Asselot et al. – A missing link in the carbon cycle: phytoplankton light absorption.

Summary

Phytoplankton light absorption is the process by which incident short wave radiation is absorbed by phytoplankton in the ocean's surface waters. This process increases the amount of heat that is absorbed in surface layers while decreasing the depth to which this radiation penetrates. Because phytoplankton also decrease the albedo of the ocean, the net effect is that more shortwave radiation is absorbed in the ocean, specifically the near-surface waters, when more phytoplankton are present.

Asselot et al. present experiments that explore the effect of phytoplankton light absorption on key climate variables of societal importance: ocean surface temperatures, phytoplankton biomass, atmospheric CO2 concentrations, and surface atmospheric temperatures. They do so on the global scale. To do so, they use an Earth System Model of Intermediate Complexity (an ESMIC) called the EcoGEnIE. The ocean-biogeochemical-ecosystem components of the model are equipped with phytoplankton light absorption, such that experiments can be performed with this process turned on or off.

Asselot et al. acknowledge that other studies have already undertaken modelling experiments that assess the effect of phytoplankton light absorption under climate change scenarios. The novelty of their study is therefore in (1) its use of multiple, extended RCP scenarios (2.6, 4.5, 6.0 and 8.5), and (2) its insights into the effect on atmospheric CO2 concentrations since they use emissions, rather than specifying the atmospheric CO2 concentration.

The main findings of this study are:

• That phytoplankton light absorption increases surface temperatures and phytoplankton biomass.
• That the effects are strongest under the weaker global warming scenarios.
• That the warming of the surface ocean weakens the ocean's ability to absorb CO2, which increases atmospheric CO2 and thereby increases surface temperatures in a positive feedback response.

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

Major comments

This paper is presented simply, and in that sense the authors are honest with their results. They do not oversell the lessons. However, this suggests that the mechanisms involved are very simple, and actually I am left wanting more. I thus have three major concerns with this paper as it stands. All relate to the discussion of their results.

The three main concerns of the referee will be addressed in the following paragraphs.

First, the authors do not explain the mechanisms (physical and/or biogeochemical) that underly the relatively greater chlorophyll biomass when phytoplankton light absorption is included. As far as I can tell from the work, they speculate about the mechanism, but do not definitively show it. If upwelling increases, why? I think the paper would be strongly improved with a more concrete explanation of this process. I have suggested that the authors undertake some 1D modelling work in my specific comments below, but if they can identify the mechanism with output from the 3D model than that is also fine. Bottom line, the reader needs a clearer more convincing explanation.

The higher surface chlorophyll biomass with phytoplankton light absorption is due to two different mechanisms. First, the dynamics associated with phytoplankton light absorption leads to a weaker biological pump, leading to more labile inorganic matter (e.g. DIC) at the surface of the ocean (Asselot et al., 2021, JAMES). As a consequence, the remineralization is enhanced and the nutrient concentrations increase at the surface. Second, phytoplankton light absorption increases the oceanic temperature along the whole water column, leading to more energy being stored in the ocean. As a consequence, upward vertical velocity is enhanced in the upwelling and mid-latitudes regions. This physical process brings more nutrients at the ocean surface. All in all, these two mechanisms explain the higher surface chlorophyll concentration.
Two sentences are added in the "surface chlorophyll biomass" section.

Second, the real novelty of this study is in the feedback on CO2 concentration, as the authors state in the introduction. No other modelling study (to theirs and my knowledge) has specifically targeted this feedback. However, the consequences for atmospheric CO2 are reported but the mechanisms are not explained. Why exactly does phytoplankton light absorption increase atmospheric CO2? What I'm really asking here is what portion of the atmospheric CO2 increase can be apportioned to a decrease in the solubility pump, a decrease in the biological pump or a decrease in the carbonate pump (assuming this is included)? I have little doubt that the increase in surface temperature is the main culprit, but quantifying these terms would significantly improve the paper.

In a previous study (Asselot et al., 2022); we estimate that the decrease in CO2-solubility due to warmer SST via phytoplankton light absorption enhances the air-sea CO2 fluxes by roughly 10%. The changes in the other mechanisms such as the biological pump and the carbonate pump only increase the air-sea CO2 fluxes by <1%. We find by far that the solubility pump has the largest effect on the increase of atmospheric CO2 concentrations.
A sentence is added to the "synthesis" section.

Third, the discussion is very short. There is no discussion of which models include this feedback as part of their architecture and which do not. There is little discussion of observational studies that see this in the real ocean, nor a discussion of whether the mechanisms that cause a relative increase in chlorophyll biomass are realistic. There is little discussion regarding how phytoplankton community composition changes might affect the magnitude of phytoplankton light absorption in the future (i.e. cyanobacteria becoming dominant). There is also no acknowledgement of the uncertainty in future primary production, which may increase, remain stable or decrease depending on the model and region of interest. Would phytoplankton light absorption cause substantially greater greenhouse warming if global warming was coincident with stable or increasing global primary production? To what degree would the solubility pump outweigh the oceanic gains in carbon from the biological pump? These sorts of discussion points would be highly beneficial to this paper.

We add a complete new paragraph in the "discussion" section of the revised manuscript to answer the concerns of the reviewer.

Finally, the appendices are not really appendices. They are single sentence additions, and they really shouldn't be appendices if they can easily be added to the main text.

We modified the appendices by removing the Appendix 4 that could easily be added to the main text.

Specific comments

• Line 21: "decreases or will decrease" isn't that the same thing?

We removed "will decrease".

• Line 23: Using (Boyce et al. 2010) reference is not recommended. See the responses to this paper in Nature underlining its caveats (https://www.nature.com/articles/nature09951)

We remove the sentence with Boyce et al., 2010 and rather introduce the study of Boyce et al., 2014.

• Line 27: -2.99 ± 9.11 is not significantly different from zero. The overall response of the oceanic primary production to global warming is highly uncertain. I would stress that as biogeochemical models have evolved from CMIP5 to CMIP6, the response of phytoplankton biomass and Net Primary Production are more uncertain. This doesn't undermine your study, but it I think it is important to tone down the confidence with which you are projecting a decline.

We add the sentence "This estimate is not significantly different from zero due to the evolution of biogeochemical models from CMIP5 to CMIP6, thus the response of phytoplankton biomass is more uncertain."

• Line 43: "… reports a decline of chlorophyll concentrations associated with a local oceanic warming of up to 0.7 °C. This maximum warming is attributed to changes in ocean circulation, under the global warming scenario". This could be written more clearly. I am unsure of what you mean.

We re-phrase the sentence.

• Line 75: typo in model

Changed

• Line 144: It is unclear what is done following the spin-up. What years is the 737-year run and which years it is running from? If the run begins at 1765, then your total number of years is 736, not 737.

First, we run a 10,000 years spin-up with BIOGEM only. The spin-up is used as a "restart file" for the 8 simulations, thus the simulations have a realistic nutrient distributions when they start. Second, following the spin-up, we run our simulations with ECOGEM. The runs begin at 1765 so the total number of years is indeed 736 years. All the simulations consider ECOGEM.

• Line 150: It is odd to consider your model validation as being a comparison with other models. I would advocate that you either compare your model with observations, or your change the title of this section to “model inter-comparison”.

We changed the title of the section.

• Line 151: EMIC has not yet been defined.

EMIC is previously defined in the “model description” section

Section 4.1.1: Here I am a little dissatisfied with the explanation of why chlorophyll concentrations are greater in the simulation with phytoplankton light absorption. All else being equal, if more shortwave radiation is absorbed in the upper layers of the ocean and less is able to penetrate down to deeper layers, then this should increase the vertical density gradient through the upper water column. An increase in stratification is the result, limiting vertical mixing of nutrients to near-surface layers. However, I suppose that if less radiation penetrated to deeper layers, then the strongest density gradients would exist nearer the surface, lessening density gradients deeper in the water column and thereby increasing the nutrient flux to the lower euphotic zone? All in all, I think I need a clearer explanation of the physical mechanism that is occurring. It would be informative and highly beneficial to this paper to conduct 1D water column simulations that test the effect of phytoplankton light absorption on the nutrient fluxes to phytoplankton.

Although a 1D model is easier and may give concrete answers, several previous model studies (e.g. Asselot et al., 2021, Paulsen et al., 2018) show that in an ESM with an atmospheric component the dynamics are different. In our previous article we have shown that the larger surface chlorophyll concentration associated with phytoplankton light absorption is due to two different mechanisms. First, phytoplankton light absorption leads to weaker biological pump and a larger amount of labile inorganic matter at the surface, enhancing the remineralization at the surface. As a consequence, the higher remineralization leads to larger nutrient concentrations at the surface and thus enhances the surface chlorophyll biomass. Second, the upward vertical velocity, specifically in the upwelling and mid-latitude regions, is enhanced due to the global warming of the ocean. As a consequence, the penetration depth of sinking material is reduced and organic matter is trapped closer to the surface. The combination of these biogeochemical and physical mechanisms explains the higher surface chlorophyll biomass with phytoplankton light absorption.

• Line 205: “not been tuned yet”. This suggests that the model has never been tuned. Surely that’s not correct.

The model has been previously tuned to get reasonable primary production and nutrient fields but not to match the projected atmospheric CO_2 concentrations.
We rephrase our sentence.

• Section 4.2.1: What component of the increase in atmospheric CO2 is due to (i) the solubility pump, (ii) the carbonate pump and (iii) the biological pump? Just saying that the increase in CO2 is due to additional surface heating is not convincing. I am not saying that the majority of it is due to biological mechanisms, but surely a portion of the change is due to the changes in phytoplankton biomass being closer to the surface ocean, and therefore not exporting organic carbon to deeper levels as efficiently? Alternatively, the increase in phytoplankton biomass may in fact tend to increase the air-sea flux of CO2, but this is more than opposed by the solubility effect of additional warming.

With our model setup, we already showed that the increase in atmospheric CO2 concentration is mainly due to the solubility pump (Asselot et al., 2022). The changes in solubility pump enhance the air-sea CO2 fluxes by 10% while the changes in biogeochemical pumps enhance the air-sea CO2 fluxes by <1%. Clearly, the solubility pump has the largest effect on the increase atmospheric CO2 concentration with phytoplankton light absorption. Furthermore, the warmer ocean leads to a reduced ocean CO2 uptake, explaining in part the increase in atmospheric CO2 concentration with phytoplankton light absorption (see response to referee #3).
We add sentences in the "synthesis" section.

• Line 233-236: These mechanisms have been proposed to explain to explain the increase in phytoplankton biomass in the runs with phytoplankton light absorption, but they have not been explained mechanistically or shown.

We explain more in detail the mechanisms behind the increase in surface chlorophyll concentration in the "oceanic properties" section.

• Line 249: The limitation of phytoplankton growth at temperatures greater than 20 °C is an odd choice… What is the motivation for this choice in parameterisation? Because this parameterisation is really quite arbitrary and probably not realistic (i.e. does not follow the Eppley curve (Eppley 1972) nor the recent work by Anderson et al. (Anderson et al. 2021) that confirms community-wide exponential increases in growth rates with temperature), I have to encourage the authors to discuss how this result for RCP8.5 simulations is likely not realistic either. Also, please tell us what the parameterisation is. What is the equation?

The equation of the temperature limitation is an Arrhenius-like equation:

$$\gamma_T = e^{A(T - T_{ref})}$$

With γ_T is the temperature limitation, A is the temperature sensitivity, T is the sea surface temperature and T_{ref} is the reference temperature.
In the model setup, T_{ref} = 20°C thus if the SST exceeds 20°C then the temperature limitation increases and phytoplankton growth is limited. T_{ref} 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). Therefore we consider that T_{ref} = 20°C is realistic.
We add sentences in the "ecosystem component" section.
We also discuss that there may be changes if adaptation was implemented in the model setup.

• Line 250: Additionally, the limitation is definitely present in all your scenarios, but RCP8.5 is likely the only one where enough surface grid cells exceed 20 °C that it has a significantly negative effect on the chlorophyll/temperature increase. It is worth mentioning that this is the case. In fact, it may explain the slight decreases in the phytoplankton light absorption effect for each RCP scenario from 2.6 to 4.5 to 6.0.

Indeed the limitation is present in all the simulations but only under the RCP8.5 scenario there are enough grid cells exceeding 20°C. This explanation is added to the revised manuscript.

• Paragraph beginning Line 276: This paragraph discusses the stronger effect of phytoplankton light absorption in RCP scenarios 2.6, 4.5 and 6.0, with RCP8.5 showing a weaker role. The authors take this opportunity to explain that this effect is weaker in RCP8.5 because stronger warming causes an increase clarity (by reducing phytoplankton biomass) and therefore the phytoplankton absorption effect is weaker. But, the authors have only just discussed that the weaker effect in RCP8.5 is due to the arbitrary limitation of phytoplankton above 20 °C. The different response in RCP8.5 is therefore an artefact of the model, and is not realistic. Therefore, it is not evidenced by your study to say that "our findings indicate that a severely warmer world increases ocean clarity and slows down the phytoplankton-induced global warming".

As argued previously, the temperature limitation of phytoplankton growth above 20°C is realistic. The difference response under the RCP8.5 scenario is thus realistic. As a consequence the main conclusion of this study remains identical.

Thank you for considering my input to your research.

Pearse J. Buchanan

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