

We are very grateful to the Editor and the Reviewers for their constructive and relevant comments, which have helped us improve the quality and clarity of this work.

Please find below our detailed responses to each of the comments.

Figures prepared specifically for the review process are labeled as Figure RW X. Some of these figures will also be included in the revised manuscript; when applicable, this is indicated in the corresponding figure caption.

REVIEWER 2 :

This is an interesting and potentially valuable study investigating the role of tides in shaping nutrient and phytoplankton variability across the Amazon shelf–offshore continuum. The use of simulations with and without tidal forcing is a clear strength of the study and provides a useful framework for investigating the contribution of tidal processes. I found the results interesting, particularly the spatial differences between the shelf and offshore regions and the analysis of variability along the proposed internal-tide pathway.

My main concern, however, is that some of the conclusions appear to go somewhat beyond what is directly demonstrated by the analyses presented. I think the manuscript would benefit from a clearer distinction between the results directly supported by the analyses and the proposed mechanisms underlying them.

Major comments

1. Interpretation of chlorophyll changes and phytoplankton growth

The manuscript often interprets increases in chlorophyll as evidence that tidally supplied nitrate stimulates phytoplankton growth. However, in a system strongly influenced by vertical motions and internal tides, changes in chlorophyll concentration could also reflect the vertical or horizontal redistribution of phytoplankton, including displacement of the deep chlorophyll maximum, rather than increased production or biomass. If primary production or phytoplankton biomass is available from the model, examining these variables could help support the proposed interpretation. Otherwise, I would encourage the authors to use somewhat more cautious language when discussing enhanced phytoplankton growth.

We thank the reviewer for this very relevant comment. We fully agree that, in a region strongly affected by internal tides and associated vertical motions, changes in chlorophyll concentration may partly reflect vertical or horizontal redistribution of phytoplankton, including displacement of the deep chlorophyll maximum, and should therefore not automatically be interpreted as enhanced phytoplankton growth.

We would have very much liked to examine net primary production or a direct phytoplankton biomass diagnostic from the model, as this would have provided a more direct assessment of the biological response. Unfortunately, these diagnostics were not available in the archived model outputs used in this study.

To directly test whether the simulated chlorophyll response could be explained by Tides. We examined chlorophyll integrated over the full water column. This diagnostic (Figure 13 RW) is specifically useful in the present context because internal-tide-induced vertical displacements can strongly modify chlorophyll concentrations at a given depth while leaving the vertically integrated inventory unchanged. Therefore, a difference in full-depth integrated chlorophyll between TIDES and NO-TIDES cannot result solely from vertical heaving or displacement of the DCM and instead reflects a change in the total chlorophyll inventory of the water column.

At P1, near the internal-tide generation region, integrated chlorophyll increases by approximately 8.4 mg m^{-2} in TIDES relative to NO-TIDES, corresponding to an enhancement of about 63%. Farther offshore at P2, the increase is more moderate, approximately 2.5 mg m^{-2} or 7%. These results therefore demonstrate that the modeled chlorophyll response is not simply an artifact of vertical redistribution associated with internal-tide motions, but involves a genuine increase in the water-column chlorophyll inventory.

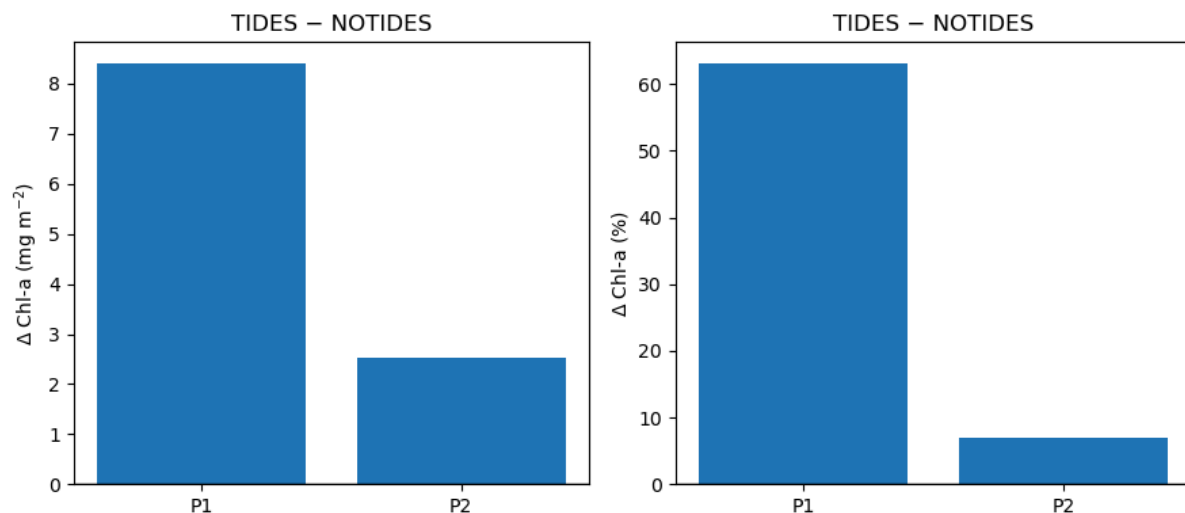


Figure 13 RW : Effect of tidal forcing on vertically integrated chlorophyll-a at the two selected locations. Left: absolute difference in chlorophyll-a inventory between the TIDES and NO-TIDES simulations (TIDES – NO-TIDES), expressed in mg m^{-2} . Right: corresponding relative change expressed as a percentage of the NO-TIDES value. (TO BE INCLUDED)

2. Interpretation of the reported 63% contribution to seasonal nitrate variability

I found the estimate that tides account for approximately 63% of seasonal nitrate variability interesting, but I was not entirely clear on how this percentage was calculated and how it should be interpreted. It would be helpful to provide a clearer definition of the metric, including how the spatial averaging was performed and how regions with small seasonal anomalies were treated. Since the analysis seems to focus on the difference between AMJ and ASO, I wonder whether describing this as a contribution to the AMJ–ASO seasonal contrast would be more precise than referring to overall seasonal variability, unless variability over the full seasonal cycle has also been considered.

Thank you for pointing this out. We agree that “seasonal variability” was too broad, since our analysis only compares AMJ and ASO. We now refer to the AMJ–ASO seasonal contrast.

The tidal contribution is calculated at each grid cell as:

$$\text{Tidal contribution} = 100 \times \{[(\text{AMJ} - \text{ASO})_{\text{TIDES}} - (\text{AMJ} - \text{ASO})_{\text{NOTIDES}}] / (\text{AMJ} - \text{ASO})_{\text{TIDES}}\}$$

Grid cells with very weak AMJ–ASO contrasts ($|\text{AMJ} - \text{ASO}| < 0.02 \text{ mmol m}^{-3}$) are excluded to avoid noise ratios since it's a percentage. The remaining values are then averaged over the domain, weighting each grid cell by the magnitude of its AMJ–ASO contrast.

Using this definition, tides contribute approximately 63% to the AMJ–ASO surface nitrate contrast over the analysed region. This value should therefore not be interpreted as a contribution to variability over the full seasonal cycle.

3. Comparison between modelled and satellite chlorophyll

The comparison between the modelled spring–neap chlorophyll signal at the depth of the DCM and satellite-derived chlorophyll should, in my view, be interpreted with some caution. Satellite observations represent near-surface conditions and cannot directly observe the DCM. The similarity in spatial patterns is certainly interesting and may support the presence of a tidal signal, but it does not directly validate the modelled subsurface response or mechanism. A comparison with model surface chlorophyll, if possible, might provide a more directly comparable assessment.

We thank the reviewer for this important comment. We agree that satellite chlorophyll represents near-surface conditions and cannot directly observe the deep chlorophyll maximum. Therefore, the similarity between the satellite-derived and modelled spring–neap patterns should not be interpreted as a direct validation of the modelled subsurface chlorophyll response or of the underlying mechanism.

In our study region, the satellite-retrieved chlorophyll signal can be considered representative of the upper part of the euphotic layer. Using the usual approximation that the first optical depth corresponds to approximately $Z_{eu}/4.6$ (Morel and Berthon, 1989), for an euphotic depth of about 100 m this corresponds to roughly the upper 20–25 m of the water column. In the model, however, chlorophyll concentrations are systematically higher than observed, and integrating chlorophyll over this upper layer tends to produce a larger-amplitude and noisier spring–neap signal, making the comparison with satellite observations less informative.

For this reason, we chose to compare the satellite-derived spring–neap spatial pattern with the modelled chlorophyll signal at the DCM, which is located at approximately 23 m in the model and therefore remains within the upper part of the euphotic layer. This choice also allowed us to address an additional scientific question that cannot be examined directly from satellite observations, namely how tidal forcing affects the DCM itself.

4. Attribution to internal tides

Although the manuscript focuses strongly on internal tides, the difference between the simulations represents the overall effect of including tidal forcing. Therefore, I think it would be helpful to distinguish more clearly between the net tidal effects demonstrated by the experiment and those that can specifically be attributed to internal tides. The analyses along the proposed internal-tide pathway are useful in supporting this interpretation, but the distinction should be reflected consistently in the discussion and conclusions.

We agree and we are going to make it clearer

5. Interpretation of the shifted spring–neap signal

The proposed explanation for the shift in the spring–neap spectral signal through tide–eddy interactions is interesting, but it currently seems more like a possible explanation than a mechanism that has been directly demonstrated. I would encourage the authors to make this distinction clearer and present this interpretation more cautiously unless additional analyses can directly support the proposed mechanism.

Thank you for this comment. We agree that the proposed role of tide–eddy interactions should be presented as a plausible interpretation rather than as a demonstrated mechanism. The current analysis does not directly isolate or quantify the contribution of tide–eddy interactions to the shift in the spring–neap spectral signal. We have therefore revised the manuscript to make this distinction clearer and to use more cautious wording throughout this section. We now present tide–eddy interactions as a possible mechanism consistent with the observed shift, rather than as a mechanism that has been directly demonstrated by our analyses.

Minor comments

The manuscript would benefit from careful proofreading and editing. I noticed many typographical and grammatical errors, some duplicated text, and inconsistencies in terminology and formatting, particularly in the naming of the simulations and in several figure captions.

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