

The authors would like to express their gratitude to the reviewer for the insightful comments and valuable suggestions.

General comments

One of the less-studied components of the biological carbon pump is the MMP, which contributes significantly to carbon flux. While most research has focused on zooplankton, the role of micronekton in the present-day carbon budget remains poorly quantified. This study addresses an important gap in our understanding and is highly relevant to marine ecology. What stands out in the modeling approach is the incorporation of physiological traits specific to different taxonomic groups, adding a useful dimension to the analysis. However, I believe the model remains somewhat theoretical due to the lack of data for validation.

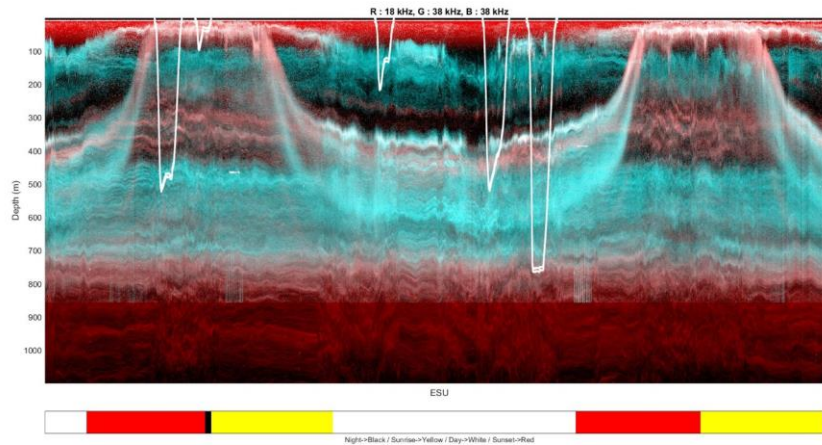
Incorporating net sampling and acoustic data, if available, would be crucial for calibrating and validating the model outputs. The authors mention the presence of trawl and acoustic sampling data from the APERO cruise, conducted in the same region during June and July, but it is unclear whether these data were used for model validation. This would be a key step to enhance the reliability of the results.

Answer:

We thank the referee for pointing out this aspect. Some of the data collected during the APERO cruise are useful for the calibration and validation of the model.

Regarding the calibration of the model, we have used the trawling data collected during the APERO cruise to calibrate the size classes and the taxonomic ratios, i.e. the relative biomass of fishes, cephalopods and crustaceans (Figure S7).

For the validation of our results, identifying species from acoustic data remains a challenge and this goes far beyond the scope of our study. Nevertheless, APERO acoustic and trawl data enabled us to characterize several migrating layers, moving from depths of 150-700 m during the day to approximately 40 m at night. These migrating layers sampled by trawling are mainly composed of a mix of fishes, cephalopods, euphausiids and decapods. We can therefore compare the simulated depths reached by micronekton taxa in our model with the depths observed in acoustic echograms. For example, migrant fish are mainly composed of myctophids. Similar to the findings of Watanabe et al., (1999), we found a maximum concentration of fish at 0-50 m during the night and at 400-500 m/600-800 m during the day, depending on the station. Here is one example of an echogram of a station of this APERO cruise showing several migration depths, with white lines representing the trawl's trajectory:



In addition, several myctophid species was reported in the literature with a depth stratification by size during the day (e.g. Frost and McCrone 1979). Quetglas et al. 2010 observed the same pattern for migrating species of the genus *Histioteuthis*, that were the main migrating cephalopods collected in trawls of the APERO cruise.

We will clarify this point and improve the manuscript accordingly.

I would also like to have clarifications for the choice of 200 m depth as the euphotic zone for calculating the efficiency of particulate organic carbon (POC) transport. Most studies typically use a depth of 100 m for the euphotic zone, so it's important to explain the reasoning behind your choice of depth. Was this depth based on specific data from the PAP-SO station, or was it taken from existing literature? The selection of the depth threshold for the euphotic zone is critical, as it can significantly affect the calculated efficiency of POC transport. If not well justified, using 200 m instead of 100 m could potentially skew the results, leading to over- or underestimation of the POC transport efficiency. The export efficiency is usually calculated using the flux at a specific depth over the net primary production, what does integration phytoplankton concentration in the surface layers represent? is it a biomass?

Answer:

We agree with the referee that this choice needs clarification. We chose 200m depth for the export of carbon as the average of the mixed layer depth (MLD) at PAP-SO. In the manuscript, we will therefore use MLD depth rather than euphotic zone. In addition, in Figure 7b, we will choose a more realistic approach as the MLD at this station varies annually between approximately 30 and 300 m (Hartman et al., 2012). We will then calculate the variability of the efficiency of carbon transport by including the annual variation in MLD as the export depth. However, this will not significantly change the dynamic of carbon export as the MLD is deep in winter, micronekton are found in shallower depth, and production is low.

As the state variables (P, G, C) unit is in $\text{mgC}\cdot\text{m}^{-3}$, we have defined the efficiency of carbon transport as the proportion of carbon biomass exported under a certain depth over the phytoplankton biomass ($\text{mgC}\cdot\text{m}^{-3}$).

We therefore agree that we it should be called something other than pe-ratio as it is a different metric. We propose to call it Micronekton Carbon Export ratio (MCE-ratio).

Although the current model is a simplified 1D water column setup, the authors made several assumptions and choices in their study's design to assess the role of the DVM of micronekton on the organic carbon budget. One significant assumption is that mesozooplankton do not migrate and are restricted to the epipelagic layer, which could influence the estimated carbon flux. Numerous studies, including those by Kiko et al. (2017, 2020), and Bianchi et al. (2013), have demonstrated that zooplankton also exhibit DVM and that they are usually present between 300–600 m. Incorporating a portion of zooplankton in the deeper layers of the model would be important, as they could serve as prey for micronekton, potentially contributing to fecal pellet production and, in turn, to carbon transport.

Answer:

We agree with the referee that this assumption is simplistic and needs to be justified. Stomach content analyses of selected micronekton organisms were performed during the APERO cruise. Migrating micronekton had undigested prey items only at the surface and not at depth during the night, where we found only digested food suggesting that prey items were ingested earlier in the day. Sameoto (1989) had similar findings for *Benthosema glaciale*, the most common mesopelagic fish species found during the APERO cruise. We believe that they migrate to greater depths than zooplankton, notably because of their greater risk of being spotted by predators and their greater swimming ability. Therefore we did not model zooplankton diel vertical migration as this may not impact micronekton consumption.

Another issue concerns the environmental variables influencing DVM. For instance, oxygen concentration plays a crucial role, especially when micronekton inhabit oxygen minimum zones (OMZs). In such zones, low oxygen availability limits respiration and metabolic rates, impacting vertical migration behavior. However, the model assumes that micronekton feed exclusively at the surface, without considering the potential effects of hypoxic conditions on DVM patterns and metabolic processes, this might be due to the modeling of the PAP-SO station. But factoring in these environmental constraints could offer a more nuanced and accurate representation of micronekton's contribution to the carbon cycle and would make the model fit to be globally applied to a large range of environmental conditions such as the Atlantic OMZ.

Answer:

We agree that it would be necessary to add oxygen as a factor limiting migration depths under a certain threshold of hypoxia, in order to use this model at global scale. We will mention this aspect in the discussion.

We have selected the environmental variables that can significantly influence micronekton dynamics at PAP-SO station, where there is no such hypoxic conditions.

Another concern I have is with the light in the model. The equation in the supplementary materials assumes that surface irradiance is zero at night, which is not entirely accurate. Even at night, there is still some ambient light (e.g., from the moon), and predators that rely on visual predation may still be able to feed, albeit less effectively. The current model restricts feeding to dusk and dawn, which is problematic since micronekton are supposed to be migrating between surface and deeper layers during these periods. According to this assumption, predators would be unable to feed and, over time, would likely starve. However, when examining Figure 2 in the manuscript, it appears that surface light never actually

reaches zero, contradicting the assumption in the equation. This inconsistency suggests that something might be missing or oversimplified in the supplementary equation. Could it be that the equation does not fully account for low-light conditions at night? Is there a missing parameter to this equation? Additionally, have you considered varying light levels to simulate different migration depths?

Answer:

As mentioned by both referees, we agree that the method used to model surface irradiance needs to be clarified. In addition, we made adjustments to allow micronekton to feed more efficiently during twilight periods when it is not migrating.

To model daily irradiance, we propose to use a sinusoidal curve. For the gradient of surface irradiance (cf Eq.S1), which allows us to calculate migration speed, we now set a threshold: when micronekton reach the surface, they begin migrating once the light gradient falls below this threshold. The threshold has been set at 0.1 and the sensitivity of this parameter on carbon production will be investigated in the sensitivity analysis.

We also added a constant, that we now call I_{\min} , to prevent the light level from reaching zero. As suggested by both referees, we will clarify this point in the supplementary material. For the computation of the capture rate, this constant must not be too high. We thus have set I_{\min} to 0.01, so that micronekton can still feed at night but much less efficiently than during the feeding windows.

A few small comments concern these two points: I am also unclear about the changes imposed on the parameter $C\alpha$ and why even though it varies between the scenarios we still have the same resulting detritus concentration. Could you provide further clarification on how the $C\alpha$ was modified and why such large differences do not lead to corresponding changes in detritus biomass? In equation 6, it is unclear to me, if remineralization only applies to fecal pellets or if the author considered it also in the dead organisms, considering that both processes would contribute to organic matter degradation, so excluding remineralization for dead bodies seems inconsistent.

Answer:

We thank the referees to point out this problem. As indicated in the reply to reviewer 1, the objectives of Figure S4 in the supplement material are not to present results corresponding to realistic scenarios. We aimed at testing the sensitivity of seasonal variations of temperature, light and phytoplankton biomass on carbon production. Therefore, we used C_{α} as a scaling parameter to ensure a consistent prey/predator dynamic, i.e. to prevent consumers and resource concentrations from collapsing. This is why the carbon biomass is similar, as we were interested in the differences in annual variability between the scenarios. This will be clarified in the Material and methods section.

The different values of the attenuation coefficient explain the important differences in C_{α} values. With the redefined values for the attenuation coefficient, the range of C_{α} values is now reduced (between 0.7 and 7).

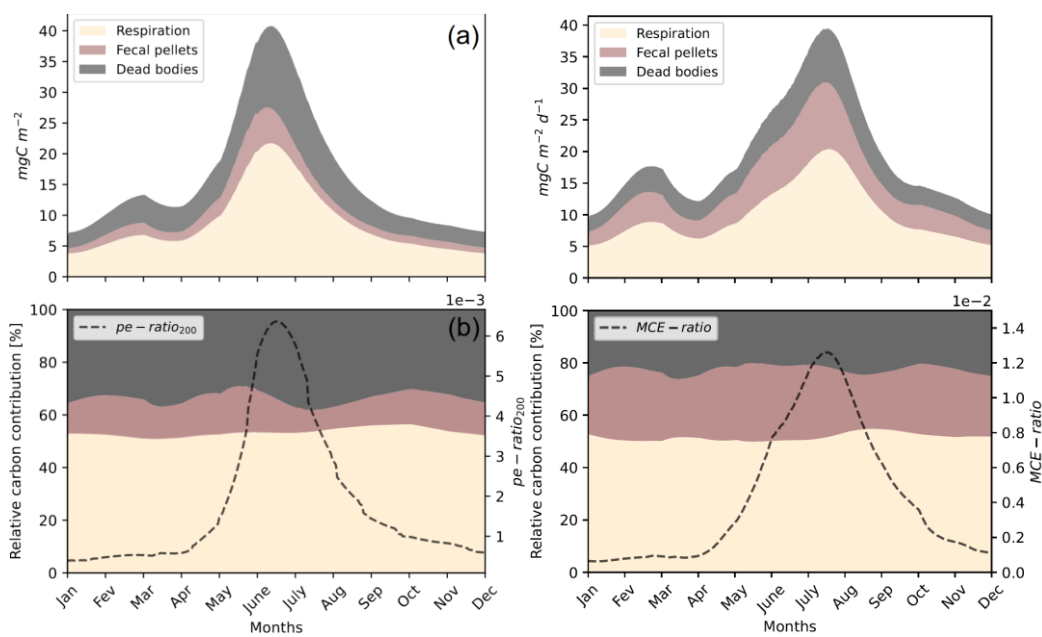
We also agree that it is not pertinent to add this remineralization term only for the fecal pellets. As our study focuses on the variability of carbon production by micronekton, we

should not have included particle remineralization. We will correct the manuscript accordingly in equation 6 as follow,

$$\frac{\partial D_g}{\partial t} = (1 - e)dG$$

We re-run the simulations for the seasonal scenario in Figure 7, taking into account the adjustments made to the model as presented in both answer to the referees. This includes the modification of surface irradiance, the threshold for migration speed, the non-remineralization of fecal pellets and the export depth varying annually with the MLD.

Here is the comparison between the old results on the left and the new ones on the right for the annual variation of the carbon production integrated along depth:



The carbon production and its dynamic remain similar, but the peak is slightly shifted.

The proportion of fecal pellets is also higher than the previous results as we removed the remineralization term.

In conclusion, while this study holds significant potential, it still needs additional work for it to be published. The article could benefit from improvements in the clarity of idea presentation and the explanations behind methodological choices. The authors should validate the model, as this is essential for ensuring the robustness of the results. They also need to refine the treatment of light, zooplankton migration, remineralization, and detritus dynamics, and resolve inconsistencies in parameter adjustments.

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