Dear reviewer,

Thank you very much for your valuable suggestions. Your comments have been extremely helpful, and we have done our best to revise the manuscript accordingly. We hope that the revised version meets your expectations. The following is a detailed account of the specific revisions.

1. General comments

Your article and the questions you raised are interesting. However, I have the impression that you could learn more from the UVP data. I don't really understand why living particles aren't separated out and studied separately from non-living particles. I know this is the method used by Kiko et al. 2022, but I would have liked a few sentences explaining why we can make the assumption that living particles can be counted as non-living particles. Moreover, in Kiko et al. 2022 the vast majority of particles are indeed non-living (by image visualization), do you have any idea of the percentage of living particles imaged in your dataset? Depending on the location, this could be very high.

This distinction would allow several things to be done, depending on the level of classification you use:

Living/non-living classification: confirmation of physical processes on biology, not only via chl-a as you are currently doing, but also on the distribution of zooplankton. It was for example done in Trudnowska et al. 2021 or Perhirin et al. 2025.

Living/non-living classification according to shape of the particles: this would make it possible to refine the calculation of biomass or the approximation currently made with the ESD.

Response: Thank you for your thoughtful and constructive comments. We appreciate your suggestion regarding the separation of living and non-living particles and the potential value of such classification. In our study, we used the UVP5.0-HD system, which generates vignette images only for particles with an ESD greater than approximately 500 μm. Therefore, it is not technically possible to classify smaller particles (ESD < 500 μm) as living or non-living, as they are not imaged individually. While classification of larger particles is feasible, our study primarily focuses on the distribution, size composition, and carbon flux of the entire particle spectrum in response to mesoscale eddies. Small particles account for a substantial portion of total particle abundance and volume and cannot be excluded from the analysis. For this reason, and in line with many previous UVP-based studies (e.g., Ramondenc et al., 2016; Fender et al., 2019; Clements et al., 2023), we treated all detected particles together without separating living and non-living fractions.

Regarding your concern that his may affect the accuracy of carbon flux estimates: we used the empirical parameterization developed by Guidi et al. (2008), which relates total particle size spectra (including both living and non-living components) to sediment trap-derived POC fluxes at a global scale. This approach has been widely adopted and validated, and does not require prior separation of particle types. The only potential source of uncertainty in this method lies in whether the empirical parameters derived from the global dataset in Guidi et al (2008) are fully applicable to the SCS. This issue has been addressed in the Discussion section.

That said, we agree that studying the biological component of the particle pool—especially zooplankton—is a valuable direction. Inspired by your suggestion, we analyzed the zooplankton vignettes from large particles identified from UVP data and observed elevated zooplankton abundance in eddy-affected regions. We have now included these results in the revised manuscript and expanded the discussion to address potential eddy effects on zooplankton distribution. However, the primary focus of

this study is on particles. The zooplankton-related findings, while valuable, will be explored more comprehensively and in greater detail in a separate manuscript we plan to prepare.

Finally, we truly appreciate your expertise reflected in the review, and your insightful comments suggest a strong expertise in this field. If possible, we would be very interested in the opportunity to collaborate with you in the future, especially on the zooplankton dynamics with UVP study in the western Pacific. If you are interested, please feel free to contact me or just leave your message. Thanks.

Clements D J, Yang S, Weber T, McDonnell A M P, Kiko R, Stemmann L, Bianchi D. 2023. New estimate of organic carbon export from optical measurements reveals the role of particle size distribution and export horizon. Global Biogeochemical Cycles, 37, e2022GB007633.

Fender C K, Kelly T B, Guidi L, Ohman M D, Smith M C, Stukel M R. 2019. Investigating particle size-flux relationships and the biological pump across a range of plankton ecosystem states from coastal to oligotrophic. Front. Mar. Sci. 6: 603.

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.

Ramondenc S, Goutx M, Lombard F, Santinelli C, Stemmann L, Gorsky G, Guidi L. 2016. An initial carbon export assessment in the Mediterranean Sea based on drifting sediment traps and the Underwater Vision Profiler data sets. Deep-Sea Research I, 117: 107-119.

Wang Z Y, Fang C, Yang C H, Zhang G Y, Sun D. 2024. Latitudinal gradient and influencing factors of deep-sea particle export along the Kyushu-Palau Ridge in the Philippine Sea. Science of the Total Environment, 906: 167460.

Next, two of your deployments were done by night (S2 and S4), could the presence of DV migrators have affect your results?

Response: Thank you for this important observation. We acknowledge that diel vertical migration of zooplankton may influence the abundance and composition of large particles observed in night-time deployments. In our study, two stations (S2 and S4) were indeed sampled during nighttime. Although our dataset is not designed to specifically evaluate the effect of DVM, we now discuss this potential bias in the revised manuscript and acknowledge it as a possible source of uncertainty in the interpretation of particle abundance and POC flux at these stations.

I also feel that there are a number of missing elements that detract from the manuscript's readability. For example, the methodology used for some the analyses is very opaque, the total number of particles observed is never indicated, etc. Some figures are not very clear. I detail these in the following comments.

Response: Thanks for the comment. In the revised manuscript, we have clarified the analytical procedures, added missing information such as the total number of particles observed (now included in Table 1), and improved the quality of several figures based on your comments. We hope this revision improve the readability of our work.

2. Specific comments

Abstract

Line 21: precise which deviation you used

Response: Thank you for pointing this out. We used the standard deviation (SD) in the expression "68 ± 69 particles L^{-1}". We have now clarified this in the revised abstract.

Line 25: you might want to give the mean and deviation as for the particle abundances.

Response: Thank you for the helpful suggestion. We have now added the mean and standard deviation for the estimated POC fluxes, consistent with the presentation of particle abundances. The revised sentence in the abstract reads: "Estimated POC fluxes ranged from 3.4 to 302.4 mg C m^{-2} d^{-1} (mean ± SD: 33.6 ± 34.9 mg C m^{-2} d^{-1}), with maxima in the upper 100 m and at stations influenced by cyclonic eddies.".

Lines 25/26: your data 'only' covers two stations influenced by an anticyclonic eddy and two in the periphery of a cyclonic eddy, I don't think that's enough to be able to generalize or to use the plural form of eddy in these two sentences.
Response: Thank you for this observation. We agree that our data coverage is limited, with only two stations associated with an anticyclonic eddy and two at the periphery of a cyclonic eddy. We have revised the relevant sentences in the abstract to avoid overgeneralization and removed the plural form of "eddy". The revised version now reads: "with maxima in the upper 100 m and at two stations located at the periphery of a cyclonic eddy. In contrast, at two stations influenced by an anticyclonic eddy, particle concentrations and export fluxes were reduced, likely due to enhanced stratification and nutrient limitation.".

Graphical abstract
I don't find your graphical summary very useful, it's not very clear. It also lacks a legend to give some context or explanation. I don't understand why the particles in the two eddies are identical. You've also included bacteria and phytoplankton, which are not visible under UVP (although I understand that phytoplankton is approximated by Chl-a in your study). It's not very easy to see that the abundance of small dots is the main difference between the two eddies. I am not sure about the downwelling eddy arrows. The sun is unnecessary. Maybe put some green shapes for chl-a or phytoplankton.
Response: Thank you for your detailed feedback regarding the graphical abstract. We agree that it did not effectively convey the core findings of the study and may have led to some confusion. Based on your suggestion, we have decided to remove the graphical abstract in the revised submission to maintain clarity and focus.

Introduction
I think you can try to focus a little bit more your introduction on the links between eddies and export, I especially regret some references that you cited only in the Discussion and that are in the topic and in the SCS (e.g. Liu et al. 2020).
Response: Thank you for this valuable suggestion. In the revised manuscript, we have rewritten the Introduction to better highlight the scientific rationale and objectives of this study. A new paragraph has been added to specifically introduce how eddies influence biogeochemical dynamics in the ocean.
Line 45: the beginning of the line is not very clear.
Response: Thank you for pointing this out. We have revised the sentence for clarity and now rephrased it as follows: "Therefore, understanding the distribution, size composition, and associated carbon export flux of marine particles is critical for assessing the efficiency of the biological carbon pump.".
Line 50: find a more appropriate reference (one using more sediment traps or at a more global scale).
Response: Thank you for the helpful suggestion. In response, we have replaced the previous reference with two more appropriate studies: Honjo et al. (2008), which provides a global synthesis of sediment trap data, and Harms et al. (2021), which focuses on particle flux variability using sediment traps in the South Indian Ocean.

Lines 58/60: add references (maybe Trudnowska et al. 2021).

Response: Thank you for the suggestion. We have now added the recommended reference (Trudnowska et al., 2021) as well as Trudnowska et al. 2023 to support the statements.

Line 94: maybe try to convert it in mg C m^{-2} d^{-1}.

Response: Thank you for the suggestion. We have converted the values reported by Hong et al. (2021) from mmol m^{-2} d^{-1} to mg C m^{-2} d^{-1} for consistency and clarity. The revised sentence now reads: "Hong et al. (2021) quantified cross-shelf POC export in the northern SCS shelf using ^{234}Th-based estimates and sediment traps, reporting a vertical POC flux of 312 mg C m^{-2} d^{-1} from the euphotic zone and a cross-shelf transport flux of 119 mg C m^{-2} d^{-1}.".

Material and methods

Line 119: three transects that are called 《sections》 in Fig. 1.

Response: Thank you for the helpful comment. Accordingly, we have revised Fig. 1 to replace "Section 1/2/3" with "Transect 1/2/3" to ensure consistency with the terminology used in the main text.

Lines 123/124: do you have any reference to cite on the multivariate optimal interpolation algorithm? Or can you at least give some details, because currently it is not very transparent nor reproducible.

Response: Thank you for your comment. The multivariate optimal interpolation algorithm used for generating the Level-4 Sea Surface Salinity (SSS) product is based on the method developed and described in detail by Buongiorno Nardelli et al. (2016), and more recently implemented regionally in Sammartino et al. 2022. We have cited the following references in the revised manuscript.

Buongiorno Nardelli, B., Droghei, R., Santoleri, R.: Multi-dimensional interpolation of SMOS sea surface salinity with surface temperature and in situ salinity data. Remote Sens. Environ., 180, 392–402, http://dx.doi.org/10.1016/j.rse.2015.12.052, 2016.

Sammartino, M., Aronica, S., Santoleri, R., Nardelli, B. B.: Retrieving Mediterranean Sea surface salinity distribution and interannual trends from multi-sensor satellite and in situ data. Rmote Sens., 14, 2502, https://doi.org/10.3390/rs14102502, 2022.

Fig.1: maybe colours the cyclonic/anticyclonic stations or indicate their position with a different shape.

Response: Thanks for your comment. To clarify the eddy-related station categorization, we have revised the figure and caption to explicitly state that red dots indicate stations influenced by the anticyclonic eddy, while blue dots represent stations located at the periphery of the cyclonic eddy.

Table 1: add the units for longitude, latitude, time (time zone). Please clarify if UVP deployment depth is the maximum depth of deployment or not. Add the total number of particles and the volume sampled per deployment. Why there is no station 7? You never mentioned or explained it.

Response: Thank you for the detailed and helpful comments on Table 1. In response:

We have added the appropriate units for latitude (°N), longitude (°E), and time (UTC + 8). We clarified in the table caption that "UVP deployment depth" refers to the maximum depth reached during each deployment. The total number of particles detected and the sampled volume per deployment have been added to the table. Station 7 was omitted due to instrument malfunction.

Line 139: add units for temperature, salinity and pressure.

Response: Thank you for the suggestion. We have now added the units for temperature (℃), salinity (psu), and pressure (dbar) in the description of CTD measurements for clarity.

Line 154: which version of the UVP5 you used? HD? SD?
Response: Thank you for pointing this out. We used the UVP5-HD version in this study. This information has now been added to the revised manuscript.

Line 156: the volume sampled by the UVP5 is 1.053 l per frame, please correct it.
Response: Thank you for pointing out this error. We have corrected the per-frame sampling volume of the UVP5 to 1.053 L in the revised manuscript.

Line 160: which custom software did you used? Can you give some details about it?
Response: We thank the reviewer for the constructive suggestion. In this study, the particle images collected by the UVP 5 were processed using the standard software suite developed by the Laboratoire d'Océanologie de Villefranche-sur-Mer, including ZooProcess (v7.22) and PkID (v1.26). These tools are widely used in UVP data processing workflows.

Line 164: do you have any idea on how the ESD approximation could have impact your volume concentration result? If you upload your images in ecotaxa, major axis and minor axis are computed for each particle and thus it is possible to use an ellipsoidal shape (that would maybe be more accurate than the ESD), or maybe your personal software can do it.
Response: Thank you for raising this important point. In our study, particle volume was estimated based on the equivalent spherical diameter (ESD), assuming spherical geometry, which is a commonly used approach in UVP-based analyses (e.g., Guidi et al., 2008; Fender et al., 2019; Wang et al., 2024). We acknowledge that his assumption introduces uncertainty, especially for irregularly shaped particles such as marine snow aggregates and fecal pellets. While we agree that using major and minor axes to derive ellipsoidal volumes (e.g., via Ecotaxa) can improve accuracy for individual particles, our current instrument (UVP5-HD) only records axis data for particles with extracted vignette images—typically the larger particles ($\geq \sim 500$ μm). For the majority of smaller particles, no image or shape data are available, thus precluding the consistent application of ellipsoidal geometry across the full size spectrum. Given this constraint, we retained the widely used ESD-based spherical approximation for continuity and comparability with previous studies.
Fender C K, Kelly T B, Guidi L, Ohman M D, Smith M C, Stukel M R. Investigating particle size-flux relationships and the biological pump across a range of plankton ecosystem states from coastal to oligotrophic. Frontiers in Marine Science, 2019, 6: 603.
Guidi L, Gorsky G, Claustre H, Miquel J C, Picheral M, Stemmann L. Distribution and fluxes of aggregates > 100 μm in the upper kilometer of the South-Eastern Pacific. Biogeosciences, 2008, 5: 1361-1372.
Wang X Y, Li H L, Zhang J J, Chen J F, Xie X H, Xie W, Yin K D, Zhang D S, Ruiz-Pino D, Kao S J. Seamounts generate efficient active transport loops to nourish the twilight ecosystem. Science advances, 2024, 10: eadk6833.

Lines 168/170: in ESD?
Response: Thank you for pointing this out. We confirm that both the upper (1.5 mm) and lower (100 μm) size limits used for particle flux calculations refer to the ESD. This has now been clarified in the revised manuscript.

Line 172: as explained before, the absence of classification between living and non-living particles is the main problem for me. Can you detail a little bit this choice? It would also be nice to have an idea of the percentage of living particles among the total amount of particles.

Response: Thank you for this thoughtful comment. As mentioned earlier, the UVP5.0 system used in this study only capture vignette images for particles with an ESD greater than approximately 500 μm. For smaller particles, only size and abundance are recorded, and no image data are available. As a result, the classification of particles into living and non-living categories is only possible for the larger particle fraction.

The main focus of our study is to investigate the abundance, size structure, and carbon flux of the overall particle field in relation to physical dynamics. While zooplankton dynamics are indeed interesting, they were not the primary research target at the outset. We initially intended to address the classification and distribution patterns of zooplankton in a separate manuscript. However, we appreciate your suggestion, and we have now included zooplankton data extracted from UVP vignette classification in the revised manuscript. Regarding your request for the proportion of living particles, we performed a calculation based on the imaged particles (>500 μm), and found that living particles (primarily zooplankton) accounted for approximately 31% ± 22% (mean ± SD) of the total particle count in this size range on average. Due to space limitations and the scope of the current study, we have chosen not to elaborate extensively on this aspect, but we fully agree it offers a valuable direction for future research.

Line 173: can you please give examples of small and large particles?

Response: Thank you for the suggestion. We acknowledge that particles smaller than 0.5 mm are generally below the UVP5 image resolution threshold and cannot be reliably identified from photographs. As a result, we cannot provide specific examples based on morphology. However, based on prior studies, these small particles likely include fine detritus, small fecal pellets, or other unresolved aggregates. Large particles included marine snow aggregates, sizable fecal pellets, zooplankton carcasses, and other biologically derived material. We followed the size-based classification scheme used by Kiko et al. (2017, 2022), in which particles with ESD < 530 μm are categorized as micrometric (small) and those with ESD ≥ 530 μm as macroscopic (large). We have added a clarification in the revised text.

Kiko R, Biastoch A, Brandt P, Cravatte S, Hauss H, Hummels R, Kriest I, Marin F, McDonnell A M P, Oschlies A, Picheral M, Schwarzkopf F U, Thurnherr A M, Stemmann L. Biological and physical influences on marine snowfall at the equator. Nat. Geosci., 2017, 10: 852-858.

Kiko R, Picheral M, Antoine D, et al. A global marine particle size distribution dataset obtained with the Underwater Vision Profiler 5. Earth Syst. Sci. Data, 2022, 14: 4315-4337.

Lines 174/175: please clarify or justify.

Response: Thank you for pointing this out. We have clarified the rationale behind this sentence. The size-based classification allow us to explore differences in vertical distribution and particle behavior, as larger particles are generally associated with aggregation and faster sinking, whereas smaller particles tend to remain suspended or result from disaggregation. We have revised the sentence accordingly.

Line 187: the formula is not readable, please modify.

Response: Thank you for your comment. To improve readability and avoid rendering issues across platforms, we have reformatted the equation as an image and inserted it into the manuscript.

Lines 188/189: the formula is not readable so my question is a little bit naive, but did you check that the m(d) and w(d) values you get were in the range of observed values for such particles? Or at least in the range of possible values in SCS?

Response: Thank you for pointing this out. We clarify that m(d) denotes the carbon mass per particle (in mg C), and that all variables in the flux equation are converted into consistent units during calculation, resulting in a final flux unit of mg C m^{-2} d^{-1}.

Line 197: are A and B coefficients adapted to the SCS? Do you have an idea on how it could impact your results?

Response: Thank you for this important question. The empirical coefficients A and B used in our flux calculations (A = 12.5, B =3.81) were derived from global ocean data by fitting UVP-derived particle size distributions to sediment trap-based carbon fluxes (Guidi et al., 2008). These coefficients have been widely applied in various regions, especially in other oligotrophic systems, such as the tropical Western Pacific and Mediterranean Sea (Ramondenc et al., 2016; Wang et al., 2004a, b). To maintain consistency with previous studies, we adopted the same set of empirical parameters. We acknowledge that regional variability in particle composition, density, and sinking behavior, may cause deviations from the global relationship. A discussion on this limitation has been included in the manuscript, and we will consider developing regionally tuned coefficients in future work as more direct flux measurements become available in the SCS.

Guidi L, Jackson G A, Stemmann L, et al. Relationship between particle size distribution and flux in the mesopelagic zone. Deep-Sea Research I, 2008, 55: 1364-1374.

Ramondenc S, Goutx M, Lombard F, Santinelli C, Stemmann L, Gorsky G, Guidi L. An initial carbon export assessment in the Mediterranean Sea based on drifting sediment traps and the Underwater Vision Profiler data sets. Deep-Sea Research I, 2016, 117: 107-119.

Wang X Y, Li H L, Zhang J J, et al. Seamounts generate efficient active transport loops to nourish the twilight ecosystem. Science Advances, 2024a, 10: eadk6833.

Wang Z Y, Fang C, Yang C H, Zhang G Y, Sun D. Latitudinal gradient and influencing factors of deep-sea particle export along the Kyushu-Palau Ridge in the Philippine Sea. Science of the Total Environment, 2024b, 906: 167460.

Lines 203/205: please give the unit for abundance, volume concentration and POC flux. Were depth profiles generated per station? Aggregated per transect/section?

Response: Thank you for your comment. We have now specified the units for particle abundance (particles L^{-1}), particle volume concentration (mm^3 L^{-1}), and POC flux (mg C m^{-2} d^{-1}). We have clarified that depth profiles were generated for each station, and additionally, section plots were constructed to show the along-transect distribution of particle abundance, volume concentration and POC fluxes.

Line 208: add references for Shapiro and Levene tests.

Response: Thank you for the suggestion. We have now added references for the tests in the Methods section.

Lines 209/210: I would avoid using PCA for Pearson correlation analysis, it is usually used for Principal Component Analysis and it can be confusing, especially if people do not read this specific sentence. Either change your acronym, or write 'pearson correlation analysis' each time.

Response: Thank you for pointing this out. We have removed the "PCA" and now refer to "Pearson correlation analysis" in full throughout the manuscript.

Line 222: a certain degree is quite vague.
Response: Thanks for the comment. We have revised the sentence to "Based on the distribution of sea surface salinity (SSS) (Fig. 1c, Supplementary Fig. S1f), the salinity at stations S9 and S10 along Transect 3 was lower than at other stations, indicating a certain degree of influence from the Pearl River plume.".

Fig. 2 and following: maybe try to indicate clearly in the figures where the cyclonic and anti-cyclonic stations were located.
Response: Thanks for the comment. In the revised figure2, we have now indicated the stations influenced by anticyclonic and cyclonic eddies using red and blue colors, respectively. This distinction has also been clearly explained in the updated figure captions.

Lines 232/244: maybe spend less time on nutrients that were not very used in the discussion.
Response: Thank you for the suggestion. We have revised this section to streamline the description of nutrient distributions.
Fig. 4: x-axis unit and name are missing.
Response: Thanks for the comment. We have added the unit and name for x-axis.
Line 266: again precise which deviation you showed.
Response: Thank you for the comment. We confirm that the value reported is the standard deviation (SD), and we have revised the sentence to clearly indicate this.
Line 276: is 'significantly' related to a t-test? If so, precise it along with the p-value?
Response: Thank you for pointing this out. Since no formal statistical test was conducted for this comparison, we have revised the sentence to avoid implying statistical significance.
Line 282: again precise which deviation you showed.
Response: Thanks for the comment. We have revised the sentence to clearly indicate "mean ± SD".

Lines 294/295: how did you choose the images, how they are representative? Moreover, why did you not show any images from the anticyclonic influenced stations? Were particles too small to be imaged? If so, precise it in material and methods.
Response: Thank you for the comment. The images included in the initial version were randomly selected and not intended to support any quantitative analysis. To avoid confusion, we have decided to remove them from the revised manuscript.
Line 295: again precise which deviation you showed.
Response: Thanks for the comment. We have revised the sentence to clearly indicate "mean ± SD".
Fig. 5 and Fig. 6: precise in the caption that the colourbars are different for each section.
Response: Thank you for the comment. Due to the large difference in value ranges among the particle variable (e.g., small particle proportions consistently >95%, large particle proportions <5%), unified colorbars would mask spatial patterns. We have now clarified in the figure caption that colorbars differ between panels and explained the reason.

Fig.7: among the 12 images you show, 4 are recognizable living particles. If this is representative of the images at S11 and S12, then it would mean that one third of the particles you used to compute POC flux are not directly sinking and thus not directly contributing to carbon export.

Response: Thanks for the comment. As we mentioned above, the images were randomly selected and not support any quantitative analysis. We have deleted it. For your worried about the POC flux estimation, we agree that the behavior and contribution of zooplankton to vertical carbon export can be complex and uncertain. As you pointed out, some living particles such as zooplankton may not be directly sinking, or their sinking behavior may be variable due to diel vertical migration or active swimming. However, as noted earlier, our POC flux estimates are based on the empirical approach proposed by Guidi et al. (2008), which integrates all detectable particles (living and non-living) in the size spectrum and was calibrated against sediment trap data across oceanic environments. This method inherently accounts for the mixed nature of the particle pool and the statistical relationship between total particle size distributions and observed fluxes. We acknowledge this as a potential source of uncertainty and have addressed it in the Discussion section of the revised manuscript.

Line 319: are you talking of the mean among stations or per 5-m depth bin? Please clarify.

Line 326: same. Are you talking of the mean among stations or per 5-m depth bin? Please clarify.

Response: Thank you for your comment. We confirm that the values were calculated by aggregating all 5-m binned data across stations, and we have clarified this in the revised sentence.

Line 327: again precise which deviation you are using.

Response: Thanks for the comment. We have revised the sentence to clearly indicate "mean ± SD".

Lines 328/329: the end of the sentence should be part of the discussion and it probably needs some references.

Response: Thanks for the comment. We have removed the interpretive part of the sentence from the Results section and will address these factors more appropriately in the Discussion.

Lines 332/333: same

Lines 339/340: same

Response: Thanks for the comment. We have deleted the contents not belonging here.

Fig. 8: why is there an empty space for what should be station S7 when it was not the case in the previous figures?

Response: Thank you for pointing this out. Station S7 was not included in this study due to a UVP instrument malfunction, and therefore no data are available at that location. We did attempt to modify the earlier UVP-related section plots to match the visual style of Fig. 8, with blank gaps at the missing station. However, the resulting figures were less clear and visually unappealing. For consistency and clarity, we ultimately chose to interpolate across the missing point using ODV settings, resulting in continuous transects.

Lines 349/350: the end of the sentence belongs to the discussion.

Response: Thanks for the comment. We have deleted the contents not belonging here.

Fig. 9: I think that this plot does not show the potential of your results. Could you find a better way to represent these data? Maybe a vertical profile, with 4 coloured paths (one per station)? What is the meaning of high/low? You do not precise it in the text or in the caption. Is there any significant difference between high and low?

Response: Thank you for your helpful comment. Following your suggestion, we revised Fig. 9 to display vertical profiles of POC flux at the four eddy-influenced stations. Each station is now represented by a separate colored line to clearly illustrate differences in the vertical attenuation of flux.

Lines 359/360: this sentence belongs to the discussion.

Response: Thanks for the comment. We have deleted the contents not belonging here.

Line 364: did you realise a statistical test? Does notably mean significantly?

Response: Thank you for your comment. Since no formal statistical tests were conducted for these comparisons, we have revised the text to avoid using the term "significantly" and instead describe the observed differences more cautiously.

Fig. 11: this figure is not clear, is it a bar plot? If so, the caption is wrong, if not, where are the box plots? If too many small values that skew the boxplots close to 0, maybe choose another type of plot that would be more suitable. The colours are unnecessary. What are the dots? (one dot = one station?)

Response: Thank you for your constructive feedback. We agree that the original figure was not sufficiently clear in distinguishing data representation. We have replaced the previous boxplot style figure with a more intuitive format—horizontal bar plots with error bars—representing the mean ± standard deviation of POC flux at each depth across all stations. We believe this updated figure more clearly conveys the vertical distribution patterns of POC flux for total, small, and large particles.

Line 387: again this name is confusing, usually PCA stands for Principal Component Analysis and not Pearson Correlation Analysis, especially if you write again correlation analysis afterwards.

Response: Thanks for the comment. We have changed PCA to Pearson correlation analysis here.

Lines 392/395: sentences belong to discussion.

Response: Thanks for the comment. We have deleted the contents not belonging here.

Lines 397/398: the end of the sentence belongs to the discussion.

Response: Thanks for the comment. We have deleted the contents not belonging here.

Line 400: discussion again.

Response: Thanks for the comment. We have deleted it.

Fig. 12: a) You might want to remove the diagonals. I don't understand why some dots don't have the correlation coefficient or the p-value (not precised) and some have it? I found it strange considering that the largest ones don't have it. Maybe correct the figure or explain it better in the caption. B-d) significant relationships?

Response: Thank you for your comment. We have removed the diagonals in a). In Fig. 12a, only non-significant correlations ($p \geq 0.05$) are labeled with Pearson r values. Significant correlations ($p < 0.05$) are visualized using colored circles without numerical labels, to emphasize their statistical relevance visually. This has now been clarified in the revised figure caption. For b-d, the p-value has been added. The regression at 200 m depth was statistically significant ($p = 0.0048$), while those at 400 m and 600 m were not ($p = 0.068$ and 0.116, respectively).

Fig. 13: the 0 of a) and b) are different, either you shift it a little bit everywhere or not at all but it is confusing that there is an offset in x-axis and y-axis for a)but only in y-axis for b).

Response: Thank you for pointing this out. As part of the broader revision of the manuscript, Fig. 13 has been removed in the updated version, as it is no longer necessary following the restructuring of the analysis and results.

Line 413: no data was available in the SCS? Even from other methods?

Response: Thank you for this question. To the best of our knowledge, there are currently no published datasets reporting in situ particle abundance or volume concentration in the water column of the South China Sea, especially based on imaging methods such as the Underwater Vision Profiler. Previous studies in this region (e.g., Liu et al., 2014; Zhang et al., 2018; Zhou et al., 2020) have primarily focused on particle fluxes using sediment traps or [234]Th-based approaches, which do not provide quantitative

information on particle abundance or size-resolved volume concentration in the water column. Therefore, we believe that our results represent one of the first efforts to characterize the distribution of suspended particles in the SCS using high-resolution optical methods.

Liu J G, Clift P D, Yan W, Chen Z, Chen H, Xiang R, Wang D X. Modern transport and deposition of settling particles in the northern South China Sea: Sediment trap evidence adjacent to Xisha Trough. Deep-Sea Research I, 2014, 93: 145-155.

Zhang J J, Li H J, Xuan J L, Wu Z Z, Yang Z, Wiesner M G, Chen J F. Enhancement of mesopelagic sinking particle fluxes due to upwelling, aerosol deposition, and monsoonal influences in the Northwestern South China Sea. Journal of Geophysical Research, 124(1): 99-112.

Zhou K B, Dai M H, Maiti K C, Chen W F, Chen J H, Hong Q Q, Ma Y F, Xiu P, Wang L, Xie Y Y. Impact of physical and biogeochemical forcing on particle export in the South China Sea. Progress in Oceanography, 2020, 187: 102403.

Line 417: as in the introduction, maybe try to find a more suitable reference.

Response: Thanks for the comment. We have changed it to a more suitable one "Buesseler et al., 2007".

Line 421: you indicated in the introduction and the results that the SCS is an oligotrophic sea, so why did you use mesotrophic results here? Also, I would have liked a reminder of the oligotrophic status of the SCS here.

Response: Thank you for pointing this out. We agree with your comment and have revised the sentence as following: When compared to UVP data from other oligotrophic oceanic regions, our findings fall within the broad range of particle concentrations and volume reported for similar low-nutrient systems (Table 2). This comparison also highlights the role of environmental variability in shaping particle distributions in the oligotrophic SCS.

Lines 424/427: the data you are comparing to were obtained in the first 100 m (see my comment on the Table 2 below). Also, is that a oligotrophic region?

Response: Thanks for the comment. We have added a line for 0-100 m layer data in Table 2, and we have revised the sentence to clarify the depth range of the SCS particle abundance data and ensure a consistent comparison with previous studies. The HNLC regions are typically considered oligotrophic due to strong iron limitation and low primary productivity. We have clarified it in the sentence. The revised sentence now reads:

The particle abundance in the SCS continental slope (0–100 m: 25–476 particles L^{-1}; 0–800 m: 0–783 particles L^{-1}) is comparable to values reported for oligotrophic regions such as the High Nutrient, Low Chlorophyll (HNLC) areas of the Southern Ocean(0–100 m: 0–500 particles L^{-1}; Jouandet et al., 2011), and markedly higher than those observed in the mesopelagic zone (200–1000 m) of the equatorial Pacific (1–4 particles L^{-1}; Pretty, 2019).

Lines 434/435: do you think it could be possible to get chl-a values from other papers to do the same figure as in Fig. 12 b-d) but at a larger scale? I think it would be easier to compare your results to other studies this way.

Response: Thank you for the insightful suggestion. We agree that compiling Chl a values from previous studies and reproducing a figure similar to Fig. 12 on a broader scale could help place our results in a wider context. However, after careful review of the relevant literature, we found that most published reports provide none Chl a concentrations or only provides over broad and inconsistent depth ranges. These inconsistencies in reporting formats and sampling depths make direct quantitative comparison

difficult. Nevertheless, we appreciate the value of this approach and agree it would be an excellent avenue for future synthesis work as more standardized and high-resolution datasets become available.

Line 436: highlight or maybe just suggest?

Response: Thanks for the comment. We have changed it to 'suggest'.

Table 2: maybe you could add one more line focusing on the 0-100 m to be comparable to Jouandet et al 2011 and Iversen et al 2010.

Response: Thanks for the suggestion. We have added one more line focusing on the 0-100 m in Table 2.

Lines 463/465: can you really affirm that just with Fig. 7, especially without showing images from other stations? I think that more analyses on classified images are necessary to justify this affirmation.

Response: Thank you for your comment. We agree with your assessment. The images shown in Fig. 7 were randomly selected and do not provide a statistically representative basis for that statement. To avoid overinterpretation, we have removed the sentence from the revised manuscript.

Lines 483/485: I agree, but a good way to check this hypothesis would be to plot the vertical distribution of LP, SP and living particles if you decide to classify your images. Or maybe try to find the distribution of zooplankton at the time of the sampling.

Response: Thank you for the helpful suggestion. Following your advice, we incorporated zooplankton data into the revised manuscript and presented their vertical distribution to better support our interpretation.

Line 486: might instead of reflect?

Response: Thanks for your suggestion. We have added 'might" before 'reflect'.

Line 495: I think this paragraph could have come earlier, even if the methods are not the same.

Response: Thank you for your suggestion. We have reorganized and revised the discussion section accordingly.

Lines 503/506: I am not really convinced by the comparisons you are doing with various ocean locations with dynamics that potentially very different from the ones you have in the SCS.

Response: Thanks for the comment. We have deleted this sentence to avoid confusion.

Lines 517/518: justify with a reference please.

Response: Thanks for the comment. We have added the reference 'Cram et al., 2018' here.

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

Response: Thanks for pointing out this. We have revised it to "Steinberg et al. (2023) found that large zooplankton fecal pellets were……".

Paragraph starting at line 530: I like this paragraph

Response: Thanks.

Lines 546 and followings: I think it would be interesting to link your results to front-enhanced zooplankton dynamics, such as Mangolte et al. 2022 (https://doi.org/10.1093/plankt/fbac010) for example. A lot of papers were published on this subject.

Response: Thank you for the insightful suggestion. Following your recommendation, we have integrated zooplankton data into the revised manuscript and included a preliminary analysis of their distribution in relation to eddy structures. We quite agree that linking zooplankton dynamics to physical features is a

very interesting direction. We plan to conduct a more in-depth analysis using our existing dataset in a future study. We also appreciate the reference to Mangolte et al. (2022), which we have reviewed and cited in the revised manuscript.

Line 569: are zooplankton included in the 'production of organic particles'?
Response: Thanks for the comment. Yes. Our zooplankton data show clearly elevated abundances in the cyclonic eddy region, supporting their role in enhancing the particle field. This has now been clarified in the revised manuscript.

Lines 588 and followings: I like the fact that you included a Data uncertainties paragraph, but the uncertainties you cited might be a little bit too obvious to have a specific paragraph, maybe elaborate.
Response: Thanks for the comment. We have elaborated it in the revised manuscript.

Line 604: "biological production" might be too large considering that zooplankton is not at all included in your study, I would replace it by chlorophyll-a concentration, or anything else focusing only on phytoplankton.
    Response: Thank you for your comment. In the revised version of the manuscript, we have incorporated zooplankton data derived from UVP vignette classification, and observed elevated zooplankton abundance in eddy-influenced regions. Given that both phytoplankton (as inferred from chlorophyll-a) and zooplankton are now considered in our analysis, we believe that the term "biological production" is more justified in this context.
    Once again, thank you very much for your valuable comments. We sincerely hope that our responses and revisions meet your expectations.
    Best regards,
Shujin Guo