Review 2 of “Climate pathways behind phytoplankton-induced atmospheric warming” by Asselot et al. (2021) for Biogeosciences

The authors have carefully revised their manuscript in response to the reviewer comments, and I believe the manuscript has improved substantially as a result and is ready to be published with a few minor revisions. Below I focus on the responses to my own comments, but as they overlapped substantially with the other reviewers I believe their queries have also been largely covered.

We thank Dr. David A. McKay for the time he invested to review our manuscript and for his suggestions. We would also thank the associate editor for his interest in our study. We address all the suggestions of the reviewer in the “specific comments” section.

The Abstract has been clarified as suggested, with more detail provided on what has been done and why along with key results. The Introduction also now has more background on how PLA affects the key pathways analysed in this study. My only suggestion is an additional statement to clarify how you go about disentangling the mechanisms in this study (see below for detail).

More detail has been provided throughout the rest of manuscript as well, for example how these results directly relate to previous numbers, mechanism explanations from Asselot et al. (2021), and methodological details for e.g. the new light scheme as well as more model description, with just a few more useful clarifications suggested below.

The sensitivity analysis of repeating Bio & BioLA with multiple size classes was performed as suggested, showing a small but non-negligible effect as suspected. The authors have included this in the Appendix which I think is worthwhile, although I’d suggest referring to this Appendix more clearly in the main text (see below). Reducing the main text dedicated to the sensitivity analyses is also wise, as is introducing a specific Limitations section.

I still have some reservations about the low CO2 baseline and how the results may quantitatively change with climate state, but the authors have now made this limitation and that they’re focusing on the relative, qualitative changes clearer throughout the manuscript (although I suggest a little extra detail on this below).

Other questions I had regarding the AMOC strength in the HEAT simulation, drivers of HCorg vs. Bio biomass differences, temperature-dependence, and size classes have also been answered and clarified in the text, along with minor issues rectified and some more implications & further work suggestions in the Conclusion.

Specific comments (line numbers refer to track changes document):

Line 69: I think this final Introduction paragraph could do with an extra sentence or two at the end to state clearly how you achieve the disentangling mentioned in the first sentence, i.e. that in this study you effectively turn on and off specific pathways by prescribing values in order to isolate the impact of the 3 different mechanisms you’ve just described above. Additionally, the Introduction’s end here feels a bit sudden now that it ends on the 3 mechanisms and the original final paragraph with paper structure has been deleted, so the above suggestion might help to round off the Introduction.

We added a sentence at the end of the introduction section. The sentence is: “To achieve the disentangling of the specific climate pathways, we turn on and off the climate pathways by prescribing values in our ESM in order to isolate their impact on the climate system.”

Lines 86-89: I did indeed ask for a little more explanation of this citation here, but this is perhaps more than necessary and somewhat disrupts the text flow in this paragraph. Feel free to reduce this to a briefer, within-sentence summary if you prefer – for example: “Additionally, cGEnIE **has been** employed to assess the sensitivity of atmospheric CO2 to biogeochemical pumps, ocean circulation and climate feedbacks in the **Southern Ocean** (Cameron et al., 2005). A new ecosystem component…”

We remove the sentence as suggested by the reviewer.

Line 126: Might be worth clarifying that labile organic matter is mostly but not entirely remineralised in the upper water column, as it follows a fixed profile (specifically the Martin Curve) that declines geometrically until reaching a small (but not zero) asymptotic flux by 1000m.

We clarified this point and added: “and labile organic matter **mostly** remineralised in the upper water column”.

Lines 127-128: Slightly confusing phrasing here – could potentially be misread as CaCO3 turns into remineralised POM. Presumably actually means PIC remineralisation **follows the same profile** as POC 2 (i.e. Martin Curve): could rephrase as “…and its dissolution below the surface follows the same profile as the remineralisation of POM”. It might also be worth mentioning that CaCO3 production is parameterised as a fixed ratio to POC production, and so is not independent in this model (at least not until a separate calcifier PFT is implemented).

We cease the suggestion of the reviewer and changed the sentence. Furthermore, we also added a sentence explaining that CaCO3 production is parameterised as a fixed ratio to POC production.

Line 210: This would be a good place to briefly mention your model setup has a reasonable match to observations (as you state well in your response doc on pg14), before referring the reader to Asselot et al. (2021) for further details (you mention earlier that cGEnIE has been calibrated to observations, but some readers will want reassurance that it’s still good with this new light parametrisation).

We added the sentence “Our model setup has a reasonable match to observations and further details can be found in Asselot et al., 2021”.

Line 211: It would be useful to mention here that you've done a related sensitivity analysis and directly link to table B1, e.g. something like: "For simplification, only one phytoplankton and one zooplankton species are included in the model setup (Appendix A1 and B1). **Repeating our main simulations with multiple size classes for each results in relatively small differences (Table B1)**."

We added the sentence "Repeating our main simulations with multiple size classes leads in small differences compared to the simulations with one size class".

Line 212: I'd suggest a little more detail in this sentence to clarify that unlike your previous study prescribing SST (and therefore removing seasonality) is necessary for your experimental design in this study – your response has helped me to understand this bit now, but given that Reviewer 3 also got a bit confused on this it's likely some readers will also need a little extra guidance on this.

We added more details to explain why we remove the seasonal cycle.

Line 220: The term "climate sensitivity" here might get mixed up with the concept of Equilibrium Climate Sensitivity (ECS) by readers, which in this model is effectively fixed – I think what you mean is sensitivity of your variables of interest (absolutes of SAT, SST, chlorophyll) to CO_2, rather than the relative change in SAT per doubling of CO_2 (ECS).

Indeed, we meant sensitivity of our climate variables. We changed this sentence.

Line: 291: Upward vertical velocity of what, exactly? (also line 322)

We meant that the upward vertical velocity in the upwelling regions is enhanced. We revised the sentence.

Lines 343-345: It's good that you explicitly discuss the model's low CO_2 baseline and its causes here, but I think you should also include some of your discussion from the response doc re. how this might affect your results (i.e. pg14 starting where you say "*We agree that our quantitative estimates would be affected if the atmospheric CO_2 concentration of the reference run would be higher but…*"), either in this section or in the new Limitations section.

We added a discussion of the low CO_2 baseline in the "limitations" section.

Lines 471-490: This paragraph could do with breaking up in to two or more for clarity.

This paragraph has been broken up in two but the formatting will be made by the publisher.

Lines 477-480: Could do with citation(s) here re. PLA, denitrification, and hypoxia if available (if not, indicate it's a suggestion). Could also do with making the hypoxia -> denitrification step explicit.

After literature research, no studies have investigated the link between PLA, denitrification and hypoxia. We changed the sentences and added suggestion on this part. We also added a link between hypoxia and denitrification.

Line 480: Is this increase just from the PLA, similar to this study, or because of adding land as well?

The increase in heat budget is due to PLA. We revised our sentence.

Line 532: I know this is not what you mean here, but this could be misread as introducing size classes has no impact on the climate when in fact here it is just less impact in this case than PLA.

We revised our sentence: "We show that introducing more size classes has a smaller effect on the climate system than phytoplankton light absorption"

Lines 547-548: Could add something like: **", indicating that surface chlorophyll biomass is more "sensitive" to PLA than an increase of 40 ppm in pCO2**" from your response doc at the end of this sentence in order to make implication clear.

As suggested by the reviewer, we revised our sentence.

Lines 563-564: Maybe add what sort of order the slight effect is for clarity, e.g. "This small increase slightly affects the carbon reservoirs in our simulations **by < 1%**".

We revised our sentence as suggested by the reviewer.

Dr. David A. McKay, 19/11/2021