Comment on esd-2021-91

Anonymous Referee #2

Referee comment on "A missing link in the carbon cycle: phytoplankton light absorption"
by Rémy Asselot et al., Earth Syst. Dynam. Discuss.,
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Phytoplankton in the ocean absorbs light, and the amount of phytoplankton exists changes the degree of shortwave penetration into the ocean. Since shortwave radiation raises water temperature, there may be differences in water temperature, primary production, and climate change if phytoplankton light absorption is taken into account in water temperature fluctuations in a model or not. Using an Earth system model of intermediate complexity, the authors conducted future scenario experiments with and without phytoplankton light absorption to evaluate how phytoplankton light absorption would affect primary production and climate change. I believe that this study points out what has been lacking in conventional climate research and provides important implications for future model development. However, before accepting this manuscript, I think that there are some aspects of the authors' analysis that could be improved as follows. I may have misread some things, and I do not think the authors need to follow all my comments, but I hope that the following comments will help the authors to improve their manuscript.

We would like to thank the referee for the constructive suggestions.

1.
Model validity
l.154 "Figure 3 shows that our increase in SAT are in agreement with Zickfeld et al. (2013)"
In order to show the validity of the model, the authors compared the change in SAT with a previous study, but what about the distribution of SST, nutrients, chlorophyll concentration, etc.? Since these are directly relevant to this study, I think it would be better to compare climatic values, etc. with observations to see if the model reproduces the approximate distribution.

In a previous study (Asselot et al., 2021, JAMES) we compare our modelled AMOC, surface chlorophyll concentration, primary production, export production and phosphate concentration with observations. We show that our model reproduces suitable patterns and distributions for these climate variables.

Mechanism enhancing upwelling
l.180 "an enhanced vertical velocity which is triggered by changes in the oceanic heat budget"
In this model, the shortwave penetrates down to the sixth layer at 221.84 m, but the authors insisted the upwelling at 326 m depth is enhanced due to the phytoplankton light absorption (l.180). It is not clear why the upwelling is enhanced.

In our model setup, phytoplankton light absorption warms the whole water column. As a consequence, more energy is stored in the ocean, increasing the oceanic velocity and thus the vertical velocity in the upwelling regions. This explanation is added to the revised manuscript.

l.235 “this biogeophysical mechanisms enhances the upward vertical velocity”
I could not understand why that would happen.

Phytoplankton light absorption increases the oceanic temperature along the whole water column. As a consequence, more energy is stored in the ocean, enhancing the upward vertical velocity.

In addition, the concentration of chlorophyll depends not only on nutrients but also on water temperature and shortwave radiation. It would be good to have a discussion of how these factors might have affected the results.

Indeed, the concentration of chlorophyll depends on nutrients, temperature and shortwave radiation. However, phytoplankton light absorption doesn’t affect incoming shortwave radiation thus this factor doesn’t affect our results. Temperature affects the chlorophyll concentration but, on a global scale, it only affects phytoplankton growth for the simulations following the RCP8.5 scenario. Similar discussion is added to the “synthesis” paragraph.

Difference from the concentration runs
l.213 “However, in Asselot et al. (2021a) we do not prescribe any CO2 emissions, neglecting their effect on the atmospheric CO2 concentration.”
It is ambiguous why the increase in atmospheric CO2 concentration of the emission driven runs is different from that of the concentration-driven runs, so it would be better to add some analysis. For example, by considering the phytoplankton light absorption, can we estimate how the water temperature changes, how it changes the carbon concentration in the ocean and the atmosphere-ocean carbon flux, and how it changes the concentration of the atmospheric CO2? If the authors could figure this out, it would clarify the difference from concentration driven experiments and the importance of this study.

With this sentence, we compare the results of Asselot et al. (2021) where CO2-emissions are not prescribed versus our actual results where CO2-emissions are prescribed. In both studies we analyse how phytoplankton light absorption affects the water temperature and its consequences on the carbon cycle. In our actual study, the changes in chlorophyll biomass, SST, SAT and atmospheric CO2 concentrations are higher than in Asselot et al. (2021), except under the RCP8.5 scenario, which is a particular case. The only difference between the two studies is the prescribed CO2-emissions in the actual study, explaining the higher changes in atmospheric CO2 concentration in our actual study.

Results and analysis on RCP8.5 runs
l.249–251 “This is due to the model setup where a SST higher than 20C limits phytoplankton growth. This threshold is only exceeded for the simulations RCP8.5-LA and RCP8.5 (Appendix D1), therefore phytoplankton growth is limited by the ocean temperature in these two simulations (Figure 10).”
First, I think that this 20 degreeC limit is arbitrary (part of tuning), so I am not confident that the results of the RCP8.5 experiments relying on it are correct. Even in the current climate, water temperatures are above 20 degreeC in equatorial regions, etc. Is this high SST in these regions not reproduced in the model? I do not believe this is the case, so the authors may want to reconsider their analysis for RCP8.5 runs.

The limitation of phytoplankton growth is set to 20°C because most experimentally determined rates are done at 20°C. Additionally, several experiments with different phytoplankton communities indicate that the maximum growth rate is reached at 20°C and exceeding this value limits phytoplankton growth (e.g. Goldman, 1977; Rhee and Gotham, 1981). However, in a warmer world, adaptation may happen and this point is added in the discussion.
The high SST in tropical regions is reproduced and the 20°C threshold is exceeded in all the simulations. But only in the simulations *RCP8.5-LA* and *RCP8.5* enough surface grid cells exceed 20°C to affect the global increase in chlorophyll biomass.
We rephrase our sentence.

Figure 10
How was this calculated? Was it calculated from the global-mean SST? If so (see Table D1), I do not understand why the temperature limitation is calculated from the global mean SST, since the growth rate of phytoplankton is determined by the water temperature and other conditions at each location.

Indeed, the values of Figure 10 were calculated from the global-mean SST. We took the global-mean SST to calculate the mean temperature limitation around the global ocean. We focus on global estimates rather than local estimates due to the limitations of EcoGEnIE which is a model of intermediate complexity.

As mentioned above, there are some points that need to be improved regarding the analysis of the results. Hopefully the authors will revise the manuscript to make it more convincing.

Some minor comments are listed below. I hope they will be useful for the authors to revise their manuscript.

Minor comments:

Section 2.1
There is no description of the emissions from 2100 to 2250, so it would be good to describe them briefly.

We add a sentence to describe the emissions between 2100 and 2250.

l.75 “The Earth system mode (ESM)l”
This should be “The Earth system model (ESM)”

Changed.

l.78–82
I think the authors want to show how much EcoGEnIE was used, and for that, it would be better to use past tense or present perfect tense.

We re-phrase our sentences.

Section 2.2.4
It would be easier to understand if the previous method without phytoplankton light absorption is also described and compared.

In the previous version of the model without phytoplankton light absorption, light was only absorbed by phytoplankton. In our model version, a new light scheme is implemented where the absorbed light by phytoplankton is converted into heat and is able to affect the oceanic temperature.
Implementing the new light scheme with phytoplankton light absorption increases the surface chlorophyll concentration, the SST, the atmospheric CO2 concentrations and the atmospheric temperature. For a more detailed comparison of the two model version, we refer to Asselot et al. (2021, JAMES).

l.136 “dT/dt denotes the temperature changes”
The dT/dt term here is the water temperature change only associated with radiative heating, so I think it should be clearly stated as such.

Changed.

l.139 “(Asselot et al., 2021a)”
This should be “Asselot et al. (2021a)”

Changed.

l.143 “The spin-up is run with a constant pre-industrial atmospheric CO2 concentration of 278 ppm.”
l.154 “In the pre-industrial simulation, the atmospheric CO2 concentration is constant and set to 278 ppm.”
I thought the model was emission driven, but do these sentences mean that it is concentration driven during spin-up? Instead of a spin-up with zero emissions? If so, when is the timing of changing from concentration driven to emission driven? Was there any shock at the timing when it became emission driven?

The atmospheric CO2 concentration is only prescribed for the spin-up phase. The spin-up in run for 10,000 years and our simulations are run for 736 years (sorry, we accidentally wrote 737 years) with emission driven atmospheric CO2 concentration. Looking at our outputs, there was no shock when we changed from concentration-driven to emission-driven atmospheric CO2 concentrations.

l.144 “after the spin-up for 737 years-long, since the CO2 emissions data are only available for this time span”
Does this 737 years refer to the past 737 years? Or does it include future data? I think it is unclear how the spin-up was done after ECOGEM was switched on, so it would be good if the authors could write it clearly.

The 736 years-long simulations represent past and future data, from the year 1765 to the year 2500. This information is added to the manuscript.

l.147 “In total, we run 8 similations following the RCP scenarios”
Before the RCP scenarios, there should be a historical run (Figure 1), but the description of the method for the historical run is ambiguous.

We re-phrase the sentence

l.176 “This pronounced chlorophyll increase is due to the coarse grid resolution”
Why does chlorophyll increase if the grid resolution is coarse?

The dimension of the grid doesn’t affect the chlorophyll concentration. Rather, the sharp patterns of the chlorophyll concentrations are explained by the coarse grid resolution.
We revise our sentence.

l.178–179 “the upwelling and mid-latitude regions” / “the subtropical gyres”
The upwelling, mid-latitude, and subtropical regions can be overlapping, so the meaning of the sentence is unclear.

We revise our sentence.

l.186 “the SST is the zonally-averaged temperature from the surface to 29 m depth”
“vertically-averaged”?

Changed.

l.204 “the expected atmospheric CO2 concentrations in 2500 under the RCP scenarios are not reached.”
It is difficult to understand for me, I am glad if the authors rewrite it.

Changed.

l.205 “EcoGEnIE has not been tuned yet.”
Are the authors saying that more tuning is needed? Since the concentrations in emission driven runs are the result of various balances and it is difficult to adjust the concentrations in the model to reality, why not simply state the fact that atmospheric CO2 concentrations are low here?

Yes we suggest that our version of the model with primary production allowed until the sixth layer of the model should be tuned to match, at least the pre-industrial CO2 level in all the simulations.

l.217 “The global changes in oceanic and atmospheric properties due to phytoplankton light absorption lead to an increase of surface atmospheric temperature (SAT) (Figure 8).”
I believe that the importance of plankton light absorption can be conveyed to readers by clearly describing the mechanism of how plankton light absorption affects SAT through the changes in ocean and atmosphere conditions.

We add a sentence to explain the main mechanism changing the atmospheric temperature.

l.234 “increasing the remineralization and thus the nutrient concentrations at the ocean surface”
Since there has been no mention of remineralization before, it is difficult to follow the discussion.

In the revised version of the manuscript, the remineralization process is mentioned earlier.

l.241 “the sea-air CO2 flux”
This is not clear whether the authors are referring to upward flux or ocean carbon uptake, so please specify.

Changed.

l.259 “the model has not been tuned in this configuration yet.”
See comments on l.205.

We suggest tuning our version of the model to have match at least pre-industrial levels.

l.286 “phytoplankton-induced global warming”
I do not think phytoplankton will induce global warming. I can see how the light absorption of phytoplankton can affect the progress of global warming, and in my opinion, the word “induced” is not appropriate here.

We rephrase our sentence

Figure 3
Does this figure represent global mean SAT changes?

It represents the global mean SAT changes between the periods 1986-2005 and 2281-2300.

Figure 4 caption “Zonally”
“Globally”?

Changed

Reference

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

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