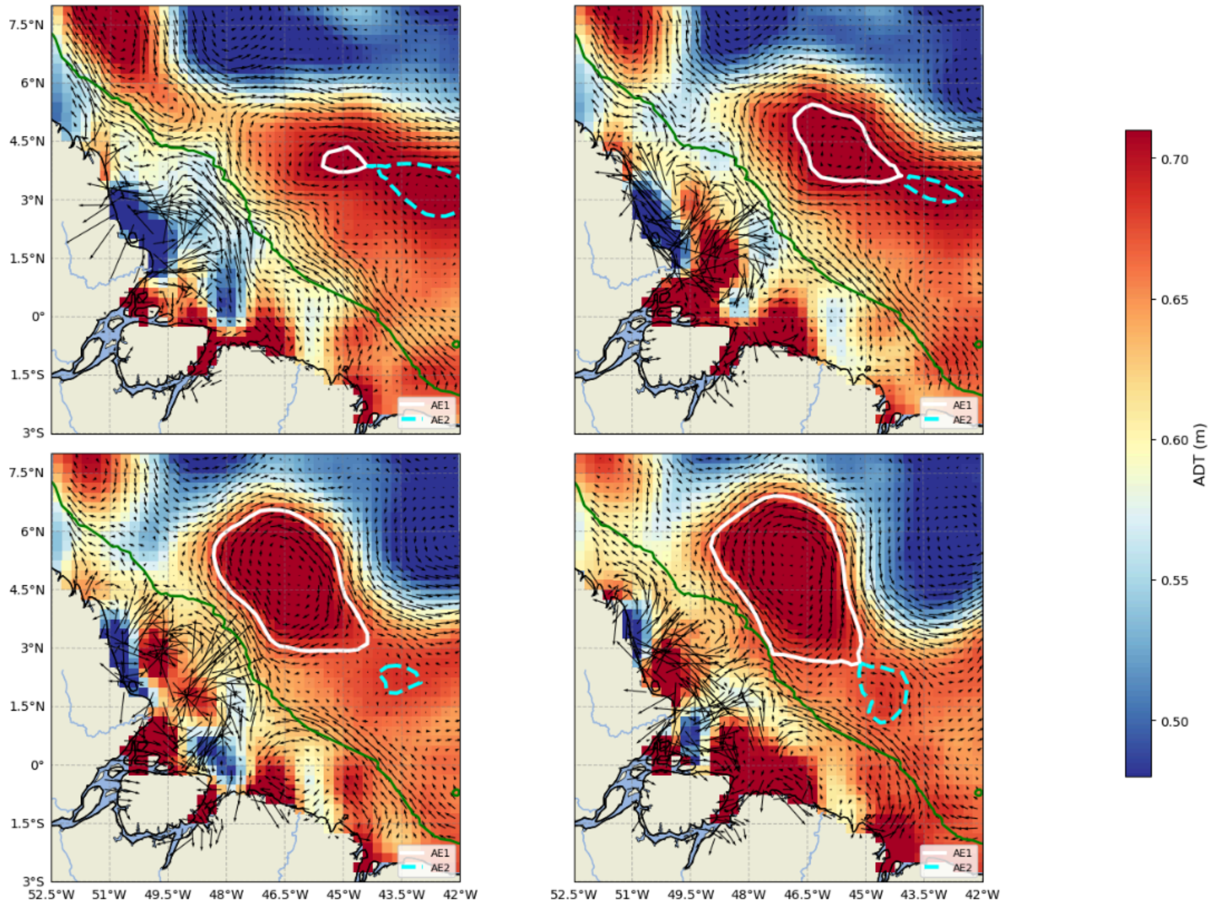
Figure 2: The diagram is hard to interpret. There is no context for why CHL_LT shows a larger peak than CHL_HT. After reading the results, this becomes clearer, but at this point is hard to follow the logic. Why are there two green lines?

Resp.: ***In Fig. 2, the green shading represents the potential impact range of SMS (biological sources and sinks), indicating where SMS could either increase or decrease chlorophyll-a concentrations. It does not represent the sum of chlorophyll-a and SMS, but rather the possible variation in chlorophyll-a attributable to SMS alone.We have updated the legend to clarify this point and facilitate understanding.***

Line 279-280: Is this growth of the eddy observed here typical this region? The speed in which it grows appear fast, but I am not be familiar with eddy activity here.

Resp.: We acknowledge that the apparent "expansion" of AE1 is not solely related to an intrinsic growth of the eddy, but rather to a merging event with a neighbouring anticyclone (AE2) during this period. This type of process has been documented in previous studies (Thesis of Cori Pegliasco, 2017), where the progressive absorption of one eddy by another leads to an increase in the detected radius when using ADT-based contours. Since the dynamics of the merging are outside the scope of the present study, we did not develop this

point in detail in this paper . Here is shown in white, while AE2 (not discussed in the main text) is shown in blue.



Line 315-317: The identification of ISWs in Figure 5d is unclear. Are these timestamps of whent hey are observed in the glider or satellite data? If satellite, how is timing assigned? ? There also seem to be solitons near the spring-neap transition, which complicates the assertion that ISWs align with spring tides. This relationship and its time scale need further clarification—maybe add more context in the introduction.

Resp.: In the revised manuscript, we clarify that the ISW occurrences marked in Figure 5d correspond to the exact timestamps of their detection from satellite imagery (SAR or sunglint MODIS), with acquisition times provided in the satellite data products. We have expanded the explanation of their relationship with the spring–neap cycle: in this region, ISWs are generated primarily by the nonlinear steepening of internal tides forced by barotropic tidal flow, which are typically stronger during spring tides. Nevertheless, ISWs can also occur during neap tides or transitional phases, albeit less frequently, which explains the detections near spring–neap transitions. This additional context has been incorporated into the introduction(cf commentary 1) to better describe the physical link and time scales involved. Furthermore, the term *crests* has been replaced with *wave packet detected* in the table 1 to avoid misunderstanding

Table 1: Figure 5 seems to show two crests on September 9—was a height threshold used to identify crests?

Resp.: No amplitude or height threshold was applied for crest identification. In Table 1, the "crest" column does not refer to individual wave amplitude but if we succeed to identifie internal solitary wave train. For example, on 9 September, the satellite imagery revealed a single ISW packet. We have clarified this wording in the table caption and main text to avoid confusion.

Line 370: The phrase "well-defined T/S stratification" needs clarification. Do you mean stronger or weaker stratification? Is it more linear? Or does it refer to T and S both increasing or decreasing with depth?

Resp.: This sentence has been removed due to its ambiguity. It referred to the presence of a lens-like feature, visible in Figure 6, with homogeneous TTT, SSS, and σ_0. Moreover, the sections *Transect Divided into Four Periods* and *Near-Surface Hydrography* were reorganized at the request of Reviewer 1, and we preferred to avoid redundancy(cf L393).

Line 371-373: I think Period C seems to be fresher than B within the 24 –24.8 mass?

Resp.: We agree with the reviewer's observation. Period C is indeed fresher than Period B within the 24–24.8 σ_0 layer. We have corrected the text accordingly to reflect this difference.

Line 386: There seems to be an assumption that ISWs coincide with tidal peaks—but this is not apparent in Figures 5 or 6. For instance, an ISW is labeled on 13 Sept, but no large oscillation is visible. Also, which peaks are being referenced? (See earlier comment about identifying ISWs.)

Resp.: We agree with the reviewer that not all identified ISWs in Figure 5d coincide with visible large-amplitude isopycnal oscillations in Figures 5 or 6. This is the majority of detections (5 out of 6) occurred during spring tide phase (yellow), which is consistent with the known stronger generation of internal tides during these phases. In this context, "peaks" refers to isopycnal crests associated with internal tide-induced vertical displacements, which in turn can steepen into ISWs.

Line 386–387: The drop in surface temperature during spring tides (sections A and C) could be due to other causes—e.g., position relative to NECC or eddy edges—rather than tides alone. This sentence seems to imply that the tides drive this drop in temperature, but is this through mixing? Or another process?

Resp.: The associated drops in temperature are consistent with previous studies off the Amazon shelf showing cooling above the thermocline and warming below during IT activity (Assene et al., 2024). We have revised the text to clarify these points and avoid overgeneralizing the ISW–spring tide relationship

Line 388: How was the glider data used and prepared to create these FFT? Were they interpolated to a uniform time series?

The sentence "A Fast Fourier Transform (FFT) analysis of isotherms (145–165 m) confirms the semi-diurnal modulation of these oscillations" is unclear in its current form and would benefit from further clarification.From the results, I'm inferring that the FFT is examining the variability of vertical displacement of an isotherm, not the variability of temperature at a fixed depth. Is this correct? Maybe adding units to the spectrum figure will also help clarify this. was some form of averaging or stacking performed across this depth interval that makes the plot so smooth? How was the glider data prepared for the FFT? Was it interpolated to a specific depth? Was it bin averaged? How would this impact your results? A more precise description of the methodology—especially the variable being spectrally analyzed and how it was derived—would greatly improve the reader's ability to interpret the results and evaluate the evidence for semi-diurnal modulation.

Resp.: In the revised manuscript L225 - 238, we have clarified the methodology used to produce the FFT. The analysis was indeed based on temperature variability at a fixed depth range (145–165 m), which was chosen because it corresponds to the layer of largest isopycnal vertical displacement. All measurements within this range were concatenated into a composite 1D time series, assuming coherent variability within the layer. The glider data, initially sampled at irregular intervals due to profiling motion, were resampled to a regular 1 hour grid using averaging followed by linear interpolation. This ensured temporal uniformity for the FFT while preserving sub-tidal variability. The time series was then detrended to remove long-term trends, and the FFT was applied to identify dominant oscillation frequencies. We have also clarified in the text that the FFT examines variability of temperature in this depth range (as a proxy for isopycnal displacement) rather than the vertical displacement of a single isotherm. Units have been added to the spectral density figure to improve interpretability.

Line 448: Could changes in DCM chlorophyll be due to biological responses, not just physical mixing? This relates to the concern above about the mixing assumption. The equation on line 454 is also unclear and needs more explanation

Resp.: We agree that the presentation of the equation on line 499 could be clearer. The underlying idea is straightforward: the loss of chlorophyll-a in the DCM layer during HT relative to LT is assumed to be entirely due to turbulent fluxes. By conservation of mass, this turbulent loss from the DCM is redistributed upward to the surface layer and downward to the deep layer.

In our formulation (Eqs. 5–7), ΔCHL_{SURF} corresponds to the turbulent gain in the surface layer plus any biological contribution, and the turbulent comp5-7). Thus, the term on line 454, $\Delta CHL_{DCM} - \Delta Diff_{DEEP}$, simply represents the turbulent flux from the DCM that is directed upward into the surface layer. We have revised L497 the text to explicitly link this equation back to Eq. 5-7 and to clarify that it follows directly from the mass-conservation assumption applied to the turbulent redistribution between layers.

Line 474 -475: The phrasing is confusing: “deeper, less dense” or “upper, denser”? Clarify what part of the eddy is being described. Additionally, the reference to McGillicuddy et al. (line 478) requires more context. Greater depth compared to what?

Resp. : We have revised the sentence for clarity L519-521. In McGillicuddy’s framework, the doming part of an anticyclonic system can drive isopycnal uplift, potentially enhancing biological productivity by injecting nutrient-rich waters into the euphotic zone. In our case, while such isopycnal uplift is indeed observed within AE1, the anticyclone core appears too deep for this mechanism to significantly increase productivity, likely because the uplifted layers remain below the light-limited depth.

Lines 482-488: I think these results should be added in the section of the results where the authors do the spectrum analysis. Its presence here is unexpected and underdeveloped. Maybe other questions can be answered from these distinctions: why is it important to distinguish between these two types of oscillations (wind forcing, length scales, etc)? Have other papers discussed these differences, and do the results agree with your findings? Also, how was the spectrum in Figure 13 produced? The same as Fig. 8 but longer time series? What is the error bar, How many spectra were averaged, and what are the error estimates?

Is there any filtering applied to the data? Are they the same depth as Fig 8?

Resp.: We thank the reviewer for these suggestions. The main focus of this paper is on the impact of internal tides on chlorophyll-a, and a full comparison with other types of oscillations (e.g., near-inertial waves) would be beyond the scope of the present study. However, we anticipated that some readers might question the potential role of wind-forced near-inertial waves in driving mixing. For this reason, we retained Figure 13 in the discussion, not as a primary result, but as supporting evidence to address this possible question from the readership. The spectrum shows that the dominant oscillations are at the M2 tidal frequency, with only a minor peak at the local inertial frequency, consistent with Kouogang et al. (2025) for the Amazon shelf break.

We have now clarified in the manuscript that the spectra in both Figures 8 and 13 were produced using the same methodology: temperature data between 145–165 m, resampled at 30 min intervals, detrended, and processed using a Fast Fourier Transform with a Hanning window applied to reduce spectral leakage. Figure 13 uses the full one-month glider time series, whereas Figure 8 uses shorter sub-periods

corresponding to the defined study periods. No additional filtering was performed. Only one spectrum was computed from each aggregated time series, so no error bars are provided. While the analysis of Figure 13 has now been moved to the spectral results section to improve logical flow, its inclusion is maintained to explicitly address potential alternative explanations for vertical mixing raised by readers.

Line 504-416:This ecological context is appreciated and very usefull—perhaps connect it more explicitly to the tidal vs. near-inertial forcing context in terms of timescale?

Resp.: We agree that linking the ecological context to the relevant physical forcing timescales would be valuable. Near-inertial pumping occurs when spatial and temporal variations in wind forcing generate inertial oscillations, leading to alternating divergence and convergence zones in the mixed layer that drive vertical displacements of its base (Gill, 1984). This process can supply nutrients to the euphotic zone on inertial timescales (~7 days at our latitude), which differ from the semi-diurnal timescales of internal tides. However, spectral analysis of our glider records indicates negligible energy in the inertial band compared to the strong M2 tidal peak, suggesting that near-inertial processes were not a significant driver of vertical mixing during our observations. This is consistent with recent findings by Kouogang (2025), who showed that internal tides dominate vertical mixing over the Amazon shelf break, with near-inertial energy levels remaining low throughout the year. Therefore, while near-inertial pumping is an important process in other oceanic regions, its detailed investigation lies beyond the scope of the present study.

Minor comments

Line 97: Add more information about why Sep and Oct 2021 were an optimal period for IT activity

Resp.: Added L88-96

Line 214: A closing parenthesis is missing from the equation.

Resp.: Done

Line 217-219: If previous studies used a similar derivation for these equations, please cite them.

Resp.:Thanks for the comment we integrated in the paper L240-242.The set of equations used in this study is an adaptation of the NPZ-type framework presented in Franks (2002) to our observational case, in which vertical turbulent fluxes are primarily driven by internal tides. In this adaptation, the vertical mixing term is explicitly linked

to the cross-isopycnal turbulent diffusivity estimated for HT and LT phases, and the three-layer structure (surface, DCM, deep) is defined according to our in situ density and chlorophyll profiles. To our knowledge, this specific formulation has not been used in previous IT-focused studies.

Line 233: The terminology DCM (ΔDiff_DCM) is not clear to me, i suggest explaining what Diff(DCM) means.

Resp.: Here, $\Delta Diff_{DCM}$ refers to the change in depth-integrated chlorophyll-a (mg m^{-2}) within the DCM layer attributable to turbulent diffusive fluxes. We now precise it L.267-269

Line 267: Add reference to figure 3e-h

Resp.:Done L307

Line 421: Why was 0.2 mg/m³ used as the threshold for chlorophyll peak thickness?

Resp.: Because the minimum of value of DCM was around 4 and 0.2 is the half of for

Suggestions and minor questions

Line 79: Consider referencing Figure 1 to help the reader visualize the study area.

Resp.: done

Line 336: Is there a specific reason why you use 35.5 as the euhaline threshold? As someone unfamiliar with this region, this seems a high threshold.

Resp.:The threshold used to define euhaline waters comes from the Venice System for the Classification of Marine Waters (1958), which defines this category as having salinities between 30 and 40.

Line 338: I suggest referencing the black lines in Figure 3a when describing the cross of AE1

Resp.: Thank you for the suggestion. We have updated the text to explicitly reference the black lines in Figure 3a when describing the transect across AE1.