Review of 'PIBM 1.0: An individual-based model for simulating phytoplankton acclimation, diversity, and evolution in the ocean' by Sala and Chen

The paper introduced a new individual-based plankton model that allows phytoplankton acclimation. The paper consists of a description of the model and a presentation of its results. This is a worthwhile effort that can contribute to the plankton modelling community. However, more effort seems to be needed to convey the meaning and structure of the model more accurately and to convince readers that this new model is an improvement over existing models in spite of substantially increased complexity. This is particularly true as the paper did not attempt to clarify phenomenon or compare it with the Eulerian model results.

Although the new model reproduces the seasonal variation of the underlying observed variables to some extent (Fig. 4), I do not expect that the results are an improvement over those from much simpler plankton models. The present model has an advantage of providing information that was not available before, but it is difficult to assess how reliable these results are (e.g., Fig. 3, 8, and 10). If validating a model directly is difficult, sensitivity tests can help. As many previous papers have predicted that the effect of phytoplankton acclimation is not important under the realistic ocean conditions, it is important to at least compare the results in which the effects of three phytoplankton traits (size, optimal temperature, and light affinities) are neglected. It is also interesting to see if the results without these effects differ from the Eulerian model results.

The model description is unclear, making it difficult to understand how the model is calcluated. I could not find the procedure for calculating the three traits in the paper. The proper explanation how these traits for Lagrangian plankton particles are calculated, except mutation (L194), and how their initial conditions are given, especially temperature and light affinity. I cannot understand the relationship between C_{div}, ESD, and V (in Table 2), while all these variables appear to represent the cell size. Nor can I understand how these traits are affected by the motion of Lagrangian particles such as z_i. Note that this is the most essential part of the new model.

It is important to mention that the inclusion of more empirical formulae does not necessarily improve the model. The authors tried to include a large number of existing empirical formulae without justifying why they should be included. The phytoplankton model largely follows that by Geider et al. (1998). Here it is important to specify how the new model differs from Geider et al. (1998). It is not clear whether the zooplankton model used the existing model, or developed a new model. Again, the model equations are listed without any explanation to justify their use.

Other points:

L.205: R package TPD - necessary to explain

L305: no explanation on how to calculate the size spectra of phytoplankton

L325: Information on the temporal resolution of forcing and on the type of the 1D model is necessary. If the 1D model includes nonlocal mixing, K_v does not necessarily predict the vertical motion of particles.

L346: I should mention that the calculation of z_i using random walk can exaggerate the variance of traits, because it neglects the correlation of two particles located nearby. It also neglects the effects of horizontal diffusion and patchiness.

L371: The Neumann boundary condition does not mean no flux at the boundary.

Fig. 2: It is unrealistic that MLD is zero during the summer.

Fig. 11: error in the caption (red solid lines for summer).

L547: This section should be moved to the introduction. It may also be important to include references of Lagrangian plankton models using different approaches such as Jokulsdottir & Archer (2016), Kida & Ito (2017), and Noh et al. (2021). It is especially so, when references of Lagrangian cloud models are included.