Alexandre Pohl (Biogéosciences, Dijon, FRANCE)
02 September 2021

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 CO_2 exchange has a much larger impact on biologically-induced global climate warming than changes in the heat fluxes.

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.

Main comments:

- Modeling setup:
    - 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).
    - Climate state- and model configuration- dependence of the results: 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 CO2 level (e.g., 350 ppm or above) (which might lead to very different results, due for instance to the lower sea-ice 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.
    - Absence of seasonal cycle: 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.
    - Absence of size classes: 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.

  - 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.
  - 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.
  - 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.

- 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.

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

Other (mostly minor) points:

- Line 8: "dissolved CO2" > "air-sea CO2 flux"?
- Line 59: "due to fluctuations"
- Line 62: "as follows"
- Line 71: "composed"
- Line 77: "cGENIE"
- Line 120: state variables for iron, too?
- Line 139: "(Eq. 2)"
- 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?
- Line 148: "(Eq. 3)"
- Line 152: delete "certain"?
- Line 162: "(Eq. 4)"
- Lines 200–201: I thought that atmospheric pCO2 was prescribed, based on lines 199–200?
- Section 4.1: I don't understand the utility of this section (which should be called "climate sensitivity" by the way?). Delete?
- Line 216: "differs"
- Line 227: "analyses"
- Line 228: "do not exceed the maximum difference of SST between our simulation results". I don't understand this.
- Line 230: "Even larger SST fluctuations … interface". Not shown?
- 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.
- Line 270: I would avoid calling chlorophyll concentration a "climate variable".

- Line 277: Is this difference larger than the one obtained when running the same experiment twice?
- Line 296: "biomass is more important in HEAT than in the reference simulation (Table 4)"
- Lines 308–310: please rephrase or add punctuation.
- Line 331: "a higher negative value" > "lower absolute value" may be clearer?
- Lines 337, 368: please delete "rather".
- Lines 351: "in these simulations"?
- Line 357: "Oschlies (2004) and Lengaigne et al. (2009)"?
- Lines 364–365: "specific humidity and evaporation are higher in the simulation CARB than in BioLA"
- Line 371: please delete "indubitably".
- Line 401: formatting of the references.
- Lines 413–415: "Observations… […] Wurl et al. (2018)." Please delete or expand. As such, the message is not easy to understand.
- Appendix B1: should refer to Table B1.
- Line 435: "decreases the mean annual SST"
- Line 437: "dampens"
- Lines 444–445: "maximum global sea-ice cover change (*or* difference)"
- Caption of Fig. 2: EMBM should be defined.
- 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.
- Table 1: "Bio – Reference run without phytoplankton light absorption".