

Submission to EGU—Biogeosciences: Answer to reviewers (round 2)

We would like to express our sincere gratitude to the reviewers for their insightful comments. We apologize for the delay in our response, which was due to a pending communication from collaborators. Please find below our detailed responses to the minor review points.

- **Reviewer 1:**

I would like to thank the authors for the revisions made following the first round of comments. The discussion has been improved and is now clearer and more compelling. I have noted a few minor revisions and typographical errors, which are listed below:

L.319 stating for starting

Done.

L.320 phytoplankton decline for phytoplankton declines

Done.

L.354 how do you define high values of CDOM?

We clarified what we consider in this study as high CDOM concentration values in this study as follows (L358): “... ($CDOM > 1.3 \text{ mmol C m}^{-3}$ i.e. $a_{CDOM}(440) > 0.5 \text{ m}^{-1}$; Matsuoka et al., 2012”.

L.399-400 The interpretation suggesting a “match-mismatch” with zooplankton is conceptually unclear in this context, given that zooplankton seems implemented as a general closure term in the model. If there is no explicit representation of zooplankton dynamics (e.g., no seasonal or phenological variability), the notion of temporal decoupling between phytoplankton and zooplankton seems inappropriate. I would suggest rephrasing this sentence to better reflect the limitations imposed by the closure term formulation — for example, by discussing how the imposed grazing pressure might artificially lower Chl a levels after the bloom, without implying an actual mismatch.

We thank the reviewer for pointing out this ambiguity. As stated in the supplementary text S1 (L9) and also now in the methods (L77–79), zooplankton is explicitly simulated in our model by two functional types and transported in the same manner as phytoplankton (as an explicit tracer). We removed the statement “*being a general closure term in the mode.*” to avoid any confusion.

Throughout the manuscript, “Chl” is used to refer to chlorophyll a. For clarity and to avoid ambiguity with other chlorophyll types, I recommend consistently using “Chl a” or “Chl a”,

which is the standard abbreviation for chlorophyll a in the oceanographic and biogeochemical literature.

Done.

There is an inconsistency in the manuscript regarding the spelling of “sea ice” — it appears both as “sea ice” and “sea-ice”. I recommend choosing one form and using it consistently throughout the text. Unless used as a compound adjective (e.g., “sea-ice concentration”), the unhyphenated form “sea ice” is generally preferred.

Done.

- **Reviewer 2:**

In my view the comments of both reviewers and editor were well addressed by the authors. I would therefore recommend the paper for publication. A few minor points from reading the manuscript again:

L11 “..(IPCC, 2023)..” I would cite the specific chapter.

Done: “Section 2: Current Status and Trends”

L14 “..or roughly 7% of the global-ocean sink..”

There are more recent more comprehensive observation and model based estimates for the global coastal ocean CO₂ sink:

Dai, Minhan, et al. "Carbon fluxes in the coastal ocean: synthesis, boundary processes, and future trends." Annual Review of Earth and Planetary Sciences 50.1 (2022): 593-626.

Resplandy, L., Hogikyan, A., Müller, J. D., Najjar, R. G., Bange, H. W., Bianchi, D., et al. (2024). A synthesis of global coastal ocean greenhouse gas fluxes. Global Biogeochemical Cycles, 38, e2023GB007803. <https://doi.org/10.1029/2023GB007803>

The introduction statement L14 refers to the entire Arctic Ocean contribution compared to the global CO₂ flux estimate, while the reference suggested by the reviewer focuses on the coastal ocean. Accordingly, we added the following coastal-specific statement to the sentence (L14-15): “*When focusing on coastal regions, the AO contribution constitutes up to 46% of the global sink (Dai, et al. 2022).*”

L65 "Here" -> Here,

Done.

L89 “..the terrestrial dissolved organic matter pool is not constrained by a constant C:N:P

ratio." -> The terrestrial dissolved organic matter pools are not constrained by a fixed stoichiometric ratio (such as in other studies?).

We added reference to the text as follows (L91): "... *is not constrained by a constant C:N:P ratio (such as Terhaar et al., 2019; Gibson et al., 2022; Bertin et al., 2023).*"

L100 Maybe state the CDOM pools are constrained by fixed ratio due to lack of data, and the assumption would be somewhat uncertain?

We added the following clarification at the end of the sentence (L102–103): "... — *Redfield ratio is used due to a lack of data regarding CDOM photodegradation products*".

L106 "...un-published data from Nunataryuk field campaign.."
Potentially add the relevant data to supplementary information?

We contacted F. Joux, who is responsible for the unpublished data from the Nunataryuk field campaign. Since these results are still unpublished, we were not granted permission to include them in the supplementary materials. However, we have added the following reference from the Sentinel North Workshop (L110):

Tisserand, L., Lizotte, M., Forget, M.-H., Lozano, J.-C., Matsuoka, A., Stedmon, C., Babin, M., and Joux, F.: Degradation of terrigenous dissolved organic matter in Arctic coastal waters: Importance of the priming effect and identification of microbial actors, Sentinel North Scientific Meeting, November 2-4, 2021
https://issuu.com/sn683/docs/rssn2021_proceedings_resumes?fr=sNzEzNDQzNDQ5OTE