Review of “Climate pathways behind phytoplankton-induced atmospheric warming” by Asselot et al.

We would like to thank the referee for the very thoughtful and constructive comments.

In this study, the authors explore the sensitivity of globally-annually-averaged atmospheric CO2 concentrations to phytoplankton induced surface ocean heating. Phytoplankton heat the surface ocean by absorbing radiation, which then heats the surrounding seawater and leads to changes in heat and carbon transfer with the atmosphere. Predicting even the sign of the change can be difficult since multiple physical factors (including changes to circulation) and chemical factors (changes to solubility) are relevant. The authors disentangle the relative strength of phytoplankton-mediated heat versus carbon transfer for influencing atmospheric CO2 via a series of idealized experiments in which physical and chemical factors are controlled. They find that phytoplankton heat absorption has a stronger influence on carbon exchange than heat exchange.

The study promises to be an excellent contribution to the question of how important the inclusion of phytoplankton heat absorption in climate models might be. Adding new code to existing models requires effort, and this study suggests this functionality alters heat and carbon fluxes and atmospheric CO2 concentrations. In their idealized framework, the effect on atmospheric CO2 is only 9 ppm on global annual average, suggesting phytoplankton heat absorption is a minor contributor. However, as the authors point out, this estimate may be a lower bound in less idealized conditions or the real world.

The experimental setup is thoughtfully constructed. Overall, results are discussed appropriately, although I found some critical information to be missing both from the description of the experimental setup as well as in the analysis of the model results. The paper is presented clearly, though I would like to see a modest expansion of the conclusions section to bring together the various experiments (including the sensitivity tests) in a more meaningful way.
I have some specific comments that should be addressed to improve the clarity of the manuscript:

- Figure 1 shows that the carbonate counter-pump is simulated, but the details of this and its implications and effects on the results are never discussed. Please provide the reader with more detail on this aspect of the biophysical feedback on carbon exchange.

The production and export of $CaCO_3$ in the surface of the ocean is linked to the export POM via a spatially uniform value which is modified by a thermodynamically-based relationship with the calcite saturation rate. The dissolution of $CaCO_3$ is treated the same way to that of the remineralization of POM (Ridgwell et al., 2007). In our previous study (Asselot et al., 2021), we show that phytoplankton light absorption accelerates the remineralization of POC at the surface. Because the dissolution of $CaCO_3$ and the remineralization of POM are treated the same way, phytoplankton light absorption also accelerates the dissolution of $CaCO_3$ at the surface. The accelerated dissolution of $CaCO_3$ and the accelerated remineralization of POM lead to a shallower downward flux of organic matter. However, our previous study evidences that the shallower downward flux of organic matter explain only a small part of the climate system’s response (Asselot et al., 2021). Therefore the carbonate counter-pump has a small implication on our results.

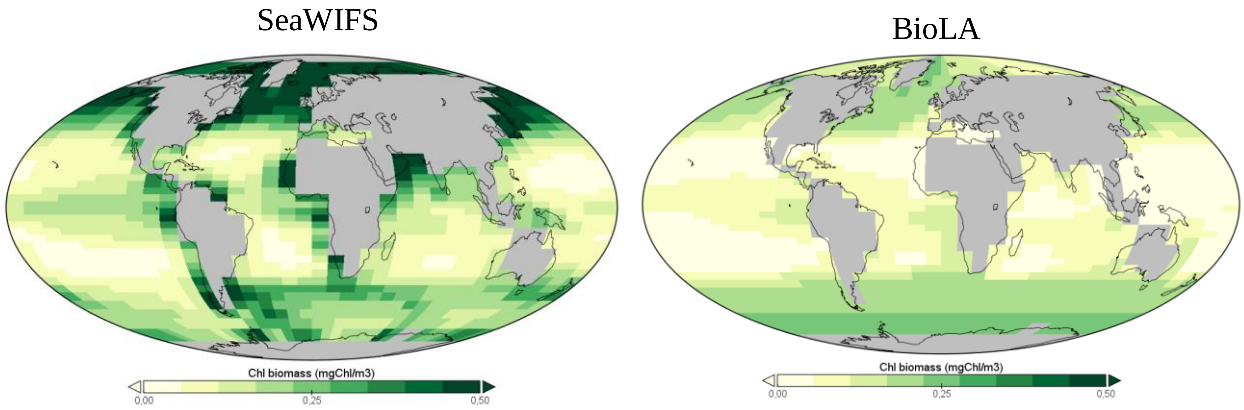
- Depending on model formulation, phytoplankton-mediated heating of the surface ocean could have an effect on the top-down control of zooplankton on primary production. Please discuss if and how this might affect results.

With our model setup, due to phytoplankton light absorption the surface phosphate concentration increases by 15.15%. The chlorophyll biomass increases by 12.36% while the zooplankton biomass increases by 12.83%. The increase in surface phosphate concentration is larger than the increase in chlorophyll biomass while the increases in chlorophyll and zooplankton biomasses are similar.

These results indicate that the top-down control of zooplankton on phytoplankton limits the increase of chlorophyll biomass and is dominant compared to the bottom-up control of nutrients. A sentence has been added to the manuscript.

- The absence of nitrogen cycling is not discussed but could have additional consequences not modelled here. If phytoplankton warm a low-oxygen region, and this causes additional oxygen consumption, then there might be additional denitrification. More denitrification would lead to more nitrogen fixation downstream, which might increase biomass regionally (a change in the spatial pattern of NPP, which affects the overlap of solubility vs biomass) and therefore any pathway sensitivity of atmospheric CO2.

We would like to indicate that even without a nitrogen cycle, the main patterns of chlorophyll biomass are well represented in our simulations (figures below). The model underestimates the magnitude of chlorophyll biomass in the upwelling regions and polar regions. We agree and including a nitrogen cycle might increases the chlorophyll biomass in low-oxygen regions which might increase the pathway sensitivity of atmospheric CO_2. These speculations have been added to the conclusion part.



- How is wind stress forcing treated in the model? If it is like Weaver et al. (2001) then there is a change in the wind stress with a change in global temperature. I could not find this information in the manuscript and it has important implications for the results.

The wind forcing in the model is treated differently as in Weaver et al. (2001). The current model setup uses an identical prescribed wind field for all simulations. The wind stress cannot change if the global temperature changes. These sentences have been added to the manuscript.

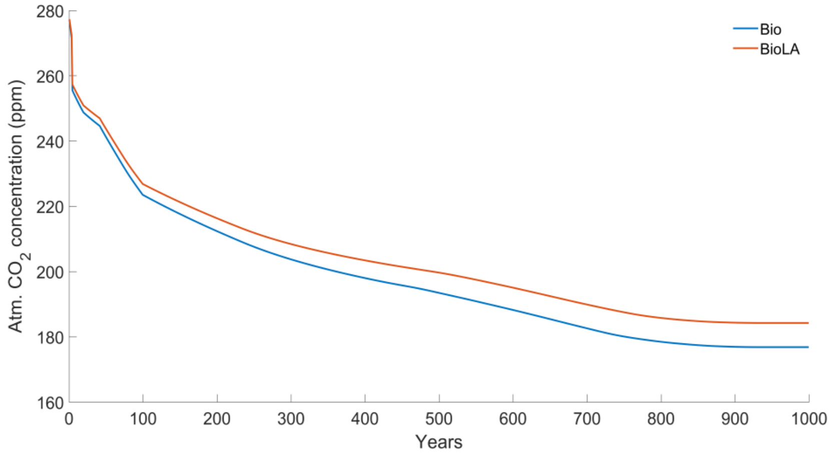
- What are the temperature dependencies in BIOGEM/ECOGEM? Is there an approximation of a microbial loop? Is remineralization accelerated by warming? I could not find this information. This is related also to point (2).

In the model, there is an approximation of the near-surface loop or "microbial loop" of carbon cycling. The heating of the ocean doesn't directly accelerate the remineralization rate; the remineralization is not temperature-dependent. However, phytoplankton light absorption affects the physical properties of the ocean (e.g. enhanced upward vertical velocity, deeper MLD) and the ecosystem structure (e.g. increases in chlorophyll and zooplankton biomasses) and thus increases the remineralization rate.

- L175-181: Doesn't the application of ECOGEM change the biogeochemical distributions in the model? If so, 1,000 additional years of spin-up might not be enough. Are the sensitivity tests applied after the 10,000 years + 1,000 years, or are they applied after 10,000 years (is the atmospheric CO2 prescribed for the 1,000 year ECOGEM spin-up)? Is atmospheric CO2 allowed to stabilize in all model simulations, or are all simulations only run 1,000 years?

More details on experimental setup would be useful here.

Including ECOGEM changes, indeed, the biogeochemical properties of the ocean. Due to the single layer atmospheric component, the non-seasonality and the non-representation of the land dynamics, running the simulations for 1,000 years is sufficient to achieve steady-state. Actually, the steady-state is reached after 800 years of simulations. After the 1,000 years simulations, the atmospheric CO_2 concentrations are stabilized. The atmospheric CO_2 concentrations only vary of 0.05% every decade in the last century of the simulations (see figure below). The sensitivity analyses are conducted after the 10,000 years + 1,000 years model runs. During the simulations (1,000 years long model run) the atmospheric CO_2 concentrations are not prescribed except for the simulation CARB because in this simulation the ocean and the atmosphere are not coupled.



- It is not clear whether Fe is a prognostic variable. If it is, then are there temperature effects on Fe solubility (and therefore, bioavailability)?

In the model, iron is a prognostic variable. Currently, there is no temperature dependence either on Fe solubility in dust or Fe scavenging from the water column.

- Section 2.3: Does sea ice have no influence on heat exchange? Please explain.

Heat is exchanged between the atmosphere, the ocean and sea-ice. In the section 2.3 we only detail the total heat flux (ocean + sea-ice) going into the atmosphere. Figure 4 gives an overview of the total heat flux going into the atmosphere for the different simulations. However, our results indicate that sea-ice have a small effect on the heat exchange. The heat exchange is mainly influenced by the ocean.

- Table 2 and Section 4.1. I don't see these as being very important to the main message of the paper and suggest moving them into the Appendix. However, it is interesting comparing Tables 2 & B1. Inclusion of seasonal cycling has more of an effect on SST than 40 ppm change in CO2! What is the effect of seasonal cycling on chlorophyll?

We moved Table 2 and Section 4.1 in the Appendix.
Including a seasonal cycle increases the chlorophyll biomass by 0.039 $mgChl/m^3$. This is due to the warmer ocean, favoring the growth of phytoplankton.

- Section 4.2 could also move to an Appendix. But, as mentioned elsewhere- is the wind forcing different across the main model experiments due to differences in SST anomaly from pre-industrial state?

Section 4.2 has been moved to the Appendix. The wind forcing is similar between all the simulations. It cannot be affected by the changes in SST or atmospheric temperature.

- Conclusions are missing some wider speculation as well as more discussion of model limitations. What would the authors expect if a land model were to be included in their simulations? What about sea ice influencing heat flux, or carbonate counter-pump effects or changes to micronutrient availability? Feedbacks in a transient state?

We speculate that including a land model will still lead to an increase in atmospheric temperature due to phytoplankton light absorption but the magnitude of changes might be smaller. This is mainly due to the uptake of CO_2 by the vegetation, decreasing the atmospheric CO_2 concentrations and thus resulting in smaller increase in atmospheric temperature. If a land model were to be included, the magnitude of changes reported would be smaller but the sign would stay the same.
The sea-ice can influence the heat flux going into the atmosphere (see Equation 3). Figure 4 shows the total heat flux (ocean + sea-ice) going into the atmosphere. These details are added to the manuscript. However, our simulations indicate that changes in sea-ice don't play an important role in changes in air-sea CO_2 and heat fluxes.
The dissolution of $CaCO_3$ at the surface is accelerated by phytoplankton light absorption. Yet, our previous study (Asselot et al., 2021) indicates that this process explains only a small part of the climate system's response. The carbonate counter-pump does not play a major role to explain our results. This explanation is added to section 5.2.1.
The model does not include any temperature effects on the Fe solubility. However, here, we speculate on the consequences of implementing a temperature-dependency of iron solubility. According to previous experiments, the solubility of iron decreases when the oceanic temperature increases (Liu and Millero, 2002). Phytoplankton light absorption increasing the oceanic temperature might therefore reduce the Fe solubility and therefore its bioavailability. As a consequence, the limitation of phytoplankton growth by iron would increase, reducing the greater chlorophyll biomass with to phytoplankton light absorption.

Finally, I have some minor suggestions for language:

L1: "in which ways", or "ways in which"?
Changed

L10: "…the freely evolving solubility of CO2…" (due to what? Is it going up or down?)
When the solubility of CO2 can evolve freely, the atmospheric temperature slightly decreases due to the slight decrease in air-sea heat flux.

L11: Some kind of summary sentence that gives the results context would be useful here.
Added

L20: "evidence supports"
Changed

L29: "Models of differing complexity…"
Changed

L61: "…as follows…"
Changed

L71: "…composed of…"
Changed

L75: "…the sensitivity of atmospheric CO2 is mainly explained…"
Changed

L119: remove “availability”
Changed

L119: “prey”
Changed

L119: Table A1 shows that phytoplankton are ~3X smaller than the zooplankton. Does this mean there is no zooplankton grazing in the model?
Zooplankton grazing is represented in the model, even if phytoplankton are ~3X smaller than zooplankton.

L125: “For simplicity…”
Changed

Eqn 1: Does sea ice not affect light attenuation? Why not? Does this mean there is biomass under sea ice?
In the model, sea-ice affects the heat fluxes but for simplification, it does not affect the light attenuation. As a consequence, phytoplankton can grow in grid cells covered by sea-ice.

L132: “…total chlorophyll concentration ..”
Changed

L142: “…is released in the form of…”
Changed

L151: “…received…”
Changed

Figure 1: There is no arrow between sea ice and anything else
We add the relevant arrow for our study. The arrow between sea-ice and outgassing of CO_2 is added to the figure.

Figure 5: Shouldn’t “Bio” look more like Figure 1 (with an arrow going from SAT to SST?) Plus, CO2 should be able to enter the ocean in this simulation? Same for BioLA.
For simplification, on Figure 5 we only kept the arrow that go to the atmospheric temperature. Figure 5 only shows how the atmospheric temperature can be affected by the CO_2 and SST.

L203: Is this only the biological pump? What about CaCO3?
We revise or sentence and changed “biological pump” by “biogeochemical pumps” to include the soft-tissue pump plus the carbonate pump. Thus $CaCO_3$ is included in the biogeochemical pumps.

L216: “…concentrations differ.”
Changed

L225: “…we ensure that the heat and CO2 interaction is negligible by …”
Changed

L227: “…analyses…”
Changed

L237: split into 2 sentences
Changed

L245: “Finally, the response of the surface atmospheric temperature due to changes in oceanic and atmospheric properties is studied”
Changed

L250: What is the temperature dependency that produces the shallower flux of OM due to warming from phytoplankton light absorption? (For those not familiar with your model).
We briefly summarize why the downward flux of organic matter is shallower with phytoplankton light absorption. This is due to: (1) A deeper mixed layer and therefore a more important mixing in the surface of the ocean (2) Enhanced upward vertical velocity, trapping more nutrients and organic matter at the surface of the ocean.

L251 (and elsewhere): “The chlorophyll biomass difference…” (Also applies to SST)
Changed

L264: remove “more important”
Changed

L 270: might this be due to the lack of an influence of sea ice on heat flux?
We added a sentence in the manuscript.

L 275/76: Is there a difference between HCorgSol and HCorg w.r.t. CaCO3 production?
The export production of CaCO3 is higher in HCorg compared to HCorgSol. This result does not change the main message of this paragraph because the CO_2-solubility is prescribed in the simulation HCorg, therefore the $CaCO_3$ production cannot affect the CO_2-solubility.

L 296/97/300: I think “more important” is not what is meant. “Larger” or “Greater”?
Changed

L 307: “…which increases…”
Changed

L 308-310: split into 2 sentences
Changed
L319: ”…heat flux, explaining…”
Changed

L330: replace “pointing out” with “which indicates”
Changed

L332: “to outer space”
Changed

L362: ”…, where the lower the humidity the higher the evaporation rate.”
Changed

L371: remove “indubitably”
Changed

L375: maybe not “clearly”, since I still have some questions about experimental setup and model assumptions beyond seasonality.
Changed

L388: ”…identical responses…”

Changed

L 390: “…smaller increase…”
Changed

Literature

Asselot R, Lunkeit F, Holden PB, Hense I. The relative importance of phytoplankton light absorption and ecosystem complexity in an Earth system model. *Journal of Advances in Modeling Earth Systems*. 2021;13(5):e2020MS002110.

Liu X, Millero FJ. The solubility of iron in seawater. Marine Chemistry. 2002 Jan 1;77(1):43-54.

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Ward BA, Dutkiewicz S, Jahn O, Follows MJ. A size-structured food-web model for the global ocean. *Limnology and Oceanography*. 2012; 57(6):1877-91.

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