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.

General comments:

1. The results focus on a small area where it is known that internal tides have a significant influence on dynamics/mixing, termed a “hotspot” by the authors. I think something more needs to be said regarding the spatial distribution of such features; that is, whether the observations represent an extreme case or are typical. That might help clarify some of the implications of the work, e.g., whether internal tides are a key driver of variability as mentioned on line 39-41 in the abstract.

Resp.: We agree with the reviewer that providing a broader spatial context is important to determine whether our observations represent an extreme case or a typical situation. The continental slope off the Amazon has been identified in previous studies (Baines, 1982; Magalhães et al., 2016; Tchilibou et al., 2022) as a major generation site for mode-1 internal tides. Our results indicate that internal tides, through vertical mixing, tend to homogenize the chlorophyll profile throughout the water column. This effect occurs only when two conditions are met: (1) the presence of active internal tides, and (2) a well-developed deep chlorophyll maximum (DCM). Similar processes have been documented by Gaxiola-Castro et al. (2002) in the Gulf of California and are also concevable in other regions with strong internal tides propagation, such as the South China Sea and the Bay of Biscay. Finally, as our study area is not located directly at the generation site but rather in a region of strong internal tide propagation, our findings should be interpreted in this broader regional context . Lines 39 - 42 have been modified to make it clearer.

2. I think there needs to some more discussion regarding the separation of spatial and temporal variations in the glider data. That is, inherently a glider that moves in space will capture variations in both space and time but without additional context it will be unclear which is more important. There are related elements already in the text; e.g. the discussion of eddy evolution and the location of the segments relative to the position of the eddy. But, I don’t see any explicit mention of it. I think that is needed, even if the aliasing turns out to be minimal. Some discussion of how diurnal-weekly-monthly temporal variations, and the impacts on the observed spatial variability, would improve the manuscript.

Resp.: We agree with the reviewer that separating spatial from temporal variability is a key challenge in glider datasets. To address this, we added a paragraph in the Discussion (L539–560) explicitly describing our approach: the transect was divided into

four hydrographically distinct regions (A–D) to isolate spatial variability, and within each region, HT and LT phases were compared under similar water mass properties. Averaging over multiple tidal cycles further reduced short-term variability. In our case, the 26-day glider record was averaged to daily resolution, which smooths out high-frequency variability (e.g., semi-diurnal and diurnal cycles) while allowing us to capture changes over 1–3 days associated with internal tide activity. The lower-frequency spring–neap modulation (~15 days) is only partially sampled within this time window, meaning that our analysis quantifies the short-timescale (1–3 day) component of the internal tide impact on chlorophyll redistribution rather than the full fortnightly cycle. This framework allows us to interpret the differences between HT and LT as representative of the mean internal tide signal, while acknowledging that some residual aliasing between spatial and temporal variability remains unavoidable

3. I think some of the information provided in the methods section is not sufficient for the results to be reproducible. I have mentioned a few places where I think specific details are needed in the comments below.

Resp.: We have carefully addressed each of the specific points raised in the line-by-line comments and have added the requested clarifications to the Methods section.

4. No direct turbulence or mixing estimates are used in this paper. While I do not think this is a problem, I think it should be clear earlier in the manuscript. Much of the language in the abstract/intro/early sections attributes changes to vertical mixing, which may be (likely is) the case but is not shown directly in the paper. I would recommend a careful edit of these sections so this is clear to the reader. Related to this (see my comment for L518), it is implied that vertical mixing is entirely a result of internal tides. While possible alternate contributing factors are clearly mentioned in the discussion, I think it would be helpful if it were mentioned earlier before presenting the results.

Resp.: We have clarified in the Abstract (L33–35) that no direct turbulence measurements were collected in this study. We have also revised the wording in the early sections to indicate that the observed vertical redistribution of chlorophyll-a is consistent with tidally-driven cross-isopycnal exchanges, which represent the only physical mechanism to explain the transfer of biomass above and below the DCM and the observed variations.

Line-by-line comments and suggestions:

L26-27 – I think this sentence is unnecessary. It is already stated later that they do modulate nutrient availability/productivity.

Resp.: I've removed it

L33 – "remarkable" compared to what? Please clarify

Resp.: L.32 We agree with the reviewer that the term "remarkable" was subjective without a clear point of reference. We have revised the sentence to state explicitly that the 50% expansion refers to the difference between HT and LT states, removing the subjective qualifier.

L36 – clarify what contributes to the other 62%

Resp.: We updated it L.36 **.At the surface, turbulent fluxes contributed 38% of the chlorophyll-a increase, while the remaining 62% resulted from net biological activity (primary production minus grazing). Both processes directly influence primary production.**

L55 – This sentence feels a bit disconnected; discuss how it influences climate variability, i.e. through air-sea interaction.

Resp.: We have revised it to specify the mechanism, noting that surface-intensified mixing can alter sea surface temperature and thus modulate atmospheric convection and precipitation through air–sea interactions. l.63-65

L96-97 – I think it would be helpful to add a sentence/references regarding the seasonality of internal tides and mesoscale features.

Resp.: Done L.88 - 104

L111 – clarify that this is in optimal conditions with no currents

Resp.: Done

L116 – change "thanks to" to "from" or "using"

Resp.: Done

L117 – strange wording. Reword "enabling to estimate"

Resp.: Done

L139 – change "imagery" to "images"?

Resp.: Done

L153 – extra space after 05

Resp.: Done

L157 – "merges"

Resp.: Done

L177 – the URL could be moved to a data statement at the end

Resp.: Done

L194 – What specific hydrographic properties were used to classify the data into these periods? Was this done objectively?

Resp.: Updated L215-217**The classification into hydrographic periods was based on distinct changes in water mass structure, identified from temperature, salinity, and potential density profiles. Transitions between periods were detected by examining vertical profiles and T–S diagrams for shifts in stratification patterns and salinity ranges across isopycnal layers. This classification was qualitative rather than based on an automated algorithm, relying on consistent, visually discernible features in the hydrographic data.**

Fig 1 – On a related note, there seem to be breaks between periods A&B and B&C. Are these transitional periods? Why were they not classified into any of the primary subregions?

Resp.: Yes, these gaps correspond to transitional zones where the glider was moving between the hydrographically distinct regions defined in our analysis. Because the glider trajectory integrates both spatial and temporal variability, these transitional periods did not meet the criteria for homogeneous water mass properties used to define subregions A, B, C, and D. To ensure that comparisons between HT and LT were made under consistent hydrographic conditions, these transitional segments were excluded from the primary regional classification.

L204 – How was the aggregating done? Is it assuming that every measurement within that depth range is treated the same? Or, was there some type of vertical averaging?

Resp.: We have clarified in the Methods section (L224-238) that all measurements within the selected depth range (145–165 m) were treated equally, without applying vertical weighting. The individual measurements were concatenated into a single composite time series, resampled at 1 hour intervals, and linearly interpolated to produce a regular temporal grid before performing the spectral analysis. While no formal vertical averaging was performed, we assume that variability within this narrow depth band is coherent enough to be represented as a single aggregated signal.

L210 – I'm curious how large of a contribution is expected from the SMS term? Is this a source of uncertainty?

Resp.: The SMS term, encompassing biological sources and sinks (i.e., net primary production minus grazing), is indeed an important component in the chlorophyll-a balance. In our approach, we isolate the turbulent mixing contribution (Diff) using an isopycnal framework, which nullifies vertical advection. This method enables the

estimation of minimum turbulent fluxes and, by difference, the residual SMS term. For example, in Period A, biological processes account for approximately 57 % of the increase in surface chlorophyll-a, highlighting a substantial contribution from SMS. While we acknowledge that SMS estimation involves some uncertainty due to the lack of direct measurements of primary production and grazing, this residual approach is widely used for separating physical and biological contributions in observational studies. Importantly, even with this uncertainty, the relative magnitude of the SMS term consistently supports our interpretation that turbulent fluxes and biological processes jointly shape the observed chlorophyll-a variability

L226 – How are high and low tidal forcing defined?

Resp.: L263-265 Internal tides (ITs) are continuously present in the study region due to persistent barotropic tidal forcing over the topography. However, their intensity varies over time as a function of the spring–neap tidal cycle and local stratification conditions. In this context, High Tidal Forcing (HT) refers to periods within each observation window when internal tidal activity is most intense characterized by stronger isopycnal displacements. Conversely, Low Tidal Forcing (LT) corresponds to weaker activity with reduced vertical displacements. HT and LT are defined relatively to each other within each period.In short, HT corresponds to the period that is closer to spring tide conditions, while LT is the farther of the two.

L233 – "integrated in DCM at the DCM" – I don't understand what this wording means. I think you mean integrated in depth within the layer.

Resp.: Corrected L266_273

Fig 2 – Nice schematic that shows how Chl changes vertically due to internal tides. I think the colors are somewhat ambiguous. It is unclear whether green refers to a) the sum of CHL and SMS or b) just Chl-a from SMS.

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.

L254 – Would a Spearman correlation analysis potentially be more appropriate, considering that I think we would not expect a linear relationship?

Resp.: We agree with the reviewer that a Spearman ranked correlation is more appropriate in this case, as it does not assume linearity in the relationship. We have therefore recalculated the correlations using the Spearman method, and the revised values are now reported in the manuscript. updated L486

L261 – missing space after the period

Resp.: Done

L268 – I disagree with this… it looks to me like euphotic depth Zeu decreases in the eddy core and increases on the eddy periphery, in a similar pattern to chlorophyll as described in the later text.

Resp.: Ok Modified L309-310

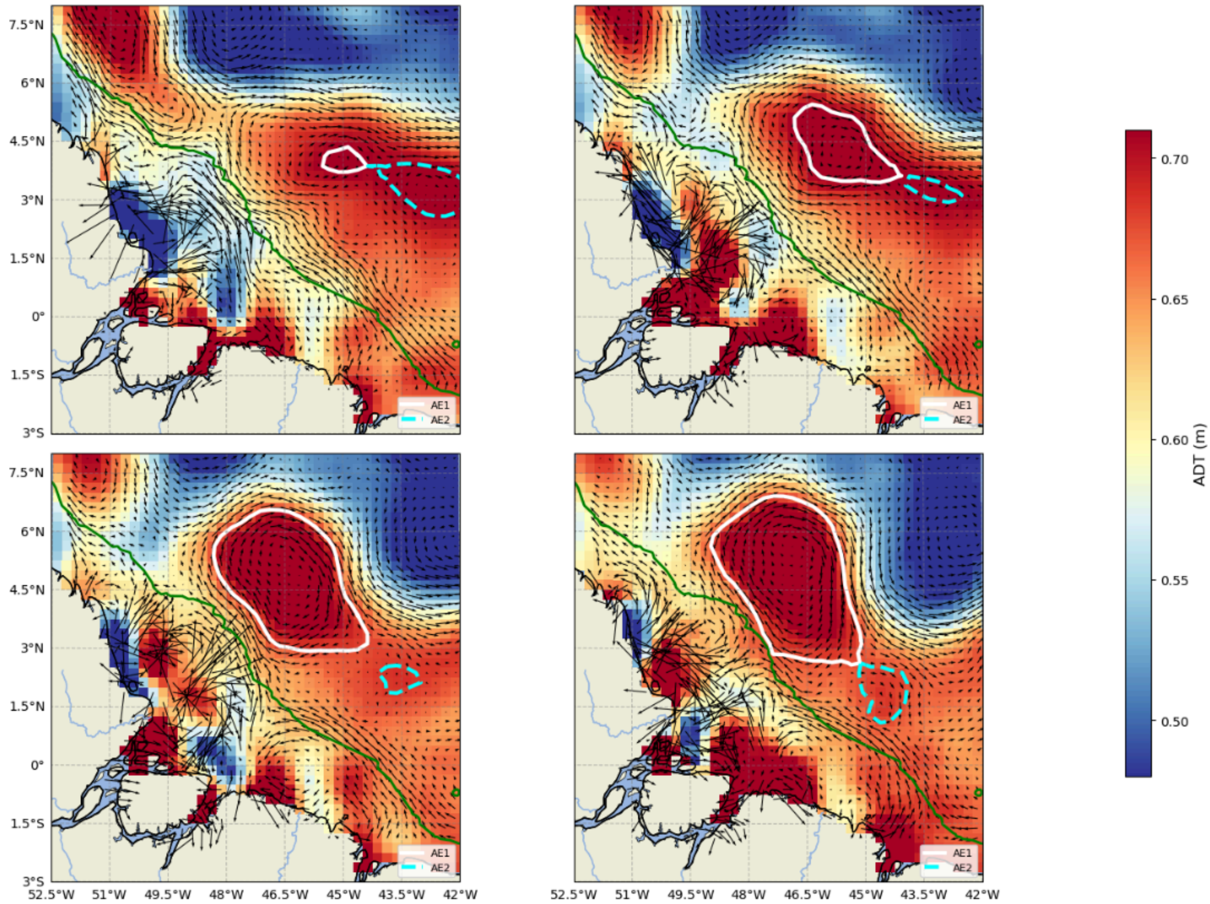
L277/279 – 11th / 12th

Resp.: Done

L277-280 – I'm not convinced there was "expansion" of the eddy. That seems, from the figure, to be an artifact of the cutoff ADT used to define the edge. Please reword to say that (or clarify if I am mistaken).

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.



L288-300 – Following my previous comment, it looks like Zeu and chlorophyll are correlated within the eddy, but that this correlation seems to break down when outside the eddy. I think an explanation of this would be helpful.

Resp.: Thank you for pointing this out. We have clarified in the manuscript that the estimation of Zeu follows the empirical relationship of Morel (1988) derived from surface chlorophyll-a concentrations cf L147, which explains the correlation observed between Zeu and chlorophyll-a within the eddy.

L336 – Fig 6 appears to show that salinity was always above 35.5

Resp.: Yes, salinity values in Fig. 6 remain consistently above 35.5 psu throughout the study period. This indicates that the study site was not significantly influenced by freshwater from the Amazon plume during our observations, and thus plume-related stratification effects are negligible in this case.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.

L333-352 – Nice summary. Much of the discussion on stratification is descriptive, however, and some of the trends mentioned in the text are not clearly apparent on the figure. For example, I do not clearly see more salinity stratification in region A than B, as is mentioned at L342. I think including quantitative information in a few places (i.e., dT/dz and dS/dz) would strengthen this section.

Resp.: Thank you for the suggestion. The quantitative differences in stratification (dT/dz and dS/dz) are already reflected in the T–S diagram, which synthesizes the vertical gradients of temperature and salinity for each hydrographic period. This representation was chosen as it provides a compact view of both water mass structure and stratification differences between periods. However, we understand that some of the trends mentioned in the text are not fully apparent in Fig. 6. Following your suggestion (Next comment), we have moved the paragraph discussing the differences between the four periods earlier in the section, and clarified in the text the link between the T–S diagram and the corresponding vertical gradients

L365-378 – The answers to some of my earlier comments are here. I think, perhaps, this should be moved earlier. I.e., discuss the four periods, then discuss their differences?

Resp.: cf previous answer

L368 – I think better to use 3 significant figures to be consistent here and in other places for the isopycnals

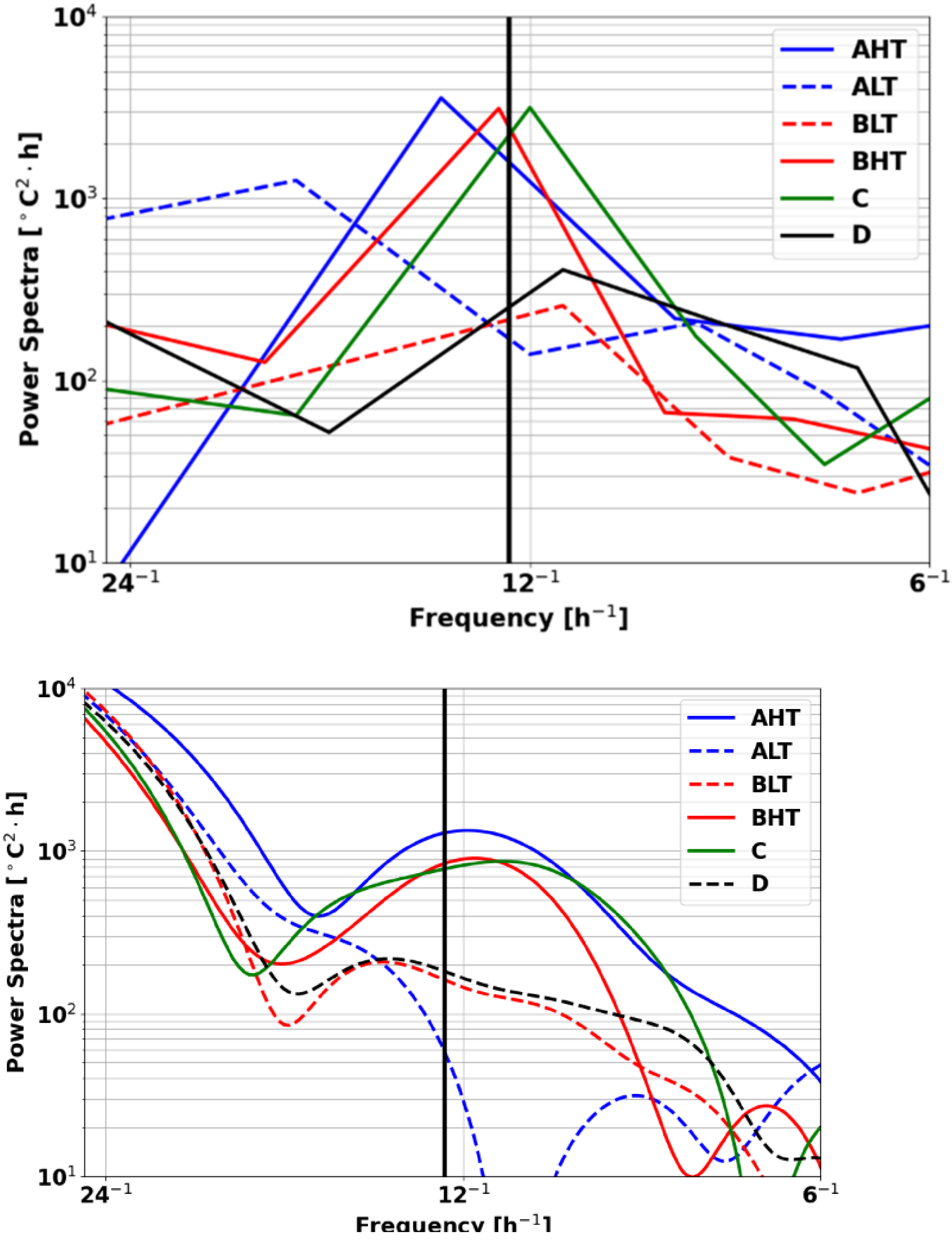
Resp.: done

L371 – "a distance was recorded" – odd wording; please rephrase

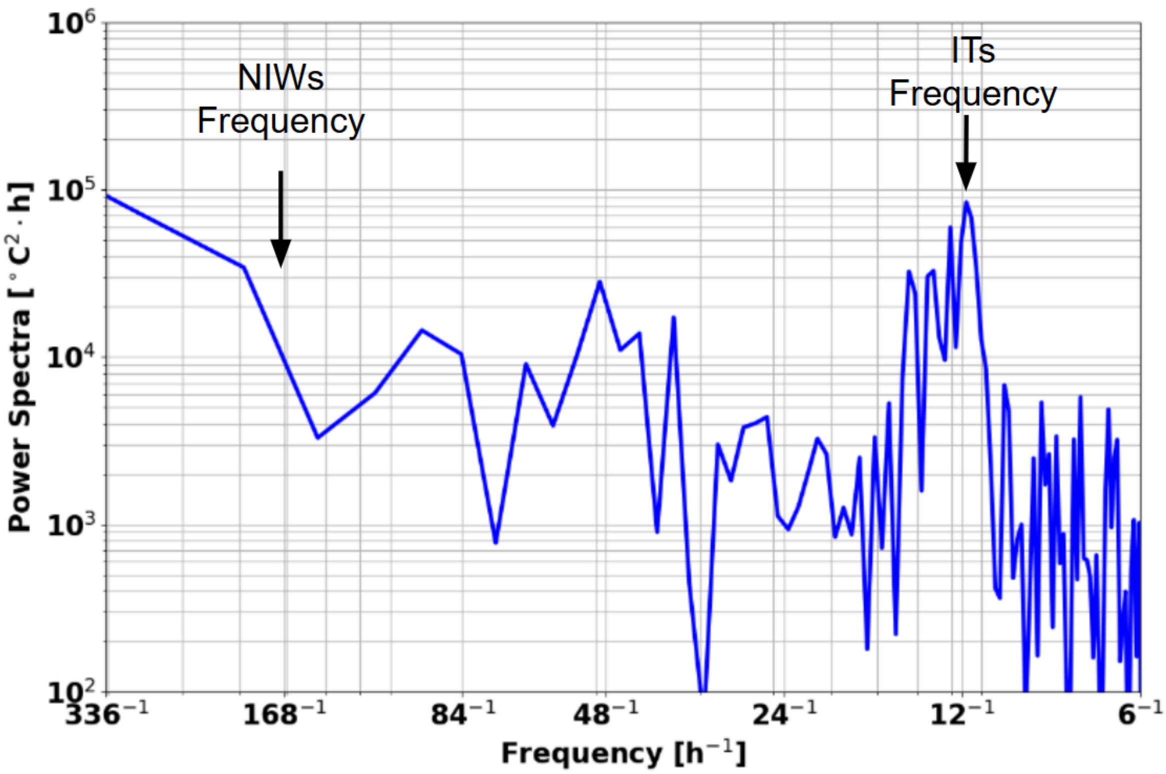
Resp.: done

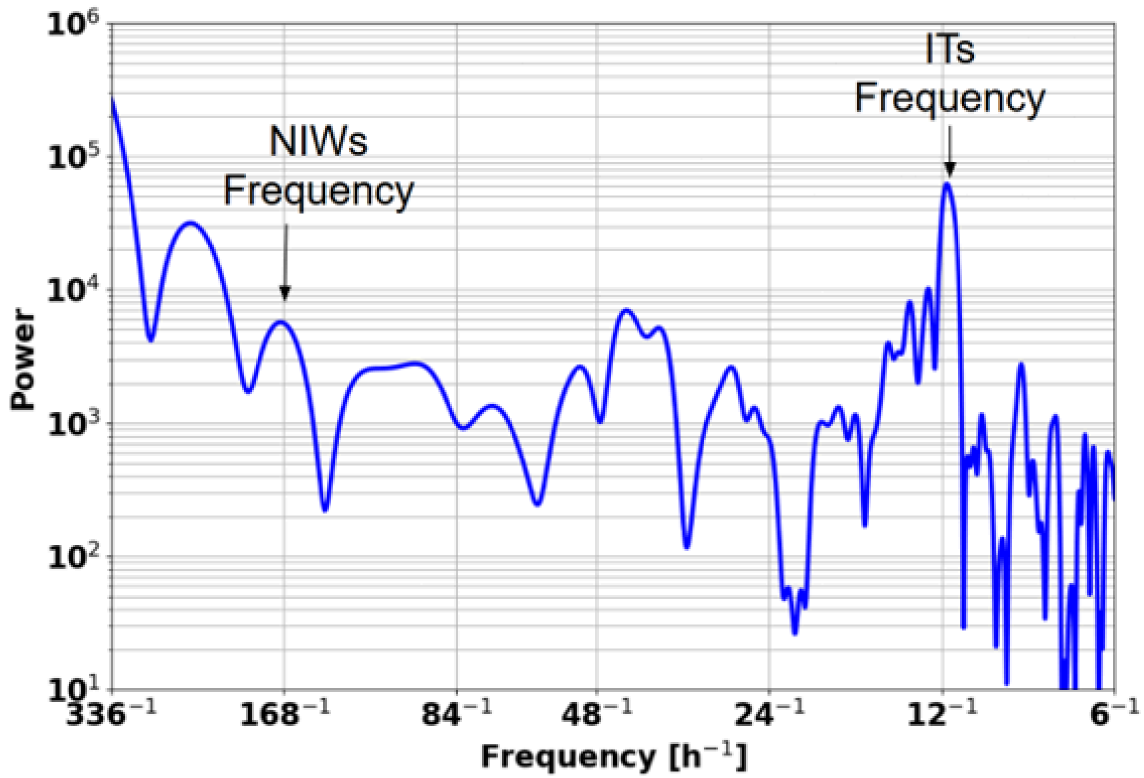
Fig 8 – Add units on the y-axis (& for Fig 13). Also, it seems strange to me that the spectra are so smooth (or, perhaps I am mistaken). Was any smoothing done to the lines on this plot? If so, I would suggest to just plot the raw spectra.

Resp.:Yes, the spectra in Fig. 8 were smoothed. Specifically, after resampling the time series to 1 h resolution, we applied a Hanning window to reduce spectral leakage, zero-padding to increase frequency resolution, and a 10-point moving average to smooth the resulting power spectra for clearer visualization. Following your suggestion, we have replaced these with the raw, unsmoothed spectra in the revised figure. here is raw spectra



up raw spectra for periods (AHT, ALT,BHT,BLT and D) / smoothed spectra (AHT, ALT,BHT,BLT and D)

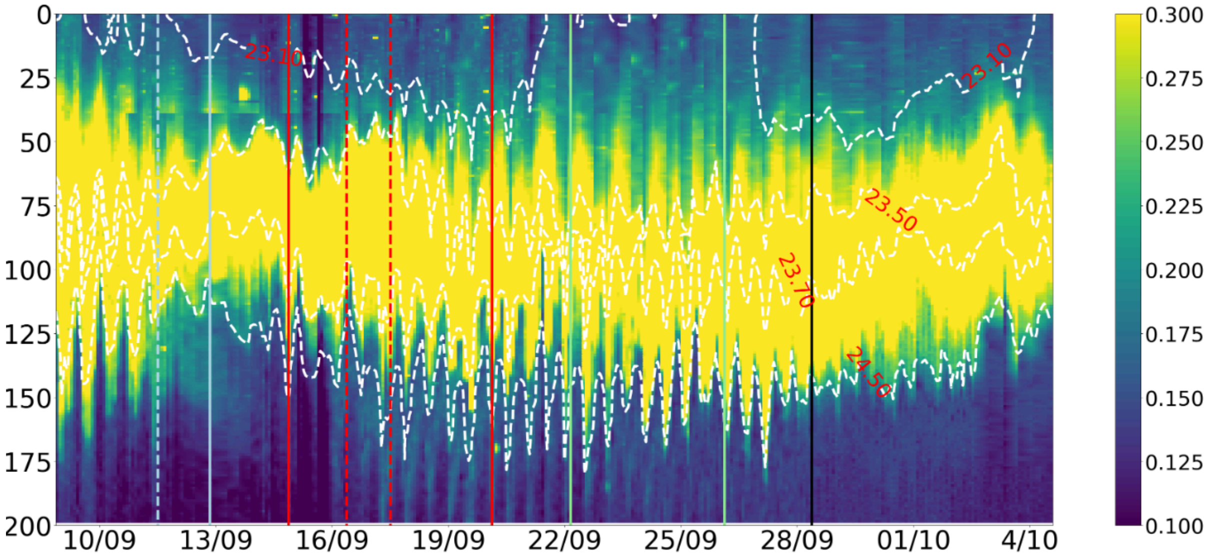




up raw spectra (Full Time series 145_165m) / down smooth spectra (Full Time series 145_165m)

L398-408 – It seems from the earlier plots that there is strong variability in surface chlorophyll. But this is not clear from Fig 9. Please explain this apparent discrepancy.

Resp.: The apparent discrepancy arises from the difference in colorbar scaling. Surface chlorophyll values typically range between 0 and 0.1 mg/m³, whereas in Figure 9 the colorbar spans from 0 to 0.8. If the same scale used in Figure 9 were applied to the surface chlorophyll plots, the variations would appear minimal or even indistinguishable

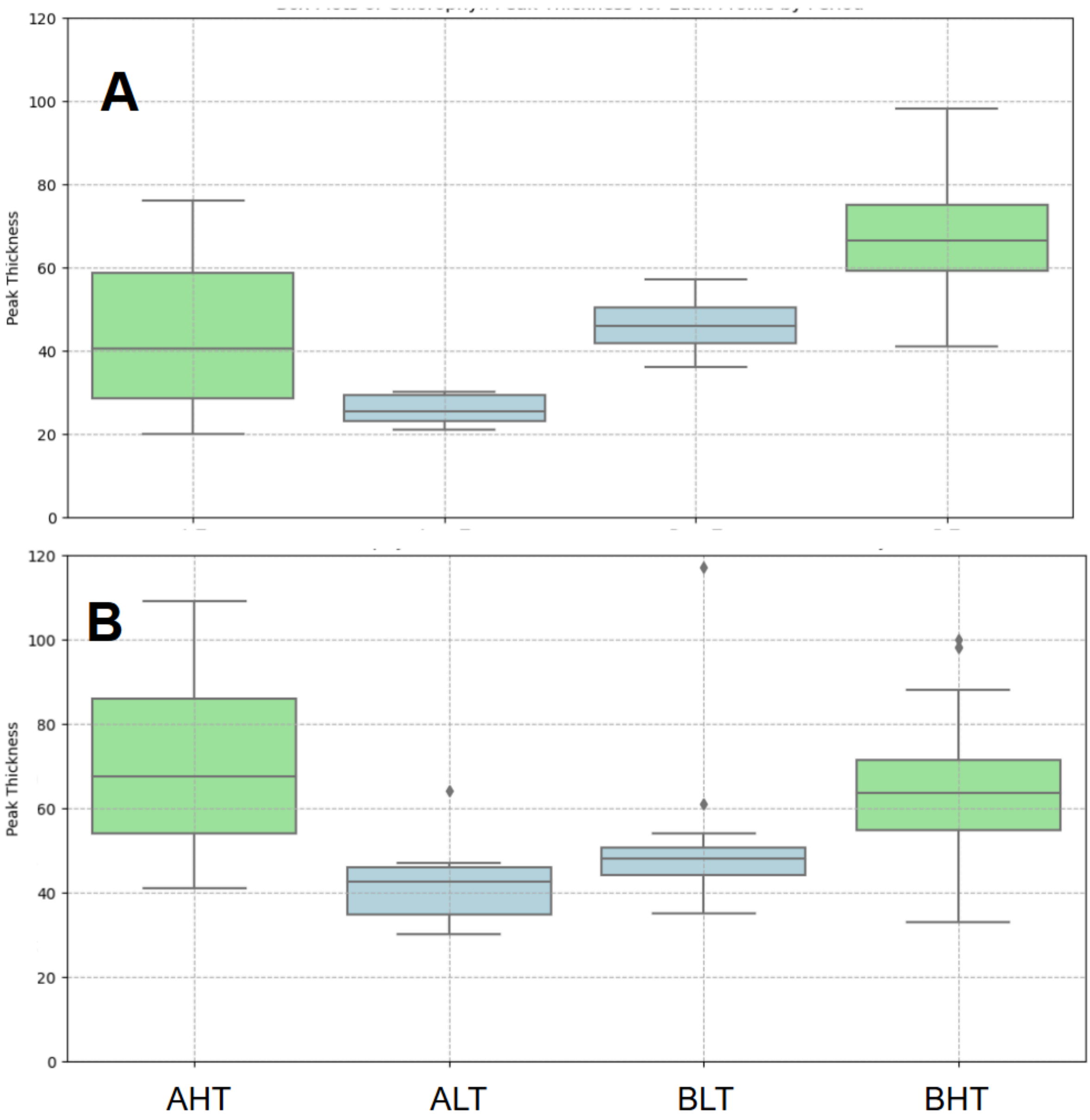


L419 – Odd wording. Maybe say “the peak is more pronounced”?

Resp.: Done

L422 – Was there significant temporal variability in the chl-a profiles? If so, perhaps a proportional criterion for thickness might work better?

Resp.: We evaluated both a proportional criterion (width at half of the chl-a maximum) and a fixed threshold criterion of 0.2 mg Chl m⁻³ (see boxplots in Figure below). In both cases, the results consistently show a broader DCM during HT compared to LT, leading to the same interpretation: internal tides promote an increase in DCM thickness. We chose to retain the fixed 0.2 mg Chl m⁻³ criterion because it allows us to illustrate how internal tides enhance the vertical dispersion of elevated chlorophyll concentrations. For example, during LT conditions, chl-a concentrations above 0.2 mg m⁻³ are typically confined within a 45 m band, whereas under HT conditions they extend over ~69 m, indicating a substantial vertical redistribution.



Boxplot A (up) Variable Criterion / Boxplot B(down) 0.2 criterion

L425 - While the relationship between thickness and high/low internal tide activity is very clear, I'm less convinced about the relationships between thickness and chlorophyll within the two IT regimes. It seems from Fig 11 that the high R values are because of peak thickness varies by much more than delta CHL, rather than a large change in chlorophyll. As suggested earlier, I think calculating a Spearman ranked correlation coefficient might be more appropriate, and would tell whether high values of Chla are associated with low values of thickness.

Resp.: Thank you for the suggestion. We have recalculated the relationships between chlorophyll-a and layer thickness within each internal tide period using the Spearman ranked correlation coefficient. For period A, the correlation was negative and moderate ($R = -0.44$, $p = 0.0125$), indicating that higher chlorophyll-a values tend to be associated with

lower layer thickness. For period B, the correlation was weaker but still significant (R = –0.29, p = 0.0377). These results are now reported in the manuscript, providing a more robust assessment of the relationship without assuming linearity. Updated L471-472

L428-430 – I think this is probably a stronger conclusion than the correlation coefficients (and is more clear in Fig 11). Maybe move earlier in the paragraph?

Resp.: Ok done

L447 – I'm a bit confused on how "chlorophyll-a loss" is calculated. From Fig 12, it does not look like the decrease in Chl at the peak is as high as 64%. Please clarify.

Resp.: . The decrease in chlorophyll-a at the DCM may visually appear small in Fig. 12 because the profiles are plotted against density (σ), not depth. Since the DCM lies within the pycnocline, where density changes rapidly with depth, this region spans only a few meters vertically. As a result, even narrow shaded areas in σ-space can correspond to significant vertical gradients and chlorophyll losses. The relative loss (64%) is computed based on the difference between peak values in each condition, not the area under the curve, and reflects the sharp decrease at the DCM across a narrow vertical extent.

L458 – Is there any SMS contribution to the DCM layer? I assume not based on the calculations in this paragraph.

Resp.: Yes, biological processes such as grazing, photosynthesis do occur within the DCM layer. However, following the results of Ma et al. (2023), we assume that these processes are, on average, in dynamic equilibrium over the tidal timescales considered here. As a result, their net contribution (SMS term) within the DCM is assumed to be negligible in our calculations, and the observed changes are primarily attributed to physical processes such as tidally-driven cross-isopycnal mixing.

L470-481 – nice summary of the differences between A and B.

Resp.: thanks

L493 – extra space between "ability"

Resp.: done

L518-524 –I think it would be useful to try to contextualize this more with existing literature – i.e. are there papers quantifying the impact of NIWs and fronts on chlorophyll. If not, are there any that have quantified physical turbulence parameters relating to these issues? I think having some additional background is important here, considering that the paper is based off of an implied assumption that the entirety of vertical mixing results from internal tides (which may be mostly true, but it would be helpful it this was put in context).

Resp.: We would like to clarify that our study does not assume that all vertical mixing originates from internal tides. Rather, our methodology is designed to construct contrasted periods—high tide (HT) vs. low tide (LT)—so that the main varying physical driver is the internal tide. This allows us to estimate the delta in chlorophyll distribution attributable to more energetic tidal phases. We acknowledge that internal tides can coexist with other physical processes, such as submesoscale fronts or mesoscale eddies, and can even act synergistically with them. However, by comparing HT and LT conditions, we isolate the incremental effect of tides on vertical mixing and chlorophyll redistribution.

In many oceanic regions, near-inertial waves (NIWs) are a dominant source of mixing and nutrient supply, with documented bio-optical impacts (Granata et al., 1995; McNeil et al., 1999). In our study area, however, spectral analysis of the glider data shows a clear and intense peak at the M2 tidal frequency, while the inertial band (~7 days at 2°–4° N) displays only a weak signal. This is consistent with Kouogang et al. (2025), who reported that internal tides dominate vertical mixing over the Amazon shelf break year-round, with low near-inertial energy levels. Our results therefore quantify the tidal contribution to mixing in a background state where other sources of variability are minimal. While the role of submesoscale fronts in modulating mixing and primary production is well recognized, an assessment of their contribution in our study area is beyond the scope of this work.

L575 – I'm not sure about the specific journal policy for this special issue regarding whether having data available upon request is acceptable.

Resp.: We are currently in the process of depositing the dataset in Seanoe in accordance with the journal's data availability policy. The data will be made openly accessible upon submission of the revised manuscript.