

Interactive comment on “A mechanistic model of an upper bound on oceanic carbon export as a function of mixed layer depth and temperature” by Zuchuan Li and Nicolas Cassar

Anonymous Referee #3

Received and published: 9 August 2017

This paper develops a theoretical estimate of the maximum NCP that can be produced by the ocean for a given season (surface light) and mixed layer depth. Differences between this theoretical maximum and observed levels can then be attributed to nutrient limitation. The model appears to explain the distribution of observations that show higher NCP values are achieved in shallower mixed layers. The contribution of the paper to the field could be better highlighted and some areas need justification to prove their methods are valid. With these additions and clarifications, the paper could be a substantial contribution to the field.

The paper would benefit from more motivation for the model at the start. The introduc-

[Printer-friendly version](#)

[Discussion paper](#)



tion is fairly short and general. The reader would be more eager to dive into all the details of the model if the need for this model and the questions that the authors hope to address with it were clearly laid out near the beginning of the paper. Figure 3 demonstrates that there are patterns in the observations that we should seek to explain, but this is only briefly introduced at the start of the paper. Figure 4 shows intuitive results, so here too the motivation to do the global analysis should be specifically stated.

A large proportion of export is potentially controlled by bloom dynamics as phytoplankton escape heterotrophic grazing control or not. The proposed model misses these dynamics by forcing heterotrophic respiration to be solely proportional to phytoplankton concentration, rather than also include heterotroph concentrations. Of course, this simplifies the model considerably. However, this simplification may render the results irrelevant since the model then does not approximate the real system closely enough. At the very least, the authors need to carefully argue that their model remains valid for the questions they wish to address despite this simplification of heterotrophic respiration. Such an argument is presently missing from the paper.

I would like to see more clarity about how the generalized conclusions of the model depend on choices for specific constants. For example, the discussion in the paragraph beginning on line 121 only holds where k_c is significant. As k_c goes toward zero, self-shading decreases and NPP will continuously increase as C increases. The text is not clear on whether the k_c required to cause the self-shading induced decrease in $dNCP/dC$ above a certain C is reasonable. The paper discusses specific values for some of these constants later in section 2.5, but it seems as though the values of these constants affect earlier conclusions as well.

The simplification in the last part of equation 15 appears to remove the dependence of average mixed layer irradiance on the depth of the mixed layer. Equation 16, based on this simplification, demonstrates that only the respiration term is now sensitive to the mixed layer depth (MLD cancels from the first term). This seems to run counter to all the previous arguments that MLD is important to integrated NPP values.

[Printer-friendly version](#)[Discussion paper](#)

Lines 51-56: The discussion of attribution of these patterns seems too limited. Low NCP at high temperatures could be primarily a function of a tendency toward increased stratification and nutrient limitation in warm waters. Additionally, deep mixed layers can bias the O₂/Ar method low if entrainment of deeper waters brings low oxygen into the mixed layer.

Line 82: “light” attenuation coefficient rather than “diffusion” attenuation coefficient?

Lines 113-120 and following paragraph: This section is unclear in places. Figure 2 could be actively discussed to demonstrate why $dNCP/dC$ asymptotes at $-r^*MLD$ through comparison of the production and respiration terms on the right side of Figure 2a where the production term becomes stable. I spent a long time thinking about this, so the authors could really lead the reader through these arguments better. The text implies in places that $dNCP/dC$ always decreases with increasing C (lines 113-114), but this is only the case at C larger than C^* .

Lines 138-140: the statement here that integrated NCP is maximized when the MLD is below the compensation depth seems contrary to the schematic representation of the system in Figure 1a vs. 1b where the integrated NCP is maximized at the compensation depth.

Line 163: Why the MLD should satisfy the given conditions are not clear here until Line 171, where the authors state that they have chosen not to consider other possibilities.

Equations 20a and 20b: These are written as simple proportionalities here, but later treated as though the proportional sign is replaced with an equal sign. It seems like there should be an additional constant.

Section 2.5: Where specific values or ranges of values are chosen for model constants, it would be helpful to list these in the table defining notation.

Line 196: It's unclear why data could be below the theoretical line due to light limitation, when the theoretical line is specifically modeled to include light limitation.

[Printer-friendly version](#)[Discussion paper](#)

Model-data differences are difficult to clearly discern in Figure 3b. Perhaps it would be useful to directly plot model-data differences in a third panel. That the NCP* model performs poorly in warm deep mixed layers (as stated on lines 210-211) cannot be clearly seen in the figure.

Line 281: The text discusses discrepancies between predicted and observed NCP*. However, only NCP can be observed, not NCP*.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-259>, 2017.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

