Dear Editor and Reviewers,

Thank you very much for your continued time and effort in evaluating our manuscript. We sincerely appreciate the constructive and thoughtful comments provided during the second round of review. Based on your guidance, we have carefully revised the manuscript once again and provide a detailed, point-by-point response to each of the reviewers' comments below. In particular, we have addressed Reviewer #1's concern regarding the absence of diel vertical migration (DVM) discussion, which has now been incorporated into the revised version (Section 4.1). We also followed Reviewer #2's recommendation to streamline Section 4.3 and the Conclusion, and to avoid redundancy and excessive station-level comparisons. Formatting and figure presentation issues were also addressed in detail.

The following is a detailed account of the specific revisions:

For Reviewer #1

General comments

The revised version of your paper answered most of my comments. I especially liked the analysis you added about the nepheloid layer. However, some figures are still to be improved to ease the understanding of the paper. In your answer, you affirmed that the potential effects of DVM were assessed in the new version of the manuscript, however, I did not find anything on that.

Response: Thank you very much for your valuable comments. We have revised the figure colors throughout the manuscript for better clarity and consistency, and we have also provided additional explanations regarding the interpolation artifacts in UVP-derived section plots.

Regarding the potential influence of diel vertical migration (DVM), we assessed particle abundance patterns at the two nighttime sampling stations, S2 and S4. At station S2, a high particle abundance was observed in the bottom layer; however, these particles are overwhelmingly small-sized, with almost no zooplankton detected based on image analysis. Therefore, we believe this high-value layer is unlikely to be related to DVM. In contrast, at station S4, a slight peak in particle abundance was observed in the upper 100 m. Notably, this layer also showed elevated zooplankton abundance (see Supplementary Fig. S2a), suggesting that DVM could have contributed to the elevated particle signal at this station. We have now clarified this point in the revised manuscript (in Discussion 4.1) to address your concern.

Specific comments

Introduction

I liked the changes you did, especially the details on the eddies and their impact on plankton, and the unanswered questions.

Response: Thank you very much for your positive feedback. These changes were made to better highlight the scientific context and significance of our study, and we are encouraged that you found them helpful.

Lines 62/64: this sentence refers to Picheral et al. 2022 which is about the UVP6 and not the UVP5 used in this study. Moreover, it does not seem useful from a scientific point of view.

Response: Thank you for pointing this out. We are correct that Picheral et al. (2022) focuses on the UVP6, while our study utilized the UVP5. We agree that this reference is not directly relevant to our methodology or scientific discussion, and we have removed this sentence from the revised manuscript accordingly.

Line 103: since anticyclonic eddies are more frequently observed in winter and early spring while cyclonic ones are more present during summer, do you know why you have both of them in June?
Response: Thank you for your insightful question. Although climatological patterns suggest that cyclonic eddies are more prevalent during summer and anticyclonic eddies during winter and early spring, both types of eddies can co-occur in the same season due to the complex mesoscale dynamics in the South China Sea.

In our case, the two eddies observed during the June cruise were detected based on sea level anomaly. Similar coexistence of cyclonic and anticyclonic eddies in one cruise has also been reported in previous studies (Xiao et al., 2025; Xu et al., 2025), suggesting that their generation mechanisms may include not only seasonal forcing but also factors such as wind stress curl variability (Metzger, 2003; Xiu et al., 2019), current instabilities, and Kuroshio intrusions (Wang et al., 2020). Therefore, it is entirely plausible to observe both cyclonic and anticyclonic eddies during the same month, such as in June, as seen in our study.

Metzger. 2003. Upper ocean sensitivity to wind forcing in the South China Sea. Journal of Oceanography, 59: 783-798.

Wang Q, Zeng L L, Chen J, He Y K, Zhou W D, Wang D X. 2020. The linkage of Kuroshio intrusion and mesoscale eddy variability in the northern South China Sea: subsurface speed maximum. Geophysical Research Letters, 47(11): e20020GL087034.

Xiao Y F, Zhuang Z P, Xin M, Cui T W, Liu R J, Ma Y, Liu H Y, Wang D Q, Xu T F, Chen L. 2025. Bio-optical properties of adjacent cyclonic and anticyclonic eddies in the central and western South China Sea during the summer. Progress in Oceanography, 238: 103547.

Xiu P, Dai M H, Chai F, Zhou K B, Zeng L L, Du C J. 2019. On contributions by wind-induced mixing and eddy pumping to interannual chlorophyll variability during different ENSO phases in the northern South China Sea. Limnology and Oceanography, 64(2): 503-514.

Xu W L, Wang G F, Xing X G, Cornec M, Hayward A, Chen B Z, Chen X H. 2025. Mesoscale eddies drive phytoplankton-mediated biogeochemistry in the South China Sea. Journal of Geophysical Research, 130(6): e2024JG008664.

Material and methods

Line 165: You well precised the UVP version you used, however try to be consistent in the rest of the paper: sometimes you referred to it as UVP, UVP5 or even UVP5-HD.
Response: Thank you for your careful reading and helpful suggestion. We agree that consistency in terminology is important. In the revised manuscript, we have standardized the terminology and now consistency refer to the instrument as UVP5-HD, which accurately reflects the version used in this study.
Line 168/170: does this sentence refer to Picheral et al. 2010? Or did you also release some calibration experiments? It is not really clear.
Response: Thank you for pointing this out. The sentence refers to the standard calibration procedure described by Picheral et al. (2010). We did not perform independent calibration experiments in this study, but followed the established pixel-to-metric unit conversion parameters provided and validated by the instrument manufacturer and described in the referenced literature. We have revised the sentence in the manuscript to clarify this point and avoid confusion.

Lines 171: in your answer, you precised the softwares you used. I think it would be better to add them directly in the manuscript too.

Response: Thank you for the suggestion. We have added the names and versions of the image analysis software used in our study directly into the manuscript for clarity and completeness.

Line 211: the formula is now readable, thank you. Did you still used Stokes' Law to determine the sinking velocity of particles? As precised in the first version.

Response: Thank you for the follow-up question. In the revised version, we don't use Stokes' Law to estimate sinking velocity. Instead, we adopted the empirical parameterization proposed by Guidi et al. (2008), which relates particle carbon content and sinking velocity as a function of particle size. This approach allows for direct integration of particle abundance and size spectra into carbon flux estimates, using the formula: $m(d) \times w(d) = A \cdot d^B$, with A and B as best-fit parameters derived from global sediment trap and UVP data. This approach has been widely adopted in many UVP-based studies and provides a practical means to estimate carbon flux when in situ sinking velocity measurements are unavailable.

Guidi L, Jackson G A, Stemmann L, Miquel J C, Picheral M, Gorsky G. 2008. Relationship between particle size distribution and flux in the mesopelagic zone. Deep-Sea Research I, 55: 1364-1374.

Lines 227/230: Did you used the same methodology for particle abundance? You did not precised it re-appeared in the interpolation method.

Response: Thank you pointing this out. Yes, we used the same methodology for particle abundance as for particle volume concentration and POC flux. All abundance data were binned into 5 m vertical intervals and then interpolated along transects to produce section plots. This was omitted in the previous version for brevity, but we have now explicitly clarified it in the revised manuscript to ensure consistency and transparency.

Results

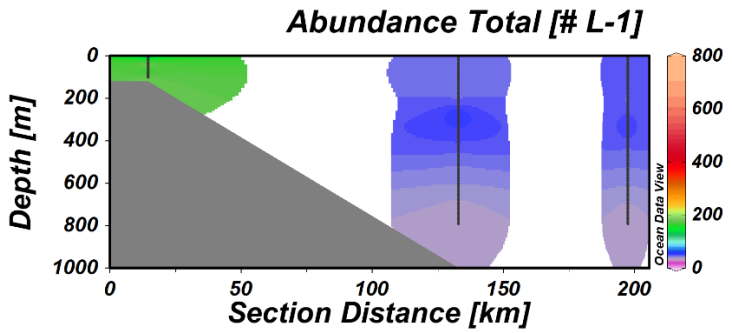
Fig. 2: it would have been nice to have the location of the cyclonic and anticyclonic eddy influenced stations as you did in Fig. 3 for example.

Response: Thank you for the suggestion. We have updated Fig. 2 to clearly indicate the stations influenced by cyclonic and anticyclonic eddies, consistent with the presentation in Fig. 3.

Fig. 4/5: why is there some interpolated data at the location of S7 while you said in the caption of Tab. 1 that the instrument was malfunctioning at this station? Especially for particle abundances where data were quite different between S8 and S6. I understand that it is due to the interpolation you used, however, you actually have no idea of the water column at this location, hence I would not be confident in showing any data there.

Response: Thank you for your comment. We fully understand your concern regarding the interpolation across the area between S8 and S6, where UVP data were not available due to instrument malfunction. In the revised manuscript, we indeed attempted to address this by adjusting the interpolation settling in ODV. However, unlikely the interpolated section plots for stratified variables (e.g., Fig. 3), the figures generated from high-resolution 5 m binned UVP data (e.g., Figs. 4, 5, and 7) posed specific challenges.

In these cases, applying stricter gridding constraints (e.g., reducing interpolation radius) resulted in large blank areas not only between stations S8 and S6, but also between other neighboring stations such as S6 and S5 (see figure below), severely affecting the continuity and readability of the section. After careful consideration, we decided to retain the current interpolation settings to preserve visual consistency and clarity across all UVP-derived section plots. That said, we now explicitly note in the figure caption that station S7 had no UVP data and that interpolated values in this region should be interpreted with caution. We hope this clarification addresses your concern.



Lines 321/324: how did you determined the 40% threshold? Only by looking at Fig. 6 or did you do any test?

Response: Thank you for your question. The 40% threshold was used as a visual reference to facilitate interpretation of the vertical profiles, rather than a statistically defined cutoff. It represents a rough division between dominant and subordinate contributions of small particles to PVC, and was marked in Fig. 6 to help illustrate the contrasting patterns between eddy-influenced regions. We have now clarified this point in the figure caption to avoid misunderstanding.

Fig. 6: I guess that the yellow dotdash line represents the 40% threshold you are talking about in the text. Can you precised it in the caption? I think the colours might be confusing, you choose blue for cyclonic eddy influenced stations and red for anticyclonic (in the previous figures) and now red and blue represent large or small particles. Maybe you could try to harmonise colours throughout your entire paper, and if you stick to red and blue for ACE and CE then Fig. 6a could be improved too.

Response: Thank you for your helpful suggestion. We have added an explicit note in the figure caption indicating that the yellow dot-dash line represents the 40% threshold discussed in the main text. To avoid confusion with the color scheme used in previous figures—where red and blue indicated anticyclonic and cyclonic eddy-influenced stations, respectively, we have revised the color scheme in Fig. 6 accordingly. Specifically, in Fig. 6a, stations S4 and S5 (ACE) are now shown in red and pink, while stations S11 and S12 (CE) are displayed in blue and light blue. In panels b-e, we have also adjusted the color palette to eliminate the use of red and blue for particle types, thereby maintaining color consistency throughout the manuscript.

Fig.7: Again, why is there interpolated data at the location of S7? Especially since it is POC flux that you derived from UVP data, that you don't actually have at S7.

Response: Thank you for your observation. As noted in our earlier response, no UVP data were collected at the region between S8 and S6 due to instrument malfunction. We acknowledge that the interpolated fields shown in Fig. 7 include a gap-filling interpolation across this region. During the first round of revision, we attempted to mask or exclude this region entirely (as we did for Fig. 3), but due to the higher vertical resolution (5 m bins) in the UVP-derived datasets, this resulted in visually disruptive gaps in horizontal interpolation fields (as shown in the image provided previously). As a compromise, and to ensure a more coherent and readable figure layout, we decided to retain the interpolated field over the S7 region, while clearly indicating in the caption that no data were actually collected at this station. We hope this approach is acceptable.

Fig.8: Same here for the colours.

Response: Thank you for pointing this out. We have revised the color scheme in Fig. 8b-e to ensure consistency with earlier figures. Specifically, stations influenced by anticyclonic eddies (S4 and S5) are

now shown in shades of red, and those influenced by cyclonic eddies (S11 and S12) in shades of blue, following the color convention used throughout the manuscript.

Discussion

Since you now discussed a lot more of the shore influenced stations, I wonder if it would be pertinent to highlight them on the map and figures, as you did for ACE and CE stations.

Response: Thank you for the helpful suggestion. While we agree that shore-influenced stations are relevant to the discussion, we did not specifically highlight them in the figures for two main reasons. First, these stations are already easily distinguishable as the innermost stations on each transect, making additional labeling unnecessary. Second, to maintain figure clarity and avoid excessive categorization, we limited the visual distinction in the plots to the anticyclonic (ACE) and cyclonic (CE) eddy-influenced stations, which are central to the objectives of our study. We believe this approach ensures better visual readability while still allowing readers to identify the nearshore locations if needed.

Lines 447/449: can you elaborate please? One can imagine that the particles at the bottom were a few days before in the top layer? (you give some elements of discussion at the very end of the paragraph–talking about zooplankton–maybe it could be nice to have this point just after the highlighted lines)

Response: Thank you for your comment. We agree that vertical settling from the surface could be a potential source of deep particle accumulation. However, in our case, the elevated abundance of small particles at the bottom layers of S2 and S10 is unlikely to have originated from surface export. If vertical settling were the dominant process, we would expect a more continuous vertical distribution of particles and similar signals at adjacent stations. However, the particle abundance in the upper water column at S2 and S10 was relatively low, and nearby station S3, which is located close to S2, exhibited much lower particle abundance in the deep layer. This spatial mismatch suggests that the bottom enhancement is unlikely to be due to vertical transport from above. Instead, we propose that the elevated abundance of small particles at S2 and S10 likely originated from lateral or bottom sources. Notably, both stations are located in the mid-slope region, where intermediate nepheloid layers are more likely to occur. Similar features have also been reported in previous studies (Jia et al., 2019; Chen et al., 2024), supporting this interpretation. Additionally, following your suggestion, I moved the discussion of zooplankton to an earlier part of the paragraph.

Jia, Y. G., Tian, Z. C., Shi, X. F., Liu, J. P., Chen, J. X., Liu, X. L., Ye, R. J., Ren, Z. Y., Tian, J. W.: Deep-sea sediment resuspension by internal solitary waves in the northern South China Sea. Sci. Rep., 9, 12137, https://doi.org/10.1038/s41598-019-47886-y, 2019.

Chen, T., Liu, X. L., Bian, C. W., Zhang, S. T., Ji, C. S., Wu, Z. S., Jia, Y. G.: Nepheloid layer structure and variability along the highly energetic continental margin of the northern South China Sea. J. Geophys. Res., 129(2), e2023JC020072, https://doi.org/10.1029/2023JC020072, 2024.

Line 524: Steinberg et al 2023 is only about salp faecal pellets, not zooplankton.

Response: Thank you for pointing this out. The reference to Steinberg et al. (2023) has been removed from the revised manuscript to avoid confusion.

Lines 580/588: the paragraph on modelling eddies is quite interesting, however I am not sure it belongs here. The subject seems a little bit far from the primary objectives of the paper, and applying it to a model

doesn’t seem that straightforward to me, especially since you don’t give any example of models revolving eddies.

Response: Thank you for your comment. We agree that the discussion on biogeochemical modeling was somewhat beyond the primary scope of this study. As suggested, we have removed the related content from the revised version to maintain focus and ensure that the discussion remains closely aligned with our main objectives.

For Reviewer #2

Both Discussion section 4.3 and the Conclusion contain redundant content. As background information on eddy-mediated nutrient availability and phytoplankton community structure has already been presented earlier in the manuscript, section 4.3 should focus directly on the observed differences in PVC and particle size composition between the two eddy regions, providing concise, data-driven explanations. Avoid station-by-station comparisons, as the two eddy regions have already been clearly defined.

Response: Thank you for your insightful suggestion. We agree that Section 4.3 and the Conclusion previously contained some redundancy, and that the Discussion would benefit from a more focused, data-driven structure. In the revised manuscript, we have streamlined Section 4.3 by removing background information on eddy-induced nutrient dynamics and phytoplankton biomass that had already been introduced earlier. Instead, we now focus directly on the key differences in PVC and size composition between the cyclonic and anticyclonic eddy regions. We also removed the station-by-station comparisons and emphasized broader patterns at the regional scale. The Conclusion section has likewise been revised to avoid repetition and to better synthesize the main findings.

The Conclusion should then succinctly summarize the main findings and highlight their broader implications.

Response: Thank you for this helpful suggestion. In the revised version, we have streamlined the Conclusion to succinctly summarize the key findings of our study and emphasize their broader implications for understanding particle-mediated carbon export in marginal seas. We removed redundant content and focused on presenting a concise synthesis of the main results and their significance.

In the Fig. S1 caption, there is an extra space between ‘μ’ and ‘mol’ that should be removed.

Response: Thank you for pointing this out. We have corrected the unit formatting in the Fig. S1 caption by removing the extra space between ‘μ’ and ‘mol’.

Once again, we sincerely thank you for your constructive feedback. We hope that our revisions and responses have adequately addressed your concerns and meet your expectations.

Best regards,

Shujin Guo