We would like to thank the reviewer for the thorough and constructive comments. These suggestions have been carefully considered and have helped us to improve the manuscript. Below, we provide a detailed response to each comment.

Main concern

The clarity and impact of the findings are hindered by a presentation that is at times diffuse and by the inclusion of numerous analyses whose relevance is not always made explicit.

The manuscript would benefit from a more focused and structured approach, with a clear emphasis on its main scientific question: the evaluation of zooplankton community responses to a frontal system (and the conclusion that the community is not just a mix of those of the surrounding water masses).

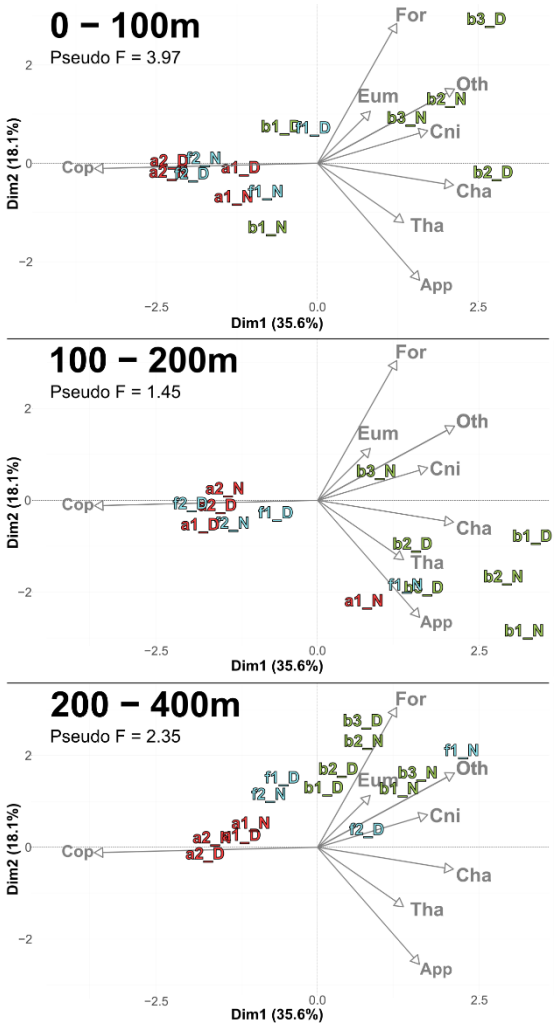
Its length could probably easily be reduced by ⅓, possibly ½.

The inclusion of data from the "M" stations introduces complexity that does not clearly support the core narrative. These stations appear to add variability that confounds rather than clarifies the analysis of the frontal signal. In particular, the inclusion of M stations significantly alters multivariate patterns such as those visible in the PCAs in Figure 5 and following, making it more difficult to discern the contrasts between the A, B, and F stations, which are those associated with the frontal gradient. I would strongly recommend excluding the M stations entirely from the analyses and focusing the manuscript on the transects most relevant to the front.

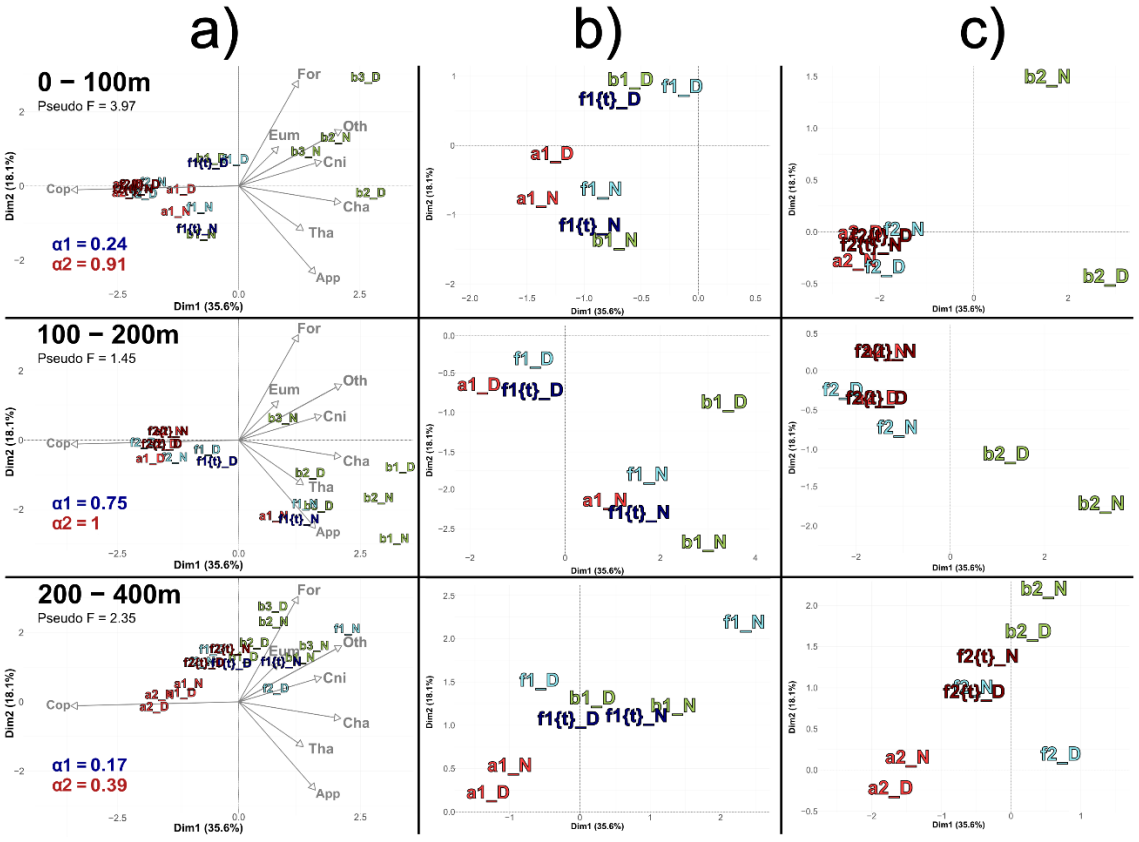
We agree with the reviewer that including the M stations in certain analyses (mainly PCA) can obscure the frontal signal. However, the results from the M stations, relatively to the other stations, are useful for other outcomes of the BioSWOT-Med campaign (e.g., Zooglider, fluxes). Consequently, we propose to retain the results from the M stations for concentration and taxonomic distribution (Figures 2, 4, and 6) in the main manuscript. To follow reviewer comment, the M stations will not be considered in the analyses presented in Section 3.4 of the main manuscript (PCAs in Figures 5, 7, and 9) focused on the frontal signal.

Moreover, we propose to present the positions of the M stations in the supplementary materials (PCA, etc.), treating these points as supplementary individuals.

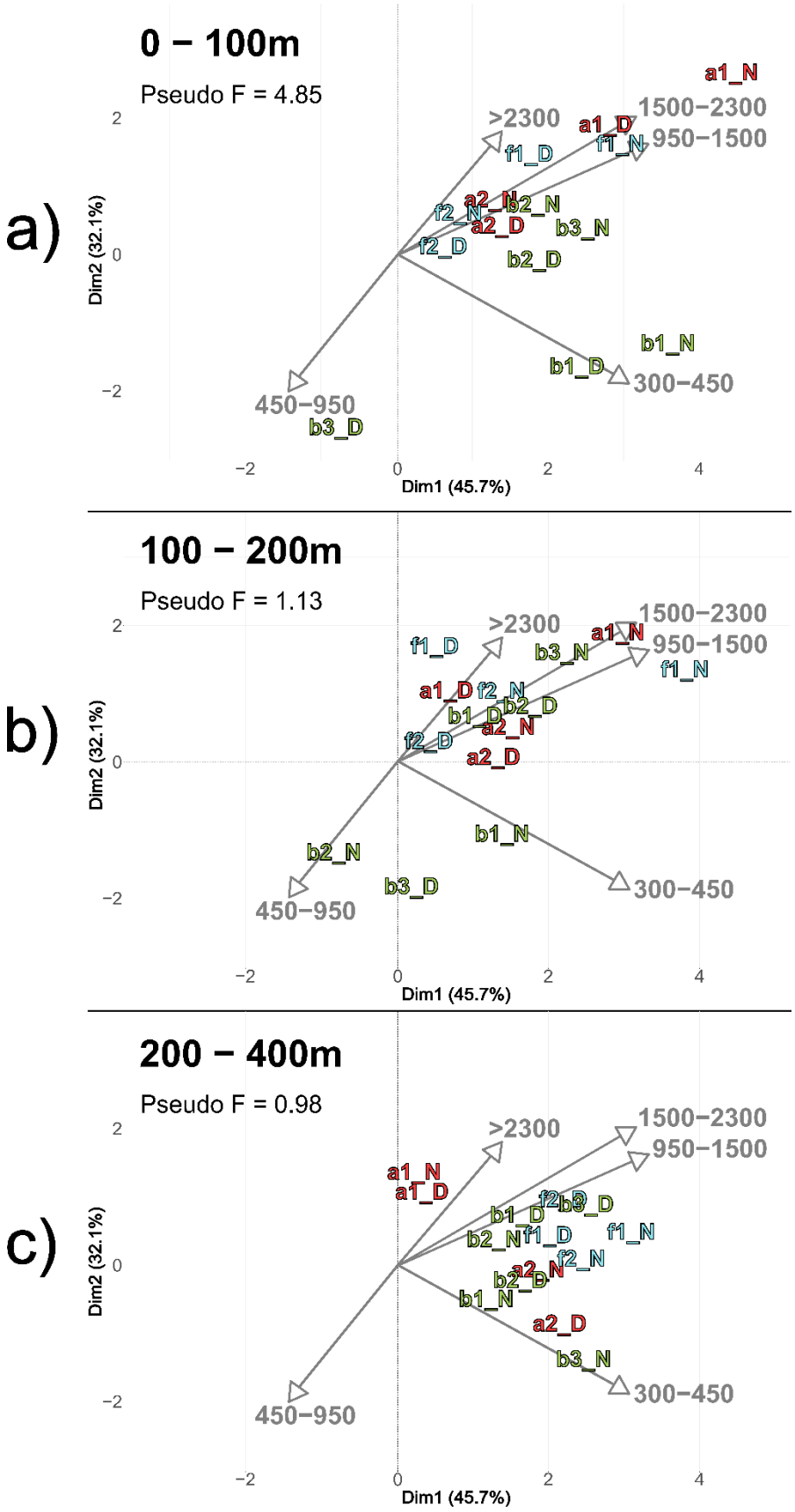
See PCAs below:



Same PCA as Figure 5 in the first version of the manuscript, but without the M stations.

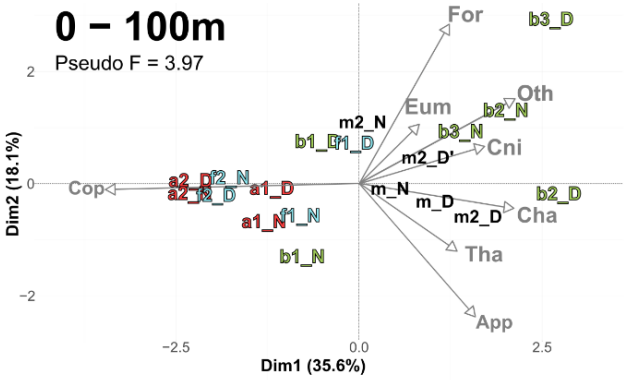


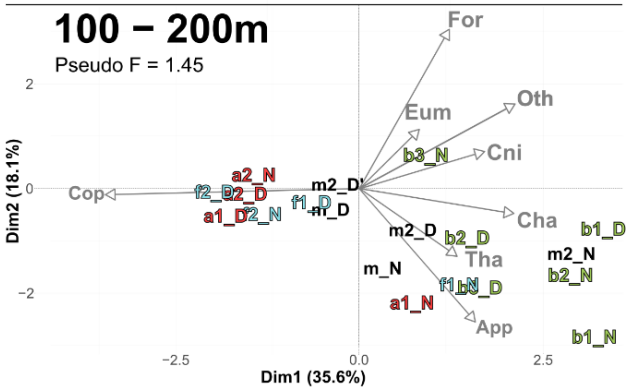
Similarly, we have the same PCA as Figure 7 in the first version of the manuscript, but without the M stations.

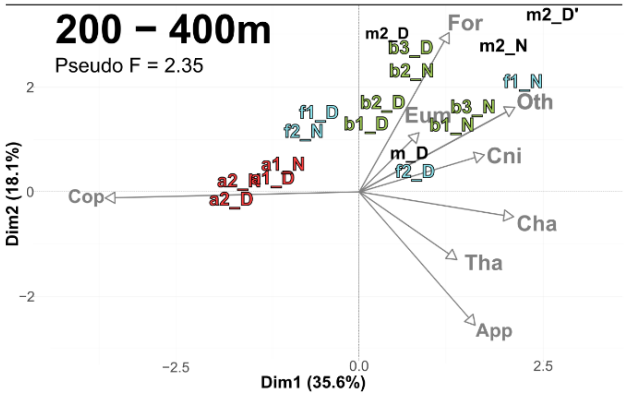


Same PCA as Figure 9 in the first version of the manuscript, but without the M stations.

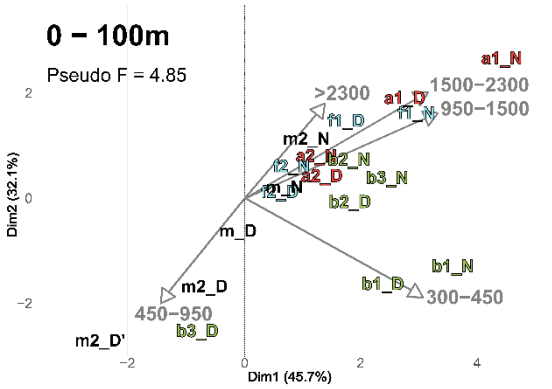
For the Supplementary Materials, the two previous PCAs are shown again, but with the M stations projected as supplementary individuals:



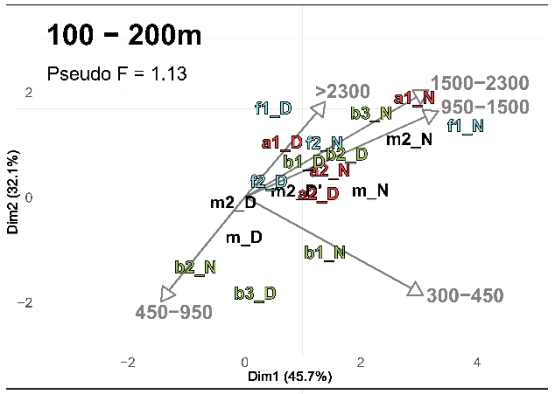




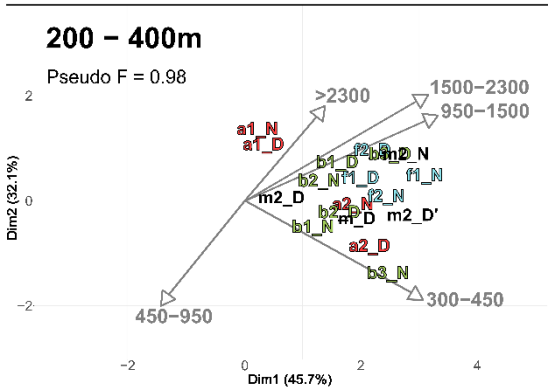
a)



b)



c)



Moreover, while the breadth of analyses presented is commendable, their number may overwhelm the reader and dilute the central message. A clearer selection of the most pertinent analyses to highlight your main message (e.g., PCA on concentration or biovolume, barplots, NBSS) would help maintain the reader's focus.

While PCA on concentration or biovolume alone does not yield meaningful projections, we have revised the manuscript to reduce the number of analyses (removed Fig. 2b, 2c) and retain only the most pertinent ones, ensuring a clearer focus on the main message.

Other analyses—such as those related to diel vertical migration or the storm—could be treated as potential confounding factors. These could then be addressed more briefly in the discussion with some plots presented in supplementary materials if necessary. In addition, the manuscript attributes several observed differences between cruise legs to the passage of a storm. However, the current dataset does not make it possible to unambiguously separate storm effects from general temporal or spatial variability. This uncertainty should be acknowledged more explicitly in the discussion, and interpretations emphasising the storm as a dominant factor should be tempered accordingly.

The Discussion has been revised following these general comments and the more specific suggestions detailed below.

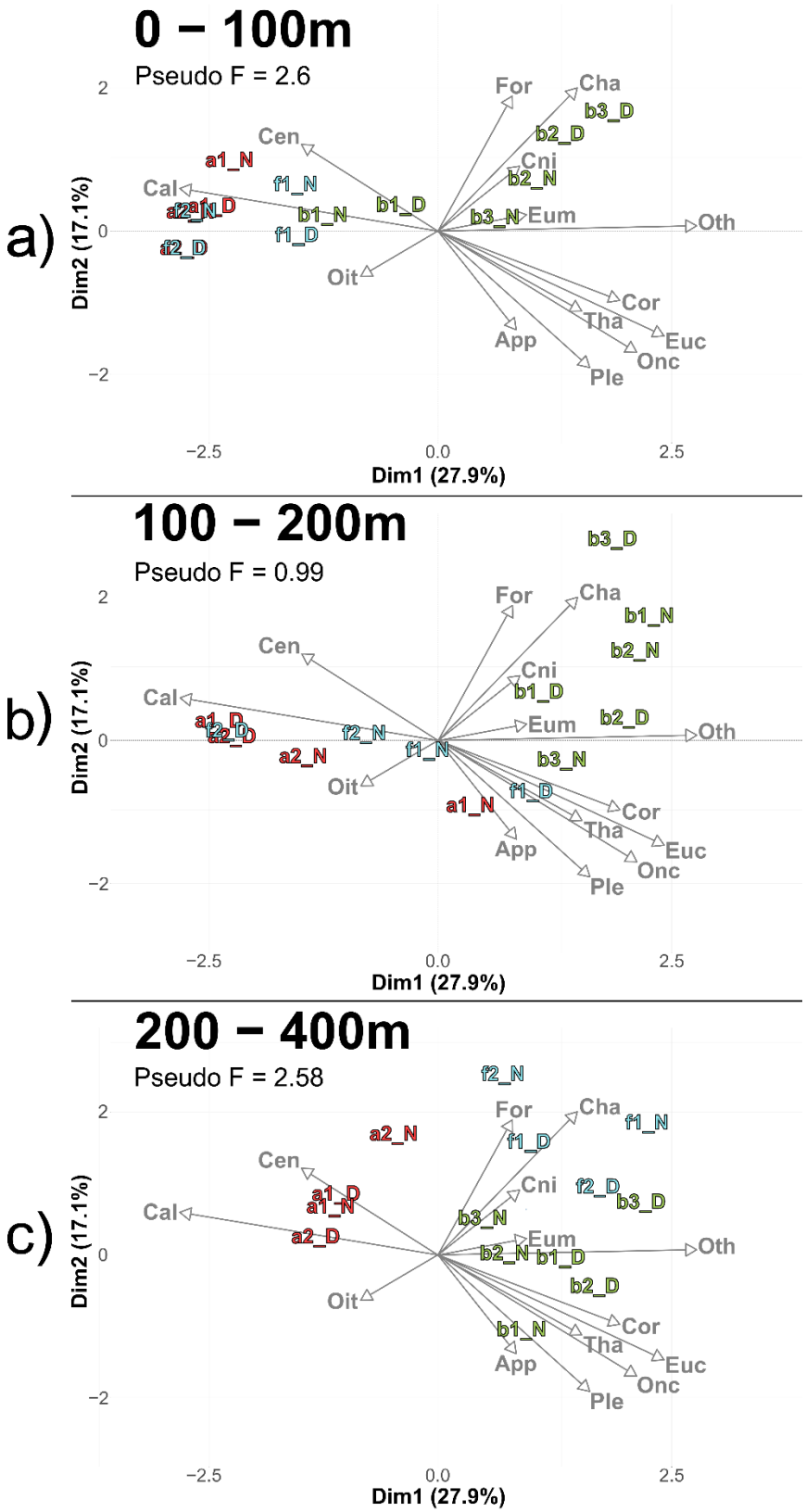
Major comments

Line 113: The authors limit the analysis to eight categories derived from the ZooScan. It is unclear why only these eight were used, given that the system allows for much finer taxonomic resolution. These categories were likely aggregates of finer taxonomic groups. Why didn't you use the finer level data? Indeed, as acknowledged in lines 423–425, the inclusion of rarer taxa enhanced the ability to distinguish between water masses, indicating that finer-scale groupings may be more informative for detecting ecological responses to fronts. Coarse groupings such as "copepods" may be too ubiquitous to reveal significant patterns.

We used eight broad categories because Ecotaxa assigns organisms at varying taxonomic levels (species, family, order), and this grouping ensures consistency.

Moreover, the semi-automatic taxon recognition process was performed on Zooscan images with a pixel size of 10.3 µm. Consequently, some taxa could be identified to the species level, while others could only be determined at the genus, family, or order level. Some taxa are either too small or could not be precisely recognized by Ecotaxa for other reasons (e.g., sample quality, image quality during Zooscan scanning) and therefore were not identified to species. Instead, they were grouped at the finest taxonomic level that Ecotaxa could assign, which in some cases is only the order. For example, 65 % of the total copepods were classified as “Calanoida undetermined” for these reasons.

Following your comments, we performed PCAs (see below) subdividing the copepods into the seven categories defined by EcoTaxa for the most abundant copepods (seven categories because only those with >1% of the total copepod abundance were retained): Undetermined Calanoida, *Centropages* spp., *Corycaeidae*, *Euchaeta*, *Oithona*, *Oncaeidae*, and *Pleuromamma* spp.



PCA with subdividing the copepods into seven categories (Ecotaxa): Undetermined Calanoida, *Centropages* spp., *Corycaeidae*, *Euchaeta*, *Oithona*, *Oncaeidae*, and *Pleuromamma* spp.

Lines ~145: The vertical distribution estimation based on two net tows at different depths, with subtraction, is methodologically weak compared to dedicated stratified sampling using e.g. a multinet. Variability in the upper layer between tows could significantly affect results. Ideally, replicate shallow tows should be presented to estimate intra-station variability and compare it with the inter-station variance. If this is not possible, a discussion of this methodological limitation should be included, supported where possible by literature.

We acknowledge that patchiness certainly leads to significant variations in concentrations between hauls, as the three net hauls were carried out within two hours. This patchiness is visible in Figure 2, where concentrations do not always decrease consistently with depth. In particular, concentrations in the 100–200 m layer may be misestimated at certain stations. To provide context, we can compare our data with reference values from Di Carlo (1984), which report concentrations of approximately 57% in 0–100 m, 27% in 100–200 m, and 16% in 200–400 m with respect to the 0-400 m layer.

In our study, the observed mean concentrations were 46.2 ± 18.2% in the 0–100 m layer, 26.9 ± 18.5% in the 100–200 m layer, and 26.8 ± 15.5% in the 200–400 m layer with respect to the 0-400 m layer.

These numbers show that, on average, the concentrations follow a reasonable pattern but with significant departures to the average situation.

As Di Carlo (1984) did not differentiate between day and night sampling and used different net mesh sizes, this comparison is only an indication.

We add in discussion that concentrations in the 0–100 m layer are accurate, while uncertainties remain for the two deeper layers, which cannot be fully resolved.

Line 173: The use of the Hellinger transformation shifts the analytical focus to relative composition rather than absolute concentrations. This is not inherently "preferable" to the use of Euclidean distance on raw data, but represents a different analytical approach. This choice should be justified explicitly, as it influences the interpretation of the results.

The Hellinger transformation was used to focus on relative abundances, which allows all data to be analyzed together. Using absolute abundances would mainly discriminate between the first and second transects and would not reveal a stable gradient between water masses. This choice is now explicitly justified in the manuscript.

Lines ~215: Since the fT stations are constructed as linear combinations of stations A and B, their positions in PCA space should likewise fall between the positions of those stations. Due to the Hellinger transformation, this may not be a straight line but a curved path. Still, the optimal mixing between a and b should be computable exactly (i.e. without iteration) from the PCA space. In any case, recalculating the PCA with each added station alters the structure of the PCA space (even if this is likely small here) and this hinders comparisons. It would be more appropriate to construct a PCA using A, B, and F stations, and then project the fT stations as supplementary points.

Thank you for this remark and sorry for our unclear explanation. Our method does exactly what you mentioned: fT stations are only projected as supplementary points on the initial PCA axes. They do not influence the axes or the positions of the actual stations. No PCA recalculation is performed; the loop is used only to find the minimum of the cumulative f–fT distances. This procedure is now explained more clearly in the manuscript.

Section 3.3: While this section demonstrates the presence of diel vertical migration, it does not quantify its importance relative to other sources of variability. A multivariate analysis such as PCA including all stations could help illustrate whether day and night samples from the same station are more similar to one another than to samples from other stations (and you have it… so cite it here).

In the global PCA (Figure 5), differences between water masses dominate axis 1, while diel vertical migration (DVM) is mainly represented on axis 2. This shows that DVM is a secondary source of variation compared to spatial differences between stations. Day and night samples from the same station (x1-D and x1-N) are generally close, and there is no grouping of day or night samples across different stations.

We also explored a PCA based on biovolume or abundance alone, as previously suggested, but this did not yield meaningful results, since the projections were too strongly clustered. Overall, the global PCA allows us to better quantify the variation induced by DVM, confirming that it is not the primary structuring factor.

Section 3.4.3: This section appears to replicate earlier PCAs using trophic groups rather than taxonomic ones. It is not clear what new insight this re-analysis is intended to provide. If there is a hypothesis suggesting that trophic groups respond differently to frontal structures, it should be clearly stated. Otherwise, the patterns described may simply reflect underlying taxonomic distributions.

This concern was also raised by the other reviewer. As a result, the trophic group analyses, including Section 3.4.3 and Figure 8, have been removed from the manuscript.

Lines 479 and 484: The discussion mentions the value of high-resolution observations and autonomous platforms. If I am not mistaken, a Zooglider was deployed during the BioSWOT campaign. It would be extremely valuable to discuss these data alongside the net samples. The integration of these two datasets could significantly enhance the interpretation of the observed patterns compared to studying them in isolation. I was actually expecting to read about this when I accepted the review and was disappointed to see only the net data.

Unfortunately, due to logistic contrains in France, Zooglider was deployed south of Majorca by spanish colleagues, and thus could not sample the front as expected. Most of the glider transects are south of Majorca and Menorca, except some transects near station B3. Therefore, M stations and B3 will be useful for comparison with these data.

The Zooglider data will be analysed in a separate study.

Detailed comments

- The manuscript contains inconsistencies in citation formatting. References should follow the format (Author Year) rather than (Author (Year)).

  All references will be formatted according to the (Author Year) style.

- **Line 36**: The assertion that fronts concentrate plankton should be moderated, as this is not always the case. Indeed, you already provide a more nuanced statement just a few lines above.

  The statement has been moderated accordingly.

- **Figure 1**: It would be helpful to include a contextual map showing general ocean circulation as introduced in the background. The ship's trajectory should also be overlaid. Each subpanel should show only the relevant stations for its respective transect.

  A map illustrating general ocean circulation has been prepared (see below). Overlaying the ship's trajectory on Figure 1 would make it too busy, but it is available in the campaign report: https://doi.org/10.13155/100060 (Figure 7, page 13).

  See below the map showing general ocean circulation of the NWMS:

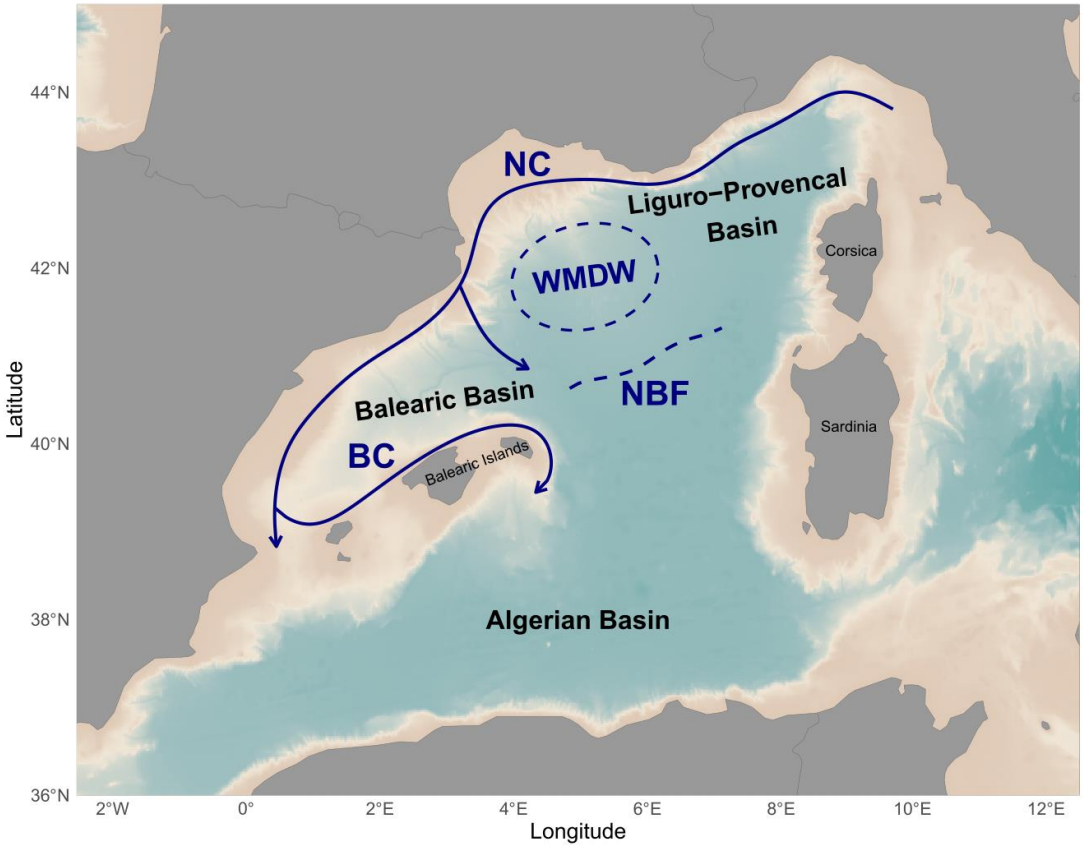


Figure 1. Maps of the NWMS showing the major oceanographical currents and front (NC: Northern Current, BC: Balearic Current, NBF: North Balearic Front, WMDW: Western Mediterranean Deep Water formation area) of the northern part of the NWMS. After Millot (1987), López García (1994) and Pinardi and Masetti (2000).

- **Line ~80**: A clearer description of the software tools used would be beneficial. Their specific functions and utility during the cruise should be outlined.

  All SPASSO software used is described in detail in Rousselet et al. (2025).

- **Lines 86–88**: This material reiterates content already provided in the introduction. It would be advisable to consolidate this information in one section and refer to Figure 1 directly from the introduction.

  The information regarding the description of the hydrography of the area has been greatly simplified in the Introduction section and now forms part of a new subsection of the Materials and Methods, *Study area*. The repetition about zones AB and F at line 86 has now been incorporated into this subsection.

- **Line 89**: If the station drift over 24 hours is not negligible relative to the map scale, this should be depicted as a trajectory rather than as discrete points.

  There was a misunderstanding: the map already shows the drift between day and night sampling locations for each station. Only station F2 experienced significant drift.

- **Line 90**: Two "f2" stations are presented: f2_D and f2_N… but the day/night distinction has not been made explicit yet.

  The day/night distinction for F2_D and F2_N is made explicit in Table 1, cited just before discussing these stations.

- **Lines 95–96**: The notation for water masses and stations should be presented earlier to guide the reader from the outset.

  Notation for water masses and stations are already presented earlier in the first paragraph of the Materials and Methods and are shown in Figure 1

- **Table 1**: The column listing station names does not provide essential information and could be removed.

  The station name column has been removed (see below)

Campaign Stage	Station Name	Water Mass	Date - Time	Latitude	Longitude	Depth (m)
1st transect	a1_D	A	25/04 - 12:38	41.240	4.553	>2500
	a1_N	A	26/04 - 00:02	41.224	4.563	>2500
	f1_D	Front	26/04 - 12:11	41.099	4.423	>2500
	f1_N	Front	27/04 - 00:32	41.102	4.456	>2500
	b1_N	B	28/04 - 00:17	40.874	4.388	>2500
	b1_D	B	28/04 - 12:28	40.884	4.389	>2500
Storm	m_N	M	02/05 - 00:37	39.555	4.101	1350
	m_D	M	02/05 - 12:22	39.493	4.087	1500
2nd transect	b2_D	B	04/05 - 12:16	40.795	4.933	>2500
	b2_N	B	05/05 - 00:13	40.849	4.936	>2500
	f2_D	Front	05/05 - 11:49	41.175	5.108	>2500
	f2_N	Front	06/05 - 00:45	41.134	5.308	>2500
	a2_N	A	07/05 - 00:13	41.412	5.24	>2500
	a2_D	A	07/05 - 12:15	41.376	5.253	>2500
Return water mass M	m2_D	M	10/05 - 11:31	39.671	3.957	1150
	m2_N	M	11/05 - 00:31	39.629	3.902	1200
	m2_D'	M	11/05 - 11:53	39.603	3.885	1300
Return water mass B	b3_D	B	12/05 - 12:15	40.782	5.152	>2500
	b3_N	B	12/05 - 23:58	40.746	5.112	>2500

- **Line 115**: The term "abundance" is used, but the actual metric is concentration (ind/m³). This should be corrected throughout the manuscript.

  All instances of “abundance” have been replaced with “concentration” throughout the manuscript.

- **Line 152**: It is not clear what the "groups" are at this point. Overall, in the methods section, I would advise to always start by explaining the ecological purpose of the analyses and only then, describe (which tests, which hypotheses, etc.) you will carry them out. Currently, the rationale of the analyses is often not clear.

  The reference to trophic groups has been removed, along with the associated figure, as the trophic analyses have been deleted.

- **Line 152**: Normality should be assessed on residuals rather than raw data. If normality assumptions were not met, alternative methods should be justified. Indicate whether data transformations were applied to reach normality (it's likely that a transformation was used).

  Normality was assessed on residuals; this is now clarified in the manuscript. And normality of residuals was confirmed without the need for any additional data transformation.

- **Line 155**: The aim of the copepod subgroup test should be more explicitly explained.

  The aim of the copepod subgroup test has been clarified in the manuscript; it was conducted to investigate diel vertical migration (DVM) patterns within the copepod community.

- **Line 163**: The x-axis of the NBSS should be in units of mm³.

  We follow the approach of Platt and Denman (1977), as in de Souza et al. (2020), for the NBSS axes; therefore, units are consistent with these references.

- **Line 165**: The y-axis should be expressed in $mm^3/m^3/mm^3$; the denominator represents Δvolume in mm^3.

  The y-axis unit was a typographical error: it was written as "Δvolume.mm^{-3})" but should be "Δvolume (mm^3)"; the values are correctly expressed in m^{-3}.

- **Line 167**: If the ellipsoid volume approximation is deemed superior, as you state at lines 135-136, explain why the spherical approximation is still used in this section.

  The spherical approximation is retained in this section because it is used only to compute the size spectrum from ECD. The ECD is only used for combining the two mesh sizes (200 and 500 µm). While the ellipsoid approximation may be more accurate, each biovolume can correspond to multiple combinations of length and width, so the spherical approximation provides a consistent, comparable metric.

- **Line ~170**: Again, the manuscript should provide the rationale ("why") for each analysis before presenting the methodology ("how").

  The rationale for each analysis is now clarified before the methods

- **Line 172**: Clarify that observed noise in the NBSS at large sizes is due to the rarity of large individuals rather than size per se.

  This point has been clarified.

- **Line 178**: I would suggest replacing the shorthand notation (y1+) with an explicit sum sign. Also, indicate that concentrations—not frequencies—are being summed, and define all variables (y_ij are not explicitly defined).

  This has been clarified: concentrations are indicated, and all variables are defined.

- **Line 181**: Please explain what is meant by "asymmetric" in this context.

  The term "asymmetric" has been clarified:

  If two sites both have zero abundance for a species (a double zero), that absence does not contribute to making them more similar. In contrast, with Euclidean distance, double zeros do contribute, which can artificially inflate similarity. Thus, the treatment of presences and absences is not symmetric: presences matter, joint absences don't.

- **Line 185**: Specify whether normality tests were performed before or after the Hellinger transformation. Note that PCA does not absolutely require normal data but is appropriate only with approximately normal input, so an actual normality test may be excessive. Also, please clarify the purpose of testing correlations between variables (since this seems to me that assessing correlations is what the PCA does already).

  Before performing PCA, the Hellinger-transformed data were checked for normality using the Shapiro-Wilk test., and correlations between taxonomic groups were examined to ensure sufficient linear structure for PCA.

- **Line 196**: Provide details on how "dispersion" was calculated.

  Details on how "dispersion" was calculated have been added:

  "Dispersion was calculated as the sum of squared Euclidean distances of individuals to their group centroid (intra-group dispersion). Inter-group dispersion was defined as the sum of

squared distances between group centroids and the global centroid, weighted by group size. These measures were used to compute the pseudo-F statistic."

- **Line 199**: Was the significance of the pseudo-F statistic tested? If so, specify the method.

  We did not test the significance of the pseudo-F statistic. It was used here only as a descriptive measure, to give an idea of how different (or not) the water masses are from each other, rather than as a formal test

- **Line 202**: The notation "fT" is potentially ambiguous. A clearer notation such as f{t}_D, where {t} is a subscript and indicates theoretical interpolation, would help avoid confusion.

  The notation “fT” has been clarified and is now written as f{t}_D

- **Figure 2**: Indicate in the axis title that (b) refers to the biovolume of small organisms.

  The figures 2.b and 2.c have been deleted, as explained above.

- **Figures 3 and 4**: Consider using a more refined and perceptually balanced colour palette, such as those offered by Tableau or ColorBrewer.

  We have revised Figures 3 and 4 (see below) by applying the ColorBrewer 'Set2' palette to improve perceptual balance and readability:

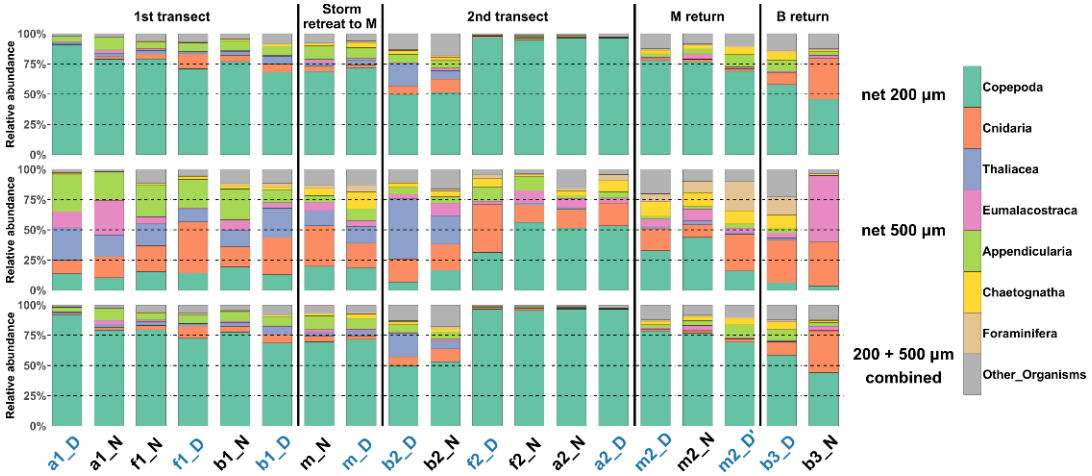


Figure 3

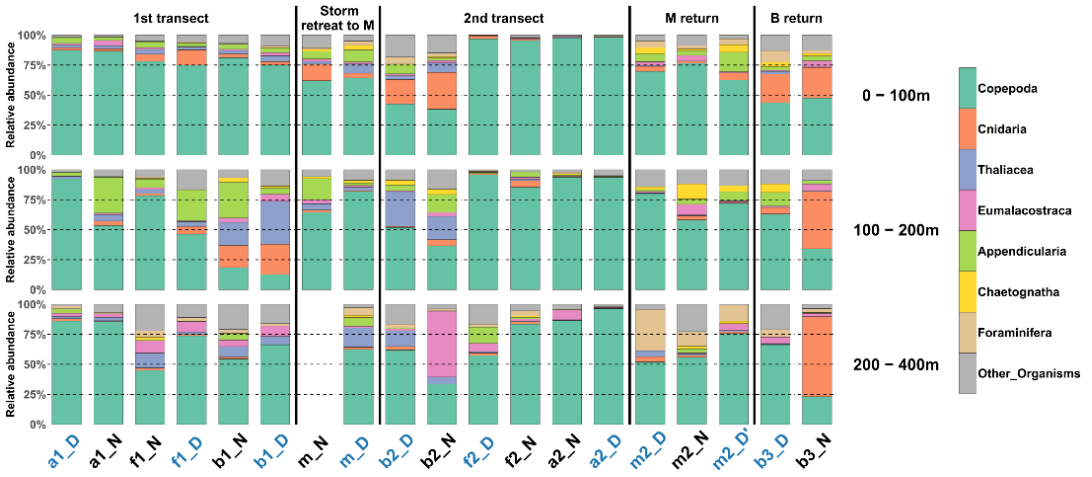


Figure 4

- **Line 252**: Why is biovolume analysed here but not in the previous section? Indeed, biovolume provides a valid view of the taxonomic composition. I am not asking for an additional analysis (there are many already); rather I would recommend choosing an angle of analysis, justifying it and sticking by it.

  We have removed the biovolume analysis, including Figures 2b and 2c, because biovolume is not suitable for large organisms (e.g., salps, cnidarians, and eumalacostracans) due to high variability and sampling limitations.

- Also, the claimed similarity between different depth layers should be demonstrated using multivariate analyses (e.g., PCA based on Hellinger distances), which would better capture the structure of the data.

  We note the suggestion, but additional multivariate analyses were not performed, as we consider them unnecessary for the current focus of the study.

- **Line 283**: The PERMANOVA test should be described in the methods section. Clarify which factors were tested.

  PERMANOVA and tested factors are now described in Methods

- **Lines 293–294**: "This indicates...dynamics of the water masses": I am not sure I understand what you mean. It is unclear why variation in the proportion of group A is interpreted as evidence for vertical migration. Could this not be attributed to bathymetric differences between the regions of A and B water masses for example?

  This statement has been clarified and nuanced; but bathymetric differences between A, B, and F water masses do not explain the observed patterns.

- **Line 299**: The reconstructions of fT station values are assessed on relative concentrations only, within the PCA framework (since the Hellinger transformation was performed). You should not state that you reconstruct "absolute" concentrations.

  The text has been clarified

- **Lines 300 and 303**: Specify what the relative increases or decreases are in reference to.

  Noted and addressed

- **Line 312**: Avoid abbreviations such as Cop_CCF or Cni unless defined. Using full names would not be much longer but would be clearer.

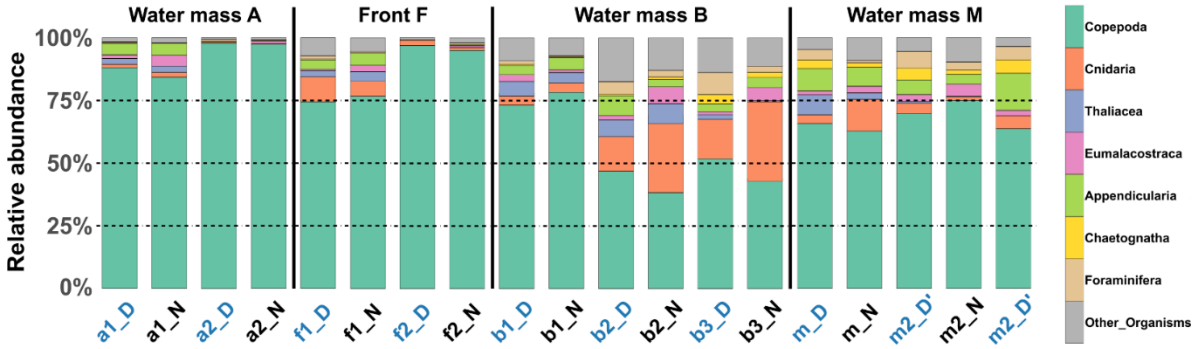
  3.4.3 has been removed as explained above

- **Line 314**: Specify what is meant by "non-carnivorous" (e.g., non-carnivorous copepods?).

  3.4.3 has been removed as explained above

- **Figure 6**: If there are only three samples per station (corresponding to depth layers), boxplots may not be appropriate (a boxplot summarises a data distribution with 5 values; if you have only 3, this does not make sense). Consider an alternative method of data representation if that is indeed the case.

  Figure 6 has been replaced by a new version (see below). The boxplot in the first version used data from both nets (distinguishing 200 µm and 500 µm mesh, as in Figure 3). Since all results are now presented for the merged nets, the figure was replaced by one averaging the three depth layers and ordering stations chronologically, consistent with Figure 4. The previous figure was clearly not valid based on the merged data, and we thank the reviewer for pointing this out.



- **Figure 9**: Much of the description of these results are in terms of shifts between depth layers or between regions, but these are difficult to see since the depth layers are in different subplots. A single PCA plot with region and depth encoded by colour or symbol would facilitate interpretation.

  We agree that this could be a useful approach. However, encoding both region and depth in a single PCA plot would result in a cluttered and unreadable figure, particularly in the center of the PCA, and is therefore not feasible.

- **Line 337**: Rather than referencing previous literature, the nutrient-rich nature of water mass A should be demonstrated using nutrients data collected during the cruise, if possible (I image that basic oceanography variables were collected).

  Here is the revised first paragraph of 4.1 below.

  The spatial differences between water mass A and B in late spring can be linked to the regional hydrological and ecosystem functioning of the NWMS in the post-bloom period (D'Ortenzio and Ribera d'Alcalà (2009)). Water mass A has its origin in the Liguro-Provencal area (NWMS), characterized by intense convection and mixing (Barral et al. (2021)), high nutrient concentrations (Severin et al. (2017)) and more productivity (Mayot et al. (2017); Hunt et al. (2017)) with the formation of a deep chlorophyll maximum around 50 m (Fig. S3; Lavigne et al. (2015); Doglioli et al. (2024)). Water mass B in the southern part of the NBF comes from the epipelagic waters of the Algerian basin, which are warmer and fresher than waters from the NWMS, with virtually permanent stratification and a DCM deeper than 50 m (Fig. S3; Lavigne et al. (2015)).
  In the transitional post-bloom period (April-May) encountered during the BioSWOT-Med cruise, water mass A was nutrient-richer than water mass B with mean nitrate (phosphate) concentrations in the euphotic layer ranging 0.64-1.27 (0.003-0.144) µM in A compared to 0.04-0.44 (below detection limit-0.003) µM at B. Those contrasts also appeared at 500 m depth, nitrate (phosphate) concentrations ranging 8.38-9.43 (0.34-0.40) µM in A compared to 7.49-8.89 (0.26-0.36) µM in B (Joël et al., 2025, submitted). Water mass A shows higher zooplankton stocks strongly dominated by copepods and larger forms whereas in water mass B, and community structure is dominated by small sizes and slightly more diversified within the non-copepod organisms (Fig. 4, 9), consistent with Fernández de Puelles et al. (2004).

- **Line 369**: If prior studies in the NWMS do not address the NBF specifically, this literature review may be condensed.

  Noted, the review has been condensed accordingly.

- **Lines 370–379**: These results from the literature are not clearly linked to your findings. Consider moving this paragraph later, where the discussion is more integrative.

  This revision has been made.

- **Line 395**: Fronts may actually have their strongest effect when nutrients are limiting, such as during the normally post-bloom period of the year, when the cruise occurred. Indeed, they can then enhance nutrient availability and prolong productivity later in the season

  Added

- **Line 408**: Clarify what is meant by "higher taxa."

  We clarified that 'higher taxa' refers here to the broader taxonomic categories we defined earlier (Table 2)

- **Line 411**: "highlighting the importance of considering individual taxonomy groups rather than just overall abundance patterns when analysing community dynamics": this claim that taxonomy matters for community analysis is self-evident: community dynamics is the dynamics of various species, so, of course, it cannot be assessed with only the overall concentration. Consider removing or rephrasing.

  You are right indeed, it's tautological. This sentence has been removed

- **Line 429**: The observation that zooplankton differences are stronger at 100–200 m despite the fact that the front is stronger in 0-100m is intriguing and warrants further discussion.

  We already mention in the manuscript that the 100–200 m layer likely acts as a transitional zone in the context of DVM, which explains the stronger differences observed there despite the surface front being more pronounced.

- **Lines 431–432**: "emphasises the stronger influence of hydrology and biological productivity at the surface": and of the front! The fact that the two water masses that meet at the front have a different history is also a good explanation for this observation.

  Yes, indeed, this is now clarified in the manuscript

- **Line ~440**: Diel vertical migration should be introduced early in the results as a potential confounding factor. Explain how this was controlled for/avoided (e.g., comparing only daytime samples) so that you can safely go on with the analyses despite this confounding factor.

  Yes, indeed, the analyses concerning DVM have been reshaped, which addresses this comment in particular.

- **Line 461**: Provide specific details regarding what you observed on the chlorophyll a fluorescence profiles.

  We observe no dilution of the DCM after storm. Added in the manuscript.

- **Table 3**: Ensure that this table is referenced appropriately in the text. Note that *Centropages typicus* should be italicised and use lowercase for the species epithet.

  This issue has been resolved, see new table below:

Type of analysis	Depth	Taxonomic group / species	ANOVA p-value	p-value A1st vs A2nd	p-value B1st vs B2nd	p-value F1st vs F2nd	p-value M1st vs M2nd
ANOVA Depth	0-100 m	Appendicularians	0.124				
		Chaetognatha	**0.039** *	**<0.001** ***	0.0659	**<0.001** ***	0.108
		Cnidaria	**<0.001** ***	**<0.001** ***	**<0.001** ***	0.418	0.765
		Copepoda	**<0.001** ***	0.189	**<0.001** ***	**<0.001** ***	0.617
		Eumalacostraca	0.534				
		Foraminifera	0.429				
		Other_organisms	0.375				
		Thaliacea	0.929				
ANOVA Copepod species	0-100 m	Calanoida	0.255				
		Centropages spp.	**0.014** *	**0.002** **	0.104	**< 0.001** ***	1
		Corycaeidae spp.	**0.0104** *	**< 0.001** ***	0.797	**< 0.001** ***	0.992
		Euchaeta	0.581				
		Oithona	**0.0231** *	0.448	**0.0197** *	0.923	0.876
		Oncaeidae	**0.015** *	**< 0.001** ***	**0.031***	**0.025** *	0.87
		Pleuromamma spp.	0.928				

- **Line 484**: Satellite data are mentioned but not utilised. If available, these should be incorporated into the analysis or explicitly discussed.

  The use of satellite data is outside the scope of this paper, but it will be mentioned and presented in future works from the BioSWOT-Med cruise.