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Summary:

Asselot et al. study the mechanisms by which light absorption by the phytoplankton impacts the ocean atmosphere system, global temperatures in particular. They use the Earth System Model of intermediate complexity GENIE, extended to include light absorption by Asselot et al. (2021), and conduct several numerical experiments by turning on and off key feedbacks. This approach allows the authors to quantify the impact of the different mechanisms studied. Asselot et al. conclude that the air-sea CO2 exchange has a much larger impact on biologically-induced global climate warming than changes in the heat fluxes.

We would like to thank Alexandre Pohl for his constructive comments.

General evaluation:

I think that the authors approach an interesting topic. They present a clear modeling strategy. Most of the main text is clear and concise. Figures and Tables are mostly well adapted and satisfactorily convey the authors' message. However, several points should me made clearer. More importantly, I'm worried about key choices made by the authors regarding their modeling setup, which I think are not really obvious and have the potential to significantly impact the results and conclusions.

We revise the manuscript to add clarity. We tried to answer the concerns of the reviewer and to justify our model setup.

Main comments:

- Modeling setup:
    - o Model spinup and equilibrium: Based on Section 3, the authors ran cGENIE with biogem for 10 kyrs and then restarted the model with ecogem for another 1000 years. First, I don't understand how ecogem can be run based on a biogem-only restart, but this is a technical point. More importantly, I don't think that this setup can lead to robust results. Indeed, changing from biogem to ecogem is expected to lead to important changes. An example is the pCO2 that drops from 278 ppm in the biogem run to 169 ppm in the Bio simulation. Such drastic change is expected to impact global climate and I don't think that 1000 years are enough to reach a new equilibrium. I would instead suggest running ecogem simulations for 10–20 kyrs and make sure that equilibrium is reached. Ensuring a robust equilibrium is particularly important considering the subtle changes reported between the different experiments (e.g., Table 7).

BIOGEM represents the transformation and spatial distribution of elemental biogeochemical tracers, such as nutrient, DOC and POC. This model component needs ~8,500 years until steady state. We run the 10,000-years spinup only with BIOGEM to start the simulations with a realistic nutrient distribution. ECOGEM considers only the living component that needs only 800 years to reach a steady state. The changes in atmospheric CO_2 concentration after 1,000 years are < 0.03 ppm/year, thus we consider the climate system in equilibrium.

o <u>Climate state- and model configuration- dependence of the results</u>: I also don't understand the choice of a cold climate (169 to 178 ppm; Table 5). I do expect the results of the study to be climate state-dependent. It would be interesting to determine if the same conclusions can be reached when using a higher baseline CO_2 level (e.g., 350 ppm or above) (which might lead to very different results, due for instance to the lower seaice cover). At least, the authors should state that their results are expected to be climate state-dependent. I also think that it should be made clear that the present-day continental configuration is used. I also expect results to potentially vary with the land-sea mask.

We agree that we should discuss the climate state in our final section and will do this in the revised version. Yet, higher CO_2-concentrations will not fundamentally change our conclusions, because higher CO_2-concentrations imply higher atmospheric temperature and thus higher SST. The higher SST decreases the CO_2 solubility, leading to a larger air-sea CO_2 flux. Please note that we do not prescribe the atmospheric CO_2-concentrations neither prescribe CO_2 emissions but allow the system and the CO_2-concentrations to evolve freely.
The present-day continental configuration is used.

o <u>Absence of seasonal cycle</u>: Based on Appendix B, it appears that neglecting the seasonal cycle in the version of ecoGENIE modified to include light absorption leads to a global temperature difference bias (0.33 – 0.14 = 0.19 °C) that is larger than most temperature differences computed in Table 7. I think that the absence of seasonal cycle thus constitutes a major limitation to this work and would encourage the authors to repeat the experiments with seasonality.

For technical reasons the seasonality is removed. In this study, for several simulations, we prescribe a SST field. The prescribed SST field comes from the reference run (Bio) and unfortunately the SST outputs are annual means. As a consequence, it does not make sense to prescribe a yearly-averaged SST field while turning on the seasonal cycle.
Enabling seasonality would lead to larger seasonal increase of temperature but it would also lead to larger seasonal decrease in CO_2 solubility. We don't think that the heat-pathway would overrule the CO_2-pathway. Enabling seasonality would not change the qualitative importance of each climate pathway. Therefore, we consider that removing the seasonal cycle is an adequate simplification for our study. We would also like to refer to our comments to the 2^{nd} reviewer on this issue.

o <u>Absence of size classes</u>: I don't understand why the authors modified the model of Ward et al. In Appendix A, it is stated that it permits reducing computational time. However, a 10-kyr ecogem model run using the 36x36 grid with 16 vertical levels requires less than 5 days on a single core. Although I understand that using 32 levels probably makes the model more expensive, I guess that the model remains very fast to run and tractable. In any way, please make sure to describe the model in a consistent manner. For now, it is not very clear: in section 2.1.3, the model of Ward et al. is described as including 2 PFTs with size classes, while in Appendix A, it is described as including 16 PFTs. There is a confusion between the number of plankton types (zoo vs. phyto) and the number of size classes.

We agree and apologize; we will modify the section 2.1.3 and Appendix A for clarification about how many size classes we use. Please note that in a previous paper (Asselot et al., 2021, JAMES), we already analyse the effect of phytoplankton light absorption between simulations with 1 phyto- and 1 zooplankton size classes and simulations with 6 phyto- and 6 zooplankton size classes. Between these two different ecosystem complexities, the effect of phytoplankton light absorption on the climate system is similar. Thus we decided to simplify the ecosystem and only keep 1 phyto- and 1 zooplankton size-classes. Again, we would like to refer to our answer to the 2nd reviewer.

o Temperature-dependent remineralization: Is any temperature-dependent remineralization scheme employed in this study? It is not stated anywhere, but lines 249–251 suggest that the higher SSTs lead to a shallower remineralization. If so, please clarify this point and provide a reference.

The shallower remineralization is due to the higher production with phytoplankton light absorption and due to a larger source to the DOM with a shallower remineralization length scale. We will add the explanation in the revised manuscript.

o Absence of light limitation by sea ice in ecogem: Although it probably has a minor impact, I also note that the attenuation of the photosynthetically available radiation by sea-ice in ecogem, as now part of the muffingen release, does not seem to be used in these experiments.

We thank the reviewer for this comment. Indeed, the light limitation by sea-ice in ECOGEM is not included in our simulations. This clarification will be added to the revised manuscript.

o Absence of dynamical atmosphere: Based on Section 4.2, it seems that changing wind stress could have a major impact on the results. cGENIE wind fields are boundary conditions and do not vary with changing climate. I agree with the authors that a dynamical atmosphere would be useful (lines 412–413) but would rather present this as a current limitation / necessary next step rather than a possible way forward and encourage the authors to expand on this point.

Yes, in our simulations, the wind fields are prescribed thus the wind field is similar between simulations. We agree that we should make clear that our discussion is rather an outlook. Previous studies evidence either an increase in wind speed in the subpolar regions (Patara et al., 2012, Climate Dynamics) or an enhanced atmospheric dynamics (Gnanadesikan et al., 2009, Journal of Phy. Oceano.; Wetzel et al., 2006, Journal of Climate) due to phytoplankton light absorption. Therefore, with a dynamical atmospheric component, the wind speed would increase due to phytoplankton light absorption. As a result, the air-sea CO2 flux increases as well. One indeed could speculate that including a dynamical atmosphere would reinforce the importance of the CO2-pathway. We will make it more clear and will add these aspects in the revised manuscript.

- Model description: In section 2.1.2, the authors should clearly state that the description only refers to their model setup / the choices that they made. For instance, all productivity schemes of cGENIE do not include light nor iron limitation.

We will better explain our specific model setup.

- "Previous estimates": Please provide the correspond values, on lines 252, 253, 289, 374.

We add the previous estimates for a better comparison with previous studies.

Other (mostly minor) points:

• Line 8: "dissolved CO2" > "air-sea CO2 flux"?
Changed.

• Line 59: "due to fluctuations"
Changed.

• Line 62: "as follows"
Changed.

• Line 71: "composed"
Changed.

• Line 77: "cGENIE"
Changed.

• Line 120: state variables for iron, too?
Changed.

• Line 139: "(Eq. 2)"
Changed.

• Lines 142–144: I don't follow this. It is stated that "the whole light absorption leads to heating of the water". Shouldn't the fraction used by the plankton be subtracted, according to line 142?
We add clarification by stating: "Part of the light absorbed is used by phytoplankton for photosynthesis and part leads to heating of the water."

• Line 148: "(Eq. 3)"
Changed.

• Line 152: delete “certain”?
Changed.

• Line 162: “(Eq. 4)”
Changed.

• Lines 200–201: I thought that atmospheric pCO_2 was prescribed, based on lines 199–200?
No, for this simulation CARB, we force the atmosphere with the heat fluxes from the reference simulation Bio and with the atmospheric CO_2 concentration from the simulation BioLA. Between CARB and Bio the heat fluxes are similar but the atmospheric CO_2 concentrations differ. Thus the simulation CARB permits to understand the effect of phytoplankton-induced changes in atmospheric CO_2 concentration.

• Section 4.1: I don’t understand the utility of this section (which should be called “climate sensitivity” by the way?). Delete?
We put the whole section 4 in the appendix.

• Line 216: “differs”
Changed.

• Line 227: “analyses”
Changed.

• Line 228: “do not exceed the maximum difference of SST between our simulation results”. I don’t understand this.
We remove this part of the sentence.

• Line 230: “Even larger SST fluctuations … interface”. Not shown?
This sentence refers to the previous sentence where we increase the SST by 1°C while the air-sea CO_2 flux only increases by 2.58%. We will move this part in Appendix.

• Lines 231–232: “We increase the wind speed by 0.2 m/s… (Knutson and Tuleya, 2004).” Please expand. It would be useful to the reader.
We will add more explanation.

• Line 270: I would avoid calling chlorophyll concentration a “climate variable”.
This study and several previous studies focusing on biogeochemical and biogeophysical mechanisms evidence that phytoplankton (and thus chlorophyll biomass) influences the climate system. Thus we consider chlorophyll being a “climate variable”.

• Line 277: Is this difference larger than the one obtained when running the same experiment twice?
Running the simulations twice does not change our results.

• Line 296: “biomass is more important in HEAT than in the reference simulation (Table 4)”
Changed.

• Lines 308–310: please rephrase or add punctuation.
Changed.

• Line 331: “a higher negative value” > “lower absolute value” may be clearer?
We change the sentence by: “a more negative value…”.

• Lines 337, 368: please delete “rather”.
Changed.

• Lines 351: “in these simulations”?
Changed.

• Line 357: “Oschlies (2004) and Lengaigne et al. (2009)”?
Changed.

• Lines 364–365: “specific humidity and evaporation are higher in the simulation CARB than in BioLA”
Changed.

• Line 371: please delete “indubitably”.
Changed.

• Line 401: formatting of the references.
Changed.

• Lines 413–415: “Observations… […] Wurl et al. (2018).” Please delete or expand. As such, the message is not easy to understand.
Changed.

• Appendix B1: should refer to Table B1.
Changed.

• Line 435: “decreases the mean annual SST”
Changed.

• Line 437: “dampens”
Changed.

• Lines 444–445: “maximum global sea-ice cover change (or difference)”
Changed.

• Caption of Fig. 2: EMBM should be defined.
The names of the different model components are now defined in the main text.

• Figure 5: Unless I missed something, this figure can be deleted since it is redundant with Fig 3 and the 3rd column of Table 7.
We will change this figure and will remove Table 7.

• Table 1: “Bio – Reference run without phytoplankton light absorption”.
Changed.