Review comments for the manuscript “Simulating vertical phytoplankton dynamics in a stratified ocean using a two-layered ecosystem model” by Zheng et al.

Reviewer: Camila Serra Pompei (DTU).

We would like to thank the referee for their thoughtful and constructive comments, which have helped us improve the quality and clarity of our manuscript. Below, we address each comment in detail and outline the revisions made to the text. The referee’s comment is shown in black, and our response is beneath in blue. In addition, to improve the manuscript, we have corrected grammatical errors and rewritten the summary section to improve the flow.

In this study, the authors developed a 2-layered NPZ model to better understand the mechanisms driving surface and sub-surface plankton dynamics in a stratified system. The BATS Ocean station is used as a case study, and the model is used to better understand the diverging phytoplankton trends observed in the surface vs subsurface layers in this location. The model provides an interesting avenue to better represent stratified systems with simple NPZ models, where the good model performance compared to field data highlights the strength of the approach. The article is relevant as it shows that different processes drive the dynamics of these two plankton communities, and that changes seen at the surface do not necessarily reflect changes observed in the sub-surface layer.

The manuscript is well written, clear, and model assumptions are well stated. My comments mostly center on the choice of euphotic zone depth and how this affects the representation of the sub-surface community and model comparison with data. There are also some aspects of the sensitivity analysis that need to be clarified.

Thank you for your kind comments. We appreciate the referee's insightful feedback regarding the euphotic zone depth and its influence on the representation of the subsurface community and model comparison. We have carefully addressed these points in the revised manuscript. Below, we provide detailed responses to each comment and outline the corresponding changes made to the text. In the tracked-change manuscript, we highlighted all the changes in blue except for the appendix text.

Comments regarding the assumption of the euphotic zone depth:

The assumption of 0.001% might need to be discussed in the discussion section.

We appreciated this suggestion and have added a paragraph to discuss the impact of different assumptions of euphotic zone on subsurface chlorophyll comparisons (see lines 491-497 in page 25 in the tracked-change manuscript).

Although phytoplankton might have been seen growing at these low light levels, is the choice of such a light level representative of where most phytoplankton in the subsurface layer reside?

To clarify this, the choice of the euphotic zone must meet two key criteria in our model. First, the euphotic zone must always extend deeper than the mixed layer depth. This ensures that our simple box model, designed for stratified ocean conditions, consistently maintains two distinct layers, which is essential for mass-conservative. Second, we use the euphotic zone as a reference for setting a lower boundary, indicating that the majority of phytoplankton reside above this depth.

The choice of this low-light level results in the sub-surface layer always having a zeu deeper than 300m, and a depth range (zeu-zm) varying between 150 and 300m. On the other hand, when I look at BATS Chl data, the bulk Chl does not seem to go deeper than 180m, and anything deeper than that has an extremely low Chl concentration.

We show the vertical distribution of chlorophyll measurements in Figure A1 in Appendix A (see page 29). As you said, most of the measurements are above 200 m; however, several measurements extend to around 250 m. We define the euphotic zone to include all available chlorophyll measurements in the upper ocean.

So, it seems to me that the depth range of the sub-surface layer encompasses two layers in the “real world”: the deep Chl maximum and whatever is below it.

Yes, it is designed to contain the deep chlorophyll maximum and whatever is living below it, until the bottom boundary we defined.

So, in short, could the authors discuss how this assumption affects the model compared to the real world and how it affects its comparison to field data for the sub-surface layer?

Yes, following your comment, we have done a sensitivity analysis (see appendix A in pages 27-31) and discussed this below.

The above comment specially applies in regard to the method chosen to compare the subsurface layer with field data: first field Chl is averaged between zm and zk and then multiplied by the thickness of the sub-surface layer in the model (zeu-zm). Can the authors mention if the sub-surface layer is representative of the zk-zm depth range?

We assume that the subsurface layer in model (blueline minus redline in Figure A1) is a representative of the z_k-z_m depth. Figure A1 shows that, the deepest measurement above the euphotic zone as defined in the model, is relatively close to the modelled euphotic zone depth at each time step, although there is a difference of approximately 50 m. Thus, we believe this assumption is reasonable.

The value of zk is missing in the text and should be provided.

The value of z_k varies at different time steps because the measurements are not uniformly distributed. In the manuscript, we defined z_k as the deepest depth level just shallower than the euphotic zone (z_{eu}). Therefore, it is also dependent on the euphotic zone definition. To clarify the values of z_k for readers, we have added Figure A1 in an appendix to illustrating the definition of z_k explicitly and revised the sentences in lines 252-254 in page 11.

If possible, it could have been nice to have some additional figures (e.g. in an appendix) showing the vertical distribution of the field data used. This will help the reader have a better notion of the effect of the data transformation for model comparison.

Thank you for the suggestions. We have added Figure A1 in Appendix A (see page 29) to show the vertical distribution of the field data used.

Does the zeu assumption affect the light experienced in the sub-surface layer?

First, we apologise for the misunderstanding caused by a typo in this manuscript. Our definition of euphotic zone is based on 0.0001% light level, calculated by 13.8/Kd. We have carefully corrected this in the revised manuscript.

The z_{eu} assumption does not significantly affect the subsurface light magnitude (Figure R1 below) because we only use vertically averaged subsurface light in this model. However, there are naturally differences in subsurface light in the model when using different assumptions of z_{eu} in the summer.

We have included a sensitivity analysis of euphotic zone in Appendix A where a detailed description is provided (see pages 27-28). Here, Figure R1 below compares subsurface light from the model using three different definitions of z_{eu}, and shows different definitions of z_{eu} have only a small influence on subsurface light, with the largest differences seen during summer.

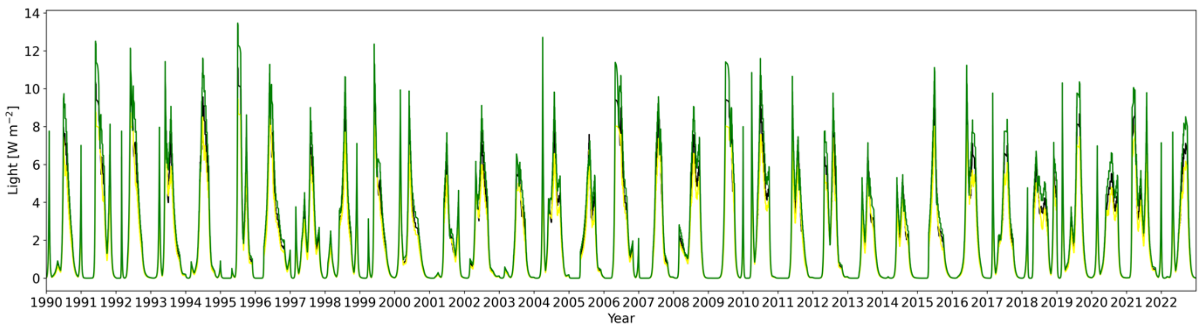

Figure R1: Subsurface light from the model running with different euphotic zone defined as 0.001% (green line), 0.0001% (black line, definition in manuscript) and 0.00001% (yellow line) light level.

Next, we examine the seasonality of the subsurface light time series (Figure R2). Figure R2 shows that the green line consistently has the highest values, while the yellow line remains the lowest. This indicates that subsurface light intensity is stronger when modelled with a shallower z_{eu} (green line) and weaker when based on a deeper z_{eu} (yellow line). This difference is particularly pronounced in summer (Jun-Aug), but again overall, these differences are relatively small.

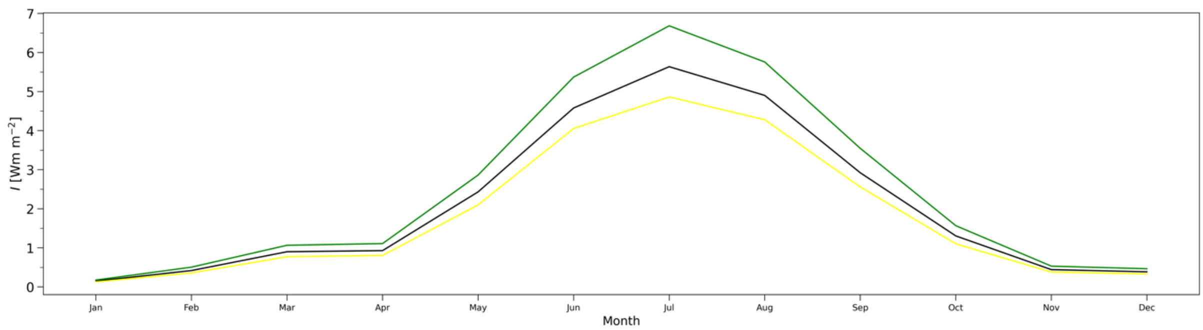

Figure R2: Subsurface light averaged seasonality from the model running with different euphotic zone defined as 0.001% (green line), 0.0001% (black line, definition in manuscript) and 0.00001% (yellow line) light level.

How would Chlc,d and Chld change when choosing a different zeu value? Given the exponential decay of PAR and its averaging to use as input in the model, I expect Chlc,d not to change much, but Chld will certainly change (simply due to its multiplication with zeu). How would this affect the data comparison? In other words, it would be good to do a sensitivity analysis on the 0.001% value assumed to define zeu.

Following your suggestions, we have conducted a sensitivity analysis and included the key results in Appendix A (see pages 27-31) to thoroughly address the questions raised above. We ran the model with a shallower z_{eu} defined at 0.001% (z_{eu}=11.5/Kds) and a deeper 0.00001% (z_{eu}=16.1/Kds) surface light level respectively to evaluate how these definitions influence the comparison of modelled chlorophyll with observations. First, we found that different definitions of z_{eu} do not significantly affect subsurface chlorophyll concentrations magnitude ($Chla_{c,d}$) (see Figure R3 below), however, they do influence the seasonal peak values of $Chla_{c,d}$ Thus, we also show the climatology of $Chla_{c,d}$ in Figure R4 below. It shows that, the $Chla_{c,d}$ simulated with a shallower z_{eu} (green) is consistently higher, while weaker when based on a deeper z_{eu} (yellow line). This is consistent with changes in subsurface light illustrated above (Figures R1 and R2). The largest difference in subsurface $Chla_{c,d}$ modelled from different z_{eu} occurs in summer.

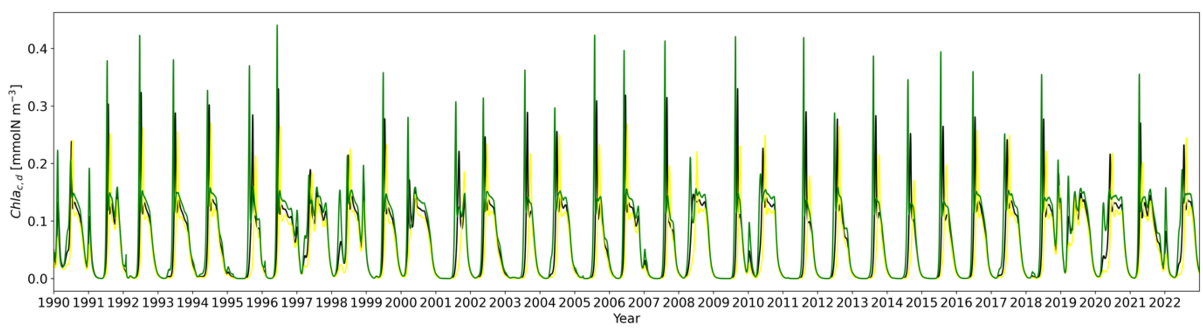

Figure R3: Subsurface chlorophyll concentration ($Chla_{c,d}$) from the model running with different euphotic zone defined as 0.001% (green line), 0.0001% (black line, definition in manuscript) and 0.00001% (yellow line) light level.

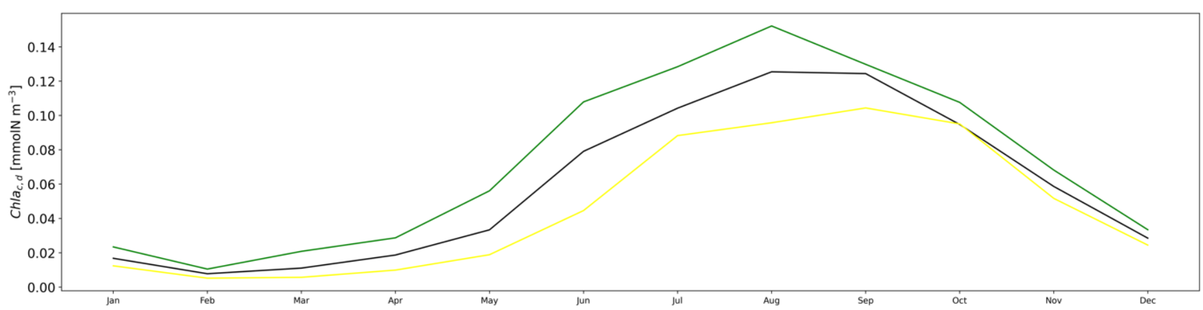

Figure R4: Subsurface chlorophyll concentration ($Chla_{c,d}$) climatology from the model running with different euphotic zone defined as 0.001% (green line), 0.0001% (black line, definition in manuscript) and 0.00001% (yellow line) light level.

Subsurface chlorophyll integration (Chl_d) does indeed vary with changes in the bottom boundary. The changes in euphotic zone in the model also correspondingly changes the observational integration, as the observational integration is calculated by a representative concentration multiplied by model's subsurface layer thickness. However, when comparing the model results with observations, we found different definitions of euphotic zone mainly impact on the seasonality comparison of chlorophyll rather than the long-term trend comparison. Therefore, these different definitions do not substantially alter the main findings (see Figure A2 and A3 in Appendix). A detailed explanation of this has been added to Appendix A of the paper (see pages 27-28).

To sum up: I suggest that (i) an additional paragraph in the discussion section is added addressing the zeu choice and how does this compare to the observed depth range of the subsurface plankton community, (ii) a sensitivity analysis is performed on the 0.001% value, and (iii) some figures showing the vertical distribution of the Chl data are provided in an appendix.

Thank you for summarising your suggestions clearly. In response, an additional paragraph has been included in the discussion section (see lines 491-497). A more detailed sensitivity analysis regarding the euphotic zone is available in Appendix A (see pages 27-31). Additionally, a figure illustrating the vertical distribution of the observational chlorophyll data has also been provided in Appendix A (see page 29).

Comments regarding the sensitivity analysis:

I am not sure I understand how ze0 has been varied in the sensitivity analysis. Ze0 is the initial value of the euphotic zone, but zeu is not a fixed parameter, it varies with surface light Kd, Chl etc. So, is this value only different in the first time-step and afterwards just follows the light forcing and kd and amount of Chl?

Yes, this value is only fixed in the first timestep to initiate the model. z_{e0} influences the concentrations of P, Z, N, and chlorophyll in the subsurface layer in the first time step (1990-01-01). Subsequently, this initial impact propagates to euphotic zone calculation from the following day onward as the calculation of the euphotic zone depends on chlorophyll. Given that the calculation of P, Z, N and chlorophyll at each time step will always depends on the euphotic zone, P, Z, N and chlorophyll from the previous time step, the impact of z_{e0} is carried forward and integrated into all subsequent calculations.

The same goes for zm0, since zm is externally forced, does this mean that only the value in the first time step is changed and afterwards all is the same as the MLD forcing?

Yes, this value is only fixed in the first timestep to initiate the model. Similar to z_{e0}, z_{m0} will impact the concentration and integration of P, Z, N, and chlorophyll in the subsurface layer at the initial timestep (1990-01-01). The calculation of P, Z, N and chlorophyll in both two layers at subsequent time steps will requires the z_m (MLD forcing), and P, Z, N and chlorophyll from the previous timestep. However, different from the z_{e0}'s impact, the z_m is externally forced.

Or does this initial condition somehow affect zm over the entire time-series?

No, it does not affect z_m. Because z_m timeseries is an input for this model. We use observational z_m timeseries from BATS site in this study.

L188, why is the initial value of zm set as the mean of the MLD? shouldn't it be set as the initial value of the MLD provided by the data at the starting date of the model? Please provide an explanation on the choice of initial conditions for these forcing variables, why are they not the value provided by the forcing input variables on the corresponding day of the time series?

This model requires a data from the day preceding the first date of the output time series to start the model. For example, if we want to output modelled results from 1990-01-01, we require all initial values on 1989-12-31. However, we do not have z_{m0} on 1989-12-31, unfortunately. We also aim to minimize uncertainty arising from selecting observations on a single specific day. Therefore, we opted to use the time-mean value of MLD as z_{m0}.

In terms of z_{e0}, it is more challenging to define z_{e0} compared to z_{m0}, since no direct observational time series of the euphotic zone depth is available. Instead, the euphotic zone depth is calculated internally within the model, requiring an initial guess. Therefore, the approach used for the MLD (time-mean value) is not applicable. According to Figure 2A in Anugerahanti et al. (2020) (https://doi.org/10.3389/fmars.2020.00549), a vertical time-mean profile of chlorophyll at BATS from their 1D modelling output and in-situ data indicates chlorophyll concentration at 250m is nearly zero. Thus, we used 250m as the initial value of euphotic zone to initialise the model.

We also used the approximate time-mean value of each variable (N, P, Z, chlorophyll) from other literatures to initialise the model because we do not have the data on 1989-12-31, and we wanted to minimize uncertainty arising from selecting observations on a single specific day.

Other comments:

L 225 Just to clarify, is the "methodology outlined by Viljoen et al. (2024)" the one explained in the rest of this paragraph?

The "methodology outlined by Viljoen et al. (2024)" explains in the rest of this paragraph.

Or is it something different?

No, it is the same.

L118 "but also involves the phytoplankton and zooplankton excursion from the surface layer (Eq. (14))" this makes it sound as if there was a vertical migration process. Perhaps rephrase this sentence as some kind of remineralization of dead plankton and sloppy feeding.

Thanks. The sentence has now been rephrased to: "It not only involves the phytoplankton and zooplankton cycle in the subsurface layer but also the remineralisation of nutrients from some of the dead phytoplankton and zooplankton from the surface layer (Eq. (14))." (see lines 125 in page 5)

L124 mention in this part of the text that table 1 shows a description of the parameters.

We have now added 'with all related parameters described in Table 1' at the end of this sentence (see line 130 in page 6).

L147, please indicate in this part of the text the units of N, P, Z and Chl in the model.

We have now added a sentence to explain the units of N, P, Z and chlorophyll in the model. Please see lines 155-156 in page 7 of the revised manuscript.

Equation 11, remove the multiplication signs (*) for consistency with the rest of equations. The (C:N) ratio could be given a parameter name (e.g. Q_{C:N}, or whatever you want to call it), since phytoplankton C:N ratio is fixed in the model. I also suggest changing "MWc" to contain only one capital letter and have the rest of letters as subscripts, as not to be mistaken as two different parameters.

We really appreciate these comments, and have removed the (*) from the rest of equations (see Eq. 11 in page 6). We gave the C:N ratio a parameter name of Q_{C:N} and we also gave MWC a parameter name of M_c. Accordingly, we have changed those names in Eq. 11 and 20, in lines 164-165 (in page 7), in Table 1 (in page 9), in line 227 (in page 10), and in Figure 2's caption (in page 12).

Mention earlier in the text that the model has not been spinned-up but that it converges quickly. This is mentioned in the results section, but I was wondering about it while reading the methods.

Thank you for this suggestion. We have added this information in the methodology (lines 245-246 in page 11).

L269, I would not use the word "histogram", as the figure is not a histogram. Perhaps "bar-plot" or simply refer to the figure.

Thank you pointing this out. We have now used bar-plot to refer to the figure (line 281 in page 12).

L283 typo in "notebaly"

Thank you for correcting the typo. We have corrected it to "notably" (line 295 in page 13).

L334 "modelling" should probably be "modelled"

Thank you for your suggestion, we have changed it to modelled (line 346 in page 16)

L335 “is very similar to in observations” should probably be rephrased.

Thank you for these suggestions. This sentence has been rephrased to “The standard deviation from the model (6.94 mg m^{-2}) exhibits strong similarity to the observations” (see line 348 in page 17).