This study analyzes the role of diurnal vertical migrations performed by micronekton (fish, crutaceans and cephalopods) on the biological carbon pump. The authors developed a simple model describing explicitly the vertical movements of the animals as well as ingestion, respiration and the production of fecal pellets. The model relies on three state variables which are the biomass of the preys, i.e. mesozooplankton which do not perform DVM, the biomass of the consumers and the gut content. The latter variable is necessary to accurately describe the production of fecal pellets. The consumers are visual predators meaning that they need light to capture their preys. During the night, they reside near the surface to feed. At dawn and dusk, they swim to stay at depth during the day so that they escape predation from their visual predators. In the model, temporal variation in the light levels triggers DVM. The model is run in constant as well as seasonnally varying environmental conditions. It is used to explore the role of size and taxonomy on the DVM patterns and its impacts on the active vertical transport of carbon. Detailed sensitivity analyses are performed by systematically exploring the parameters space. The main findings are: (1) in the temperate regions, DVM is responsible for an important vertical transport of carbon from the euphotic zone to the mesopelagic domain; (2) Size and taxonomy play a big role in driving this transport; (3) There are strong seasonal variations of this active export of carbon with a maximum reached in summer; (4) Results are very sensitive to some parameters, such as the vertical swimming speed, the metabolic rate and its sensitivity to temperature.

This study addresses an important topic which is increaslingly acknowledged as an important component of the biological carbon pump. The modeling framework is relevant as it includes the essential aspects of the animal physiology and it explicitely simulates the vertical movement of the organisms. It remains simple enough so that detailed sensitivity analyses remain feasible and can be applied to a large range of environmental conditions, except probably in the polar regions (polar days and nights). It is well written, even if as a non-english native speaker, I am not necessarily the best person to judge this aspect. Supplementary materials are interesting and bring some important additional information to the manuscript. However, I have some serious issues with the study, mainly with the model description and modeling assumptions. As the code is available on a github server, I have closely inspected it to check what is stated in the manuscript.

First, there is an issue in the system of equations 1. Specific dynamic action is a respiration term and thus, represents a loss of carbon. However, this is not included in the system despite the fact that it is routed to inorganic nutrients (equations 6). I checked the code to see if this corresponds to a bug but this is not the case as it is properly taken into account in the temporal evolution of the C biomass. This should be corrected since before I checked the code, I thought that the manuscript was relying on bugged results.

Answer:

We thank the reviewer for pointing out this mistake. There was a problem in writing the system of equations 1 as we did not included the specific dynamic action in the metabolic products. This term was well included in the code and therefore it did not affect the results. We replaced the term mC by Dm (Eq.6), which includes now all the metabolic products:

$$\begin{cases} \dfrac{\partial P}{\partial t} = \rho P(1 - \dfrac{P}{K(z)}) - \dfrac{\alpha_v(t,z)P}{1+\beta P}C \\[3mm] \dfrac{\partial G}{\partial t} = -\dfrac{\partial(wG)}{\partial z} + \dfrac{\alpha_v(t,z)P}{1+\beta P}C - (d+\mu)G \\[3mm] \dfrac{\partial C}{\partial t} = -\dfrac{\partial(wC)}{\partial z} + edG - D_m - \mu C \end{cases}$$

Second, the daily evolution of light is said to be described by equation 1 of the supplementary materials. This equation is difficult to read because the exponent n should apply to the sin function and not to wt. Furthermore, this equation implies that light levels are zero at night meaning that visual predators cannot feed at night when they stay close to the surface. The only available temporal window in that case is during dusk and dawn (about 2-3 hours a day). However, during that period, they move either up or down which implies that they are not colocated with their preys. Thus, according to that equation, they should not really be able to feed and they should starve and die. Yet, this is not the case. I was also disturbed by figure 2 which shows the daily evolution of light at the surface. On that figure, the relative light level during the night is not zero as it should be according to the equation displayed in the supplementary materials but somewhere between 0 and 0.6. I inspected the code and saw indeed that the actual coded equation is not exactly that of the supplementary materials but rather equation 1 to which a constant 0.5 is added. This explains why the consumers are able to survive in the model since with this 0.5, they are then able to feed at night. However, this 0.5 implies that light level during the night is about one third of its value during the day, which is obviously not correct! In addition, the Beer-Lambert equation is used with an attenuation coefficient of 0.001 m^(-1), meaning an attenuation depth of 1000 m! In very clear water, the coefficient is rather of the order 0.02-0.03, which is at least one order of magnitude higher ... This is not a typo because this 0.001 is the actual value used in the code. And the option to use a chlorophyll dependant coefficient is not activated and not included in the call to the beer-lambert function. To conclude with my issues on light, once in the manuscript and once in the supplementary materials, the authors say that day length at the winter solstice is 6 hours and is 12 hours at the summer solstice. This is obviously incorrect as day length at the summer solstice should be 18 hours.

Answer:

As mentioned by both referees, we agree that the method used to model surface irradiance needs to be clarified. In addition, we have 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}
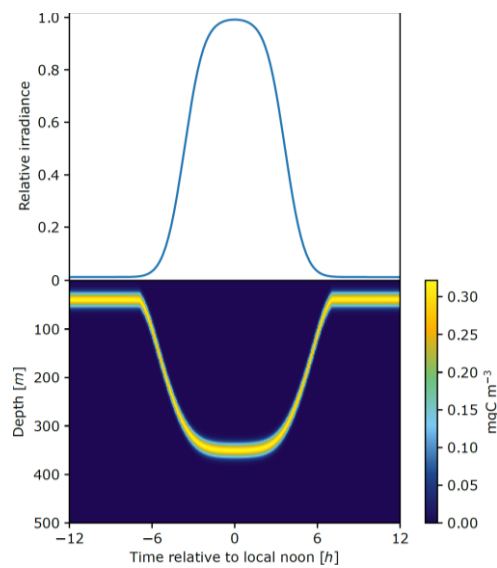
Third, in the supplementary materials, the authors shows results from some sensitivity results on the seasonal variations of temperature, light and PP (which is in fact phytoplankton biomass rather than PP). This is very interesting. Yet, I don't understant the changes they impose on the parameter C_alpha. In scenarios 1 and 2, this parameter is set to 3 but when light is seasonally varying (scenarios 3 and 4), it is set to values that are between 2 and 3 orders of magnitude lower. Yet, the resulting detritus biomass is similar. Furthermore, the chosen values in that second case are not consistant with figure S8. Weird!

Answer:

light and phytoplankton biomass on carbon production. Therefore,, we used C_alpha 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_alpha values. With the redefined values for the attenuation coefficient, the range of C_alpha values is now reduced (between 0.7 and 7).

Fourth, I don't understand why the authors have added a **remineralization term** in the fecal pellets equation (equations 6). Are the detritus pools prognostic variables or simply diagnostic variables? I did not check the code for that specific aspect. What do the authors call the production of fecal pellets? Is it (1-e)dG or (1-e)dG -r(T)D_g? Why including this remineralization on fecal pellets but not on dead organisms? Why only remineralization and not vertical sinking which, for organisms of that size, is way more important at controlling the concentration than remineralization? This needs to be explained and discussed. On the topic of production, I don't understand what is displayed in Figure 7a. If this is the temporal evolution of detritus production, then the units are wrong. If this is the temporal evolution of the detritus pools, this is impossible since the animal dead bodies and the inorganic pool do not have a sink term. This is also the case in Figure S4.

Answer :

We agree with the referee that it is not appropriate to add the remineralization term for the fecal pellets only. 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 followed,

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

In figure 7a and S4 we showed the temporal evolution of detritus production, so this should be expressed in mgC m^{-3} d^{-1}. We thank the referee to point out this error.

And as a biogeochemist, I was a little bit disturbed by the definition of the pe-ratio which traditionnally is the ratio of the export at some depth over PP whereas here, this a ratio between a flux and biomass

Answer:

As the state variables (P, G, C) unit is inmgC 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 (mgC m^{-3}). We therefore agree that it we 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).

I had some more minor points but considering the main issues I listed above, I think they are not really relevant at this stage. I did not make any comments on the results and discussion because of the concerns I had on the model formulation which according to me, raises

questions about the validity of the results. Regarding the results, one intriguing observation is the occurrence of small, localized peaks in the production of dead bodies between the epipelagic zone and the depth at which organisms reside during the day. What could explain these peaks?

We thank the referee to point out this problem. We checked carefully the code and found that this is due to a numerical artefact linked to the numerical scheme: the small peaks disappear when using a smaller time step. To counteract this numerical issue, a smaller time step of 0.2h allows us avoiding this numerical anomaly, as shown in the following figures:
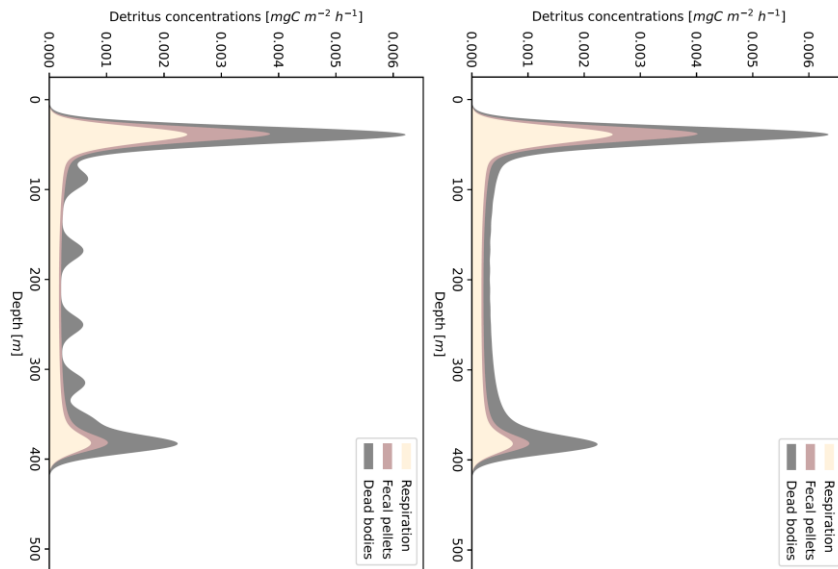
dt=0.8h   dt=0.2h

In conclusion, while this study shows considerable potential, it is not ready for publication in its current form. The authors need to revise certain aspects of the model formulation and provide clearer justifications and descriptions of their choices to ensure that the results are robust enough to support the subsequent analysis.