

Review of the paper “A missing link in the carbon cycle: phytoplankton light absorption under RCP scenarios”

General comments

I have completed the review of the manuscript egosphere-2023-921. The manuscript shows some interesting results, somehow scientifically soundings. At the same time, I found that the manuscript has several issues that the Authors should address before a possible publication of their work. Thus, I asked major revisions for this work.

We would like to thank the reviewer for their very interesting comments. The review helped to improve the quality of the manuscript and the science behind it. Our responses are in blue, with edits to the manuscript in red.

Major comments

1. The manuscript shows several typos. See for example “phytopankton” at line 8. Please consider a strong editing of text.

We used an online editing tool to remove the typos in the revised manuscript.

2. There is a lack in explaining the purpose and the methodology followed in the study.

At the end of the introduction we added the text to clarify:

“The purpose of this study is to better understand how phytoplankton light absorption will be affected by anthropogenic climate change via changes in phytoplankton biomass and distribution. To address this question, we performed simulations with and without phytoplankton light absorption in experiments with prescribed atmospheric CO₂ emissions. We are interested in long-term climate effects and so we applied the intermediate complexity Earth system model EcoGEnIE (Ward et al., 2018). We force the model with atmospheric CO₂ emissions out to 2500 following the four Extended Representative Concentration Pathways (RCP) scenarios used by the Intergovernmental Panel on Climate Change (IPCC) for their Fifth Assessment Report (Moss et al., 2010).”

3. Along the text the Authors mention the “primary production”. Do you mean “net primary production” (gross pp minus respiration) or the gross pp? please specify since they are different things that can lead to a different interpretation of your outcomes.

In the manuscript we referred to net primary production. We revised the manuscript accordingly.

4. There is a lack in the manuscript in the description of the modeling tool and forcing adopted in the simulations. For example, I do not understand why using such old values for wind forcing (Trenberth; 1989) since you run long

term simulations covered several centuries, eventually covered by CMIP models.

We apply the model with an EMBM atmosphere for computational efficiency and simplicity. The wind forcing of Trenberth et al. (1989) is the default wind forcing in this configuration of EcoGENIE. The EMBM is a single layer diffusive model and is largely insensitive to the details of the wind field forcing, so that there would be little benefit in applying e.g. time varying wind fields derived from high complexity models. A full description of the model is beyond the scope of this paper but it is described in detail in Ward et al (2018) and references therein.

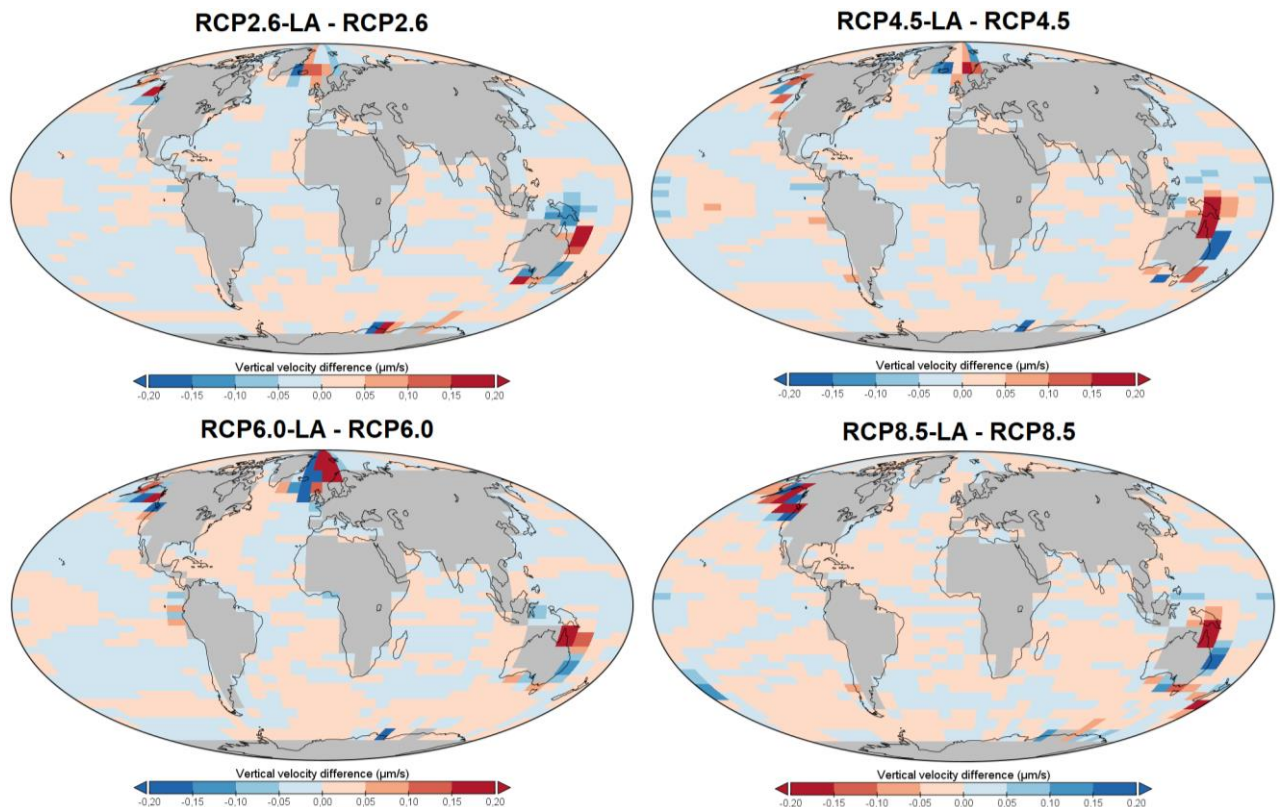
5. I'm puzzled about the fact that chl-a is not transported by ocean currents. Is it your model result? Is it an assumption of the analysis or a constrain in your numerical simulations? Please explain better. Phytoplankton (and chl-a) could be considered a passive tracer (as it is in many coupled models). Thus, in marine environment it can transported by advection or diffusion processes.

We clarify with the following addition in the section "Ecosystem community component":

"Living matter is not subject to ocean transport. Communication between biological communities only occurs through the advection and diffusion of inorganic and non-living organic matter. This approximation is justified by the coarse (~1000 km) model resolution and limited transport range of living matter, so the rate of transport between grid cells is slow in relation to the net growth rates of the plankton community (Ward et al 2018)."

6. The explanation provided by the Authors related to the increase in the vertical velocity (see also my comment on Appendix D) looks to me weird (maybe I'm missing something in their reasoning and so please help me to understand). If someone warms/cools the upper/bottom of the water column I would expect an increase of the vertical stratification and thus lower vertical transport and vertical mixing (as predicted by several studies discussing the future climate projections). The Authors talk about an increase in the vertical velocity because of the difference in temperature. I do not think this is correct. Please explain better this point.

We thank the reviewer for this interesting discussion. We agree that a warming of the surface ocean would drive an increase of the stratification and that our arguments to the contrary were not robust. We note that there is a suggestion of increased upwelling in the Pacific coast of the Americas and the Atlantic coast of Africa. However, these changes are rather modest and noisy. In view of this we have decided to remove mention of this potential mechanism. Reduced export efficiency of organic matter and enhanced remineralization at the ocean surface are sufficient to explain the patterns of chlorophyll change, and we have restricted our argument to this mechanism. For the reviewer's interest, we provide 2D maps of change in vertical velocity driven by phytoplankton light absorption in the RCPs, illustrating the increased (but weak) upwelling signal along the Pacific coast of the Americas and the Atlantic coast of Africa.



Specific comments

Line 2: I would say influence not impact.

Changed

Line 8-12: Please explain better how the light absorption would weaken the carbon pump.

We rephrased to:

“Under all RCP scenarios, our results indicate that phytoplankton light absorption leads to a shallower remineralization of organic matter and a reduced export efficiency, weakening the biological carbon pump.”

Line 16-19: This part is not very clear and should go at the end of the introduction as purpose of the work.

We removed these two sentences at the beginning of the introduction and had one sentence at the end of the introduction to better explain the purpose of this work.

The sentence added is:

“The purpose of this study is to better understand how phytoplankton light absorption will be affected by anthropogenic climate change via changes in phytoplankton biomass and distribution.”

Line 25-35. There is a dependence of the projections for the net primary production on the parametrization adopted and how they are influenced by the temperature. In

the Mediterranean sea there is an extensive review on this topic by Richon et al., 2019 and Reale et al., 2022. Please consider to add a sentence about.

We thank the reviewer for pointing out these two interesting studies. We add three sentences in the introduction:

“On a regional scale, projected changes in primary production are also uncertain. For instance, in the Mediterranean Sea, Richon et al. (2019) show a decline in net primary production of 10% in the 2090s under the high-emission SRES-A2 scenario. However, in the same basin, Reale et al. (2022) demonstrate that, under the RCP4.5 and RCP8.5 scenarios, the net primary production increase is greater than 10 gC/m²/yr by the end of the 21st century. These conflicting results come from the different parameterizations adopted which exert differing influences of temperatures on simulated net primary production.”

Line 33-35: how? Please explain

In the revised version of the manuscript, we removed this sentence and replaced it by the sentences describing the project changes in net primary production in the Mediterranean Sea (see comment above).

Line 38 and line 55: On what? Please explain

We meant on the oceanic temperature. We revised the sentences accordingly.

Line 68 e Fig.2: The RCP scenarios do not “describe possible future climate system” but hypothetical temporal evolution of greenhouse gases emission in the atmosphere. Please correct this statement and the caption.

We rephrase line 68 by:

“The RCP scenarios include the temporal evolution of greenhouse gas emissions into the atmosphere (Moss et al., 2010)”.

We rephrase the caption of Fig. 2 by:

“Atmospheric CO₂ emissions following the RCP scenarios. (a) Historical and scenarios of future CO₂ emissions over time (GtC/yr).”

Line 81-82-89: What do you mean with “association”, “intermediate” and “related to climate processes”?

With “association” we mean that EcoGENIE is a union/coupling between the new ECOGEM component and the previous model cGENIE. We rephrase. According to Claussen et al. (2002), ESM of intermediate complexity are designed to represent the Earth system, excluding the interactions between humans and nature. These models were created to close the gap between complex coupled general circulation models (CGCMs; designed to represent as much climate feedbacks as possible and computationally expensive) and simplistic models (designed to study the plausibility of climate processes and often represent only one component of the climate system).

We rephrase and replace “related to climate process” by “represent climate processes”.

Figure: is there a coupler managing the exchange of fields (arrows) among the components of the modeling tools? Please specify

The coupling is now summarised in the caption of Fig. 1

“GENIE is controlled by a bespoke coupling manager which was developed for user-friendly modularity and flexibility, so that, for instance the EMBM atmosphere can be replaced with a fully dynamic 3D atmosphere PLASIM (Holden et al 2016) via a single switch in the model configuration file.”

Line 101: I see that the purpose of the work is not to validate your simulations but please add some quantitative information to your discussion (under/over estimation is too generic).

We modify the text as follow:

“In contrast, Ridgwell et al. (2007) indicate that the low-latitude upwelling in the Western Equatorial Pacific and Equatorial Indian Ocean give an excess of phosphate of 0.5 $\mu\text{mol/kg}$ compared to observations (Conkright and Levitus, 2002).”

Line 123: Why do not you consider Nitrate? Please specify.

We do not consider nitrate here because our model does not have an explicit representation of the nitrate cycle, but represent it through the Redfield ratio. We add the sentence:

“Similar to Asselot et al. (2021), we do not explicitly consider nitrate (NO_3) but approximate it through the N:P Redfield ratio of 16:1 (Ridgwell 2007).”

Line 145: DIC as nutrient sounds to me weird since it involves different chemical species (CO_2aq , HCO_3 , CO_3 and so on). Could you please add some references or explain better this point?

We thank the reviewer for this point. We changed “nutrients” to “inorganic resources”.

Line 169: What are the criteria to choose the sixth layer as limit for light?

We chose the sixth layer as limit for light because it represents the base of the euphotic or sunlight zone (about 200 meters). We add this explanation in the manuscript.

Line 196: Why just one year (2050) instead of the entire period?

The scope of this study is to understand the long-term effect of phytoplankton light absorption on the climate system under long-term anthropogenic CO_2 emissions. The climate system needs to adjust to these emissions, thus we compare only the year 2500, when the climate system has mostly responded to the CO_2 forcing.

Studying the whole period would have implied studying transient effects, which is not the aim of our study.

Line 200: It would be nice to see the spatial distribution of these differences that could explain the differences among the different scenarios instead of a single value that is meaningless

We agree that it would be more informative to have the spatial distribution of these differences but Zickfeld et al. (2013) do not give any map showing these differences. The authors only give the global mean values of these differences. We note there are substantial uncertainties associated with the global warming response, and, as is appropriate for a model of intermediate complexity, we validate the model's climate carbon-cycle response to CO₂ emissions using this large-scale metric.

Line 226: I do not think that chl-a is a climate variable

We replaced "climate variable" by "climate carbon-cycle variable".

Line 229: How? Please explain

We rephrase by:

"This increase is due to the increased global phosphate concentrations (Appendix D1) which are driven by a reduced export efficiency of organic matter and enhanced remineralization at the ocean surface (Table 2)."

Line 230-235: see my major point 6

We refer the reviewer to our answer to major point 6. We removed the argumentation with the enhanced vertical velocity.

Line 266: what do you mean with underestimation of the oceanic circulation?

We apologize for the confusion here and changed the argumentation by:

"The polar regions experience the lowest changes in SST because temperatures are buffered by latent heat through melting sea-ice and remain close to freezing."

Line 269: What do you mean "The missing....model setup". Please explain

For clarification, we combined the two sentences and rephrase by:

"The differing spatial patterns between chlorophyll and SST can be explained by the fact that short-lived chlorophyll is not subject to transport, while (conserved) physical quantities, such as heat, are transported by oceanic currents."

Line 274-276: Please rephrase.

We rephrase by:

"The atmospheric CO₂ concentrations in our simulations do not match the atmospheric concentrations of Meinshausen et al. (2011) in 2500. This is because our version of the model, with light penetrating until the sixth oceanic layer, has been

tuned to get reasonable net primary production and nutrient fields but not to get future atmospheric CO2 concentrations”

Line 342-342: Not clear. Please rephrase

We rephrase by:

“Our model setup allow for light and primary production until the sixth oceanic layer and this configuration has not been tuned to match projected atmospheric CO2 concentrations, leading to an underestimation of the latter.”

Appendix D: It would be better to have a map to show the global distribution of this quantity. Maybe the results in the Chilean area could be associated with La Nina/El nino pattern. Did you check that?

We removed this argument on vertical upwelling as detailed above, but have included the suggested figure in this response for the reviewer’s interest.