

Reviewer 1

On behalf of all co-authors, I would like to thank you for investing your time in reviewing our manuscript and for providing constructive feedback that allowed us to prepare a revised version addressing the recommendations of both reviewers. We acknowledge that the initial submission of this manuscript required improvements in clarity and presentation, and we have carefully revised the text to address these points.

General comment

Comment: The model skill assessment appears limited and could be expanded.

Response: We agree that model evaluation is a crucial component of this study. In the original manuscript, we compared the selected variables only for the year 2023. In the revised manuscript, we have expanded the comparison to include additional years (2013, 2016, and 2020), representing normal conditions as well as strong El Niño and La Niña events. We have also clarified the limitations related to satellite data availability and uncertainty, particularly in coastal and upwelling regions. Furthermore, we have adopted a more conservative interpretation of the model skill assessment in the Discussion.

Specific comments

Comment: If access to satellite data was a limiting factor during model skill assessment, I think it would be helpful to the reader if this is explained.

Response: We thank the reviewer for this important comment. We now clearly state in the Methods and Discussion the satellite data availability and related uncertainties.

Comment: Also, why did you only test one value for the carbon-to-Chl-a ratio? According to the Xu et al. 2020 paper, the C: Chl-a ratio in the South China Sea varied from <20 in eutrophic waters (i.e., high chl a per C content), to > 90 in oligotrophic waters (low chl a per C content). It seems that to use c. 67 for these upwelling regions (eutrophic) could inflate the estimated Carbon biomass from Chl a satellite data, c.f. L210 and figure 3 d and h.

Response: We thank the reviewer for highlighting the variability of the carbon-to-chlorophyll-a (C:Chl-a) ratio in the South China Sea. Following the reviewer's suggestion, we now explicitly base our conversion on values reported in Xu et al. (2020). In that study, the basin-averaged C:Chl-a ratio was 67.77 ± 32.44 , with regional means of 64.13 ± 11.08 in the northern SCS, 74.83 ± 17.98 in the central SCS, and 67.73 ± 13.23 in the western SCS. Importantly, Xu et al. (2020) also showed that lower C:Chl-a ratios (~55–60) are characteristic of eutrophic and upwelling conditions.

Accordingly, we revised the satellite-derived phytoplankton carbon estimates by applying regionally and seasonally representative C:Chl-a ratios from Xu et al. (2020), with lower values during upwelling periods in the Vietnam and Sabah regions. This adjustment reduces the magnitude of satellite-derived phytoplankton carbon but does not alter the main conclusions regarding the relative spatial distribution and seasonal variability of phytoplankton biomass when compared with the model simulations.

Comment: In line 153 it is supposed to be Carbon: Chl a, not the other way around.

Response: The typographical error in Line 153 has been corrected to Carbon:Chl-a.

Comment: According to the Global Ocean Color website, the variables which can be obtained from this dataset also include phytoplankton functional types (diatoms, dinoflagellates etc.) Is there a reason why you did not compare your model output of diatoms to estimated biomass of diatoms from ocean color?

Response: We appreciate this suggestion. Although satellite-derived phytoplankton functional type products are available, they contain substantial data gaps in the regions examined in this study. To ensure adequate spatial and temporal coverage, we therefore used chlorophyll a as a proxy, even though this required an additional conversion step, rather than relying on individual functional type products.

Comment: Throughout the manuscript you should replace the word “observed” with “modeled” or “predicted” when you are describing modeling results, not physical observations. Similarly, in the discussion, you write that there “is a notable decline”, please rephrase so that it is clear that these are predictions.

Response: We fully agree and have revised the manuscript accordingly. The terminology has been made consistent throughout, including in the Discussion, to clearly distinguish model predictions from empirical observations.

Comment: I think you need to elaborate more in the discussion on the link between temperature, stratification and possible nutrient-depletion in the upper layers, and the corresponding predicted decline in diatom biomass. Since you put a lot of emphasis on the effect of increased stratification on nutrient (silicate) depletion and subsequent decline in diatom biomass, it would have been interesting to see modeled estimates of stratification /pycnocline depth. Is there a way to quantify stratification based on your modeling results? (for instance, calculating the pycnocline depth?)

Response: We thank the reviewer for this insightful comment. The Discussion has been expanded to better clarify the mechanistic links between ocean warming, enhanced stratification, reduced vertical nutrient supply, particularly silicate and the projected decline in diatom biomass. Although pycnocline depth would provide a direct metric of stratification, it was not calculated in this study due to gaps in the available model output. This limitation, along with the potential for future analyses incorporating pycnocline diagnostics, is now acknowledged in the manuscript.

Comment: Some parts of the discussion seem a bit unfocused, and you need to be more clear about which predictions you can actually make based on your modeling data, vs. more general assumptions.

Response: We agree and have revised the Discussion to clearly separate conclusions directly supported by model outputs from broader ecological interpretations, resulting in a more focused and conservative discussion.

Comment: Does the oceanographic model take into account river runoff?

Response: Yes, river runoff is included in the oceanographic model forcing. This has now been explicitly clarified in the revised manuscript.

Comment: You cannot actually conclude from your data that temperature in itself was a controlling factor.

Response: We agree. The text has been revised to avoid attributing causality to temperature alone and instead emphasize the combined influence of warming, stratification, and nutrient availability.

Comment: Very good that you are making these points, this shows how important your study is.

Response: We thank the reviewer for this positive comment and appreciate the recognition of the study's broader significance.

Reviewer 2

On behalf of all co-authors, I would like to thank you for investing your time in reviewing our manuscript and for providing constructive feedback that allowed us to prepare a revised version addressing the recommendations of both reviewers. We acknowledge that the initial submission of this manuscript required improvements in clarity and presentation, and we have carefully revised the text to address these points.

Major comments

Comment: The observed and model-stimulated phytoplankton carbon biomass for Sabah in Figure 3 is very different. I think this needs to be highlighted in the discussion for conservative interpretation of results.

Response: We agree. This discrepancy is now explicitly highlighted in the Discussion, and we emphasize that it introduces uncertainty in the quantitative interpretation of projected biomass changes for the Sabah region.

Comment: Figure 7d: I would not consider the overall non-diatom biomass for Sabah to be decreasing at all. Can you provide the gradient for the trend lines to get a better idea of the trend? I think it is important to distinguish between the average biomass compared to the spatial differences in figure 8. Similarly, I think the discussion lines 462-463 needs to highlight this better.

Response: We thank the reviewer for this important clarification. The Discussion has been revised to clearly distinguish between spatially averaged temporal trends and spatial heterogeneity shown in Figure 8. Statements regarding non-diatom biomass changes in Sabah have been made more conservative.

Comment: Figure 10: I am not convinced of the interpretation of the PCA plot. As you mention in the figure caption, the arrows representing the variables that are orthogonal have no relationship, yet the diatom, non-diatom and mesozooplankton variables are orthogonal to the temperature and silicate arrows, especially for the SE Vietnam plot. Yet in lines 443-444, you mention that the diatom biomass have a positive correlation with silicate with a high r value, and a negative correlation to temperature with both variables. This is especially strange since PC1 and PC2 explain 89% of the variance. Can you please double-check this analysis?

Response: We appreciate this careful observation. The PCA analysis and its interpretation have been re-examined, and the corresponding text has been revised to ensure consistency between vector orientation, explained variance, and reported correlations. The discussion of PCA results is now more cautious and avoids over-interpretation

Minor comments

Comment: Figure 6: would be good to have the same colour range for the legend for both nitrate and silicate for easy comparison.

Response: Revised accordingly.

Comment: Line 251 wrong spelling for Sabah

Response: Corrected

Comment: Line 510: “diatoms have a much higher nutritional value compared to protists”. Please change, as diatoms are protists as well.

Response: Revised to reflect that diatoms are protists, and the comparison is now made with *other* protists.

We once again thank the reviewers for their thoughtful and constructive feedback, which has substantially improved the quality, clarity, and robustness of this manuscript.