

Interactive comment on “Dissolved organic matter release by phytoplankton in the context of the Dynamic Energy Budget theory” by Eleni Livanou et al.

Anonymous Referee #3

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General comments

The authors propose a phytoplankton dynamics model based on DEB theory. They focus the application of their model for explaining the fluxes of DOM release by phytoplankton cells, by considering passive diffusion associated to cell lysis and active exudation associated to unprocessed substrates under stoichiometric constraints. Even if I'm convinced that DEB theory may provide mechanistic models which could help us to understand the processes studied here, I'm have several major problems with the present version of the paper. The first one concerns the model validation, the second one is about some choices in the processes formulation, and the third one concerns

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the model presentation.

Specific comments

- The authors use data published in Flynn et al., 2008 for comparing model outputs to experimental results. There is no description of the methods used : which distance between data and model is used, how is it optimized, which algorithm is used for the ODE's? Moreover, there is a large number of parameters and there is no information on their correlation in the estimation procedure, the set of data is rather reduced with respect to the number of parameters. Could the authors give some uncertainties on their model output?
- Most of the fluxes formulations are based on SU's dynamics, but the schemes are not provided (maybe they could be the object of a supplementary material). However, among all these formulations, two of them have not the SU's support. This is the case of formulas (5) and (6). These formulations are far from standard ones and are not explained. And furthermore, the square on the phosphorus content q_P and the nitrogen content q_N in the reserves is a surprising refinement, what is the model sensitivity to such a refinement? Could the authors show some data to validate these formulations? Another point is the fact that in the subsection dealing with the growth rate, the authors omit to mention that the growth rate is not explicit. I'm not sure that this can be understood easily by readers who did not try to use multi-reserve DEB models before. This is probably not a strong constraint for the present paper, but having to solve an algebraic equation to calculate the phytoplankton growth rate at each time step could be a strong limitation for 3D-biogeochemical models at large scales. This could at least been mentioned and if the authors have suggestions to solve this, it could be an interesting contribution.
- The model description should be accompanied by a scheme more adapted than Figure 1, I mean that a scheme focused on the fluxes included in the model would

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help to follow the model description. The subsection 2.2 should have another name, like “Formulation of processes” instead of “Model equations” because the model equations are already provided in the previous subsection, with undefined process formulations. As it is done, the model description is quite hard to follow.

Since the results and discussions could strongly depend on arbitrary choices (formulations) and parameter values (estimations), it is hard to be convinced about their validity or about their degree of generality now.

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