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Interactive comment on "Flexible C : N ratio enhances metabolism of large phytoplankton when resource supply is intermittent" *by* D. Talmy et al.

Anonymous Referee #2

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General comments

The present article explores the physiology of phytoplanktonic cells of different sizes using a mechanistic model with flexible C:N ratio and allometric relationships. The manuscript is clear and well written. It addresses a relevant scientific question within the scope of BG, since phytoplankton plays a major role in aquatic ecosystems and global ocean biogeochemical cycles.

Indeed, organism size plays a major role in structuring plankton community but its impact on the phytoplankton metabolism and photophysiology still remains poorly known. Since the impact of phytoplankton size may be important under intermittent light and

nitrogen supply, the authors have developed an original mechanistic model for phytoplankton physiology. This model takes into account photoacclimation and energy storage using a flexible C:N ratio. It is empirically constrained from allometric relationships and tuned against previous experimental data. In the Methods section, a brief overview of the model is given, before going into more details. The model equations are very clearly described and numerous references are given. The model is tuned against available experimental data sets for different species.

The comparison of the model outputs with the available data comfort the modeling approach adopted by the authors. Their results give new insight on "how energy stored in carbohydrate and lipid influences phytoplankton growth rate in environments with ephemeral" photon flux density (PFD). However, the choice of this ephemeral PFD is not clearly justified and it needs to be. Besides, it would be even more interesting to present different scenarios of intermittent PFD. This should not require too much work, so I suggest to the authors to test the impact of different scenarios of intermittent PFD and to better justify them and discuss them. This would also help the authors to build their discussion on the distribution of phytoplankton in the ocean on more solid grounds.

Finally, I am a bit disappointed by the Discussion section, since the authors finish it with two sentences that should be developed in two paragraphs (fluctuations in C:N ratio and impact on trophic interactions and biogeochemical cycles). Indeed these aspects are of huge importance of the audience of Biogeosciences.

For these reasons, and given the very good quality of this manuscript, which presents very interesting results obtained from well-applied methodology, I recommend **minor revisions** in order to 1) test and justify different intermittent light scenarios, 2) discuss in more details the implications of their work. This should easily be improved, especially since the authors mention in their conclusion that "the model presented may be combined with more detailed descriptions of PFD variability" (Page 5195, Line 24-25).

You will find more details bellow.

Specific comments

Main comments regarding the intermittent forcing:

- Page 5190, Lines 9-11: "modeled organisms were exposed to an intermittent PFD with constant nitrogen supply, and constant PFD with intermittent nitrogen supply". Please mention here the frequencies you used/tested and justify these choices.

- Page 5192, Line 5: Why did you use this light frequency? How relevant is it for phytoplankton in the global ocean? As indicated above, this needs to be mentioned and justified in the Methods section, and it will need to be discussed in the Discussion section.

- Page 5193, Line 1-2: " We used a model to understand how energy stored in carbohydrate and lipid influences phytoplankton growth rate in environments with ephemeral PFD". There is still a need to justify the frequency of PFD you are looking at...

- Page 5195, Line 24-25: "the model presented may be combined with more detailed descriptions of PFD variability". As mentioned above, this should be done in the present study (at least two simplistic cases/scenarios should be presented as in Figure 8). Otherwise, your article might just focus on the description of a new model and the comparison with experimental data, but without any implications for phytoplankton distribution at global scale, but it would then be much less interesting for Biogeosciences (whereas the quality of your work and of your manuscript are in agreement with the high standards of this journal).

Main comments regarding the discussion section:

-Page 5180, Line 17-19: " We suggest this mechanism is a significant constraint on phytoplankton C : N variability and cell size distribution in different oceanic regimes":

these two aspects should be discussed with much more details, for instance in respect to some recent articles dealing with phytoplankton stoichiometry and/or size. You could for instance refer to the recently published articles (and references therein) listed at the end of this review.

- Page 5195, Lines 10-14: " Accumulation of storage compounds is nonetheless responsible for large fluctuations in the C:N ratio, so is intimately connected with ocean biogeochemical cycles. Predator–prey interactions are thought to be modulated by cell stoichiometry (Mitra, 2006), so C:N dynamics described here may also influence foodweb interactions.": These aspects should be developed, especially in a journal as Biogeosciences.

Minor comments:

- Page 5189, Line 12: Some parameter tuning may have been better if you had use an optimization algorithm, even for a small number of parameters only.

- Page 5184 and following: it may be useful to write $V_n(S, N_R)$ instead of only V_n. Same comment for $V_m(N_R)$, $P_n(N_{LH}, N_F, E, C_R)$, $P_m(C_R)$.

Comments on the Figures and Tables

The legends of the Figures and Tables are very detailed and self-explained. However, I have a few comments on them.

- Table 4: Indicated the parameter that have been tuned and the ones obtained from previous studies.

- Figure 1: The labels a and b should be more visible. Add a layer with the continents on panel a because it seems a bit weird like this... The continent line should look like the same as in panel b.

- Figure 3: This Figure should rather appear as a supplementary figure, especially because it is not described nor discussed in the manuscript. Indeed, if I understood cor-

rectly, you just want to compare your relationships (black line simulated by the model) with (only?) three experimental points to justify your parameter values?

- Figure 5: The text on the figures seems very small.

- Figure 8: " Shaded regions correspond to complete darkness, whereas light regions correspond to 1000 ?mol photons m-2s-1": this should be mentioned in the Methods section, with an indication on the frequency of the light/dark cycle. Again, I am not convinced by the appropriateness of this light/dark cycle since you did not give any justification.

- Figure 9: The text on this figure is much too small. The frequency of nutrient supply and light and dark cycle is not clear.

Recent references that may interest the authors, especially for the discussion section:

Ayata et al. (2013) Phytoplankton growth formulation in marine ecosystem models: should we take into account photo-acclimation and variable stoichiometry in oligotrophic areas? Journal of Marine System, 125, 29-40.

Barton at al. (2013) The biogeography of marine plankton traits. Ecology Letters, 16(4), 522-534.

Barton et al. (2013) On the roles of cell size and trophic strategy in North Atlantic diatom and dinoflagellate communities. Limnology and Oceanography, 58 (1), 254-266.

Daines et al. (2013) Multiple environmental controls on phytoplankton growth strategies determine adaptive responses of the N:P ratio. Ecology Letters, 17(4), 414-425.

Daines et al. (2013) Multiple environmental controls on phytoplankton growth strategies determine adaptive responses of the N:P ratio. Ecology Letters, 17(4), 414-425.

Martiny et al. (2013) Regional variation in the particulate organic carbon to nitrogen

ratio in the surface ocean. Global Biogeochemical Cycles, 27, 1-9.

Moore et al. (2013) Processes and patterns of oceanic nutrient limitation. Nature Geosciences, 6, 701-710.

Smith et al. (2014) Leaving misleading legacies behind in plankton ecosystem modelling. Journal of Plankton Research, 36 (3), 613-620.

Ward et al. (2014). Modelling spatial and temporal patterns in marine plankton communities: top-down and bottom-up controls. Journal of Plankton Research, 36 (1), 31-47.

Interactive comment on Biogeosciences Discuss., 11, 5179, 2014.