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

Paper Synopsis:

In this paper, Asselot et al. analyse the climatic impacts of including a representation of phytoplankton light absorption (PLA) in the Earth system model of intermediate complexity (EMIC) ecoGEnIE. By intercepting more light PLA results in greater surface water warming than would otherwise be simulated, which has knock-on effects for both heat and CO2 exchange across the air-sea boundary. However, the relative importance of these different climate pathways has not been analysed. To do this, Asselot et al. have implemented a PLA parameterisation they recently developed for ecoGEnIE (in: Asselot et al, 2021, GRL) to replace ecoGEnIE’s original simplified light representation, and then rerun simulations keeping different climate impact pathways fixed in order to assess the importance of each for PLA. Based on this they find that sir-sea CO2 flux has a much larger effect on atmospheric temperature than the air-sea heat flux, making the former the dominant climate pathway for PLA.

We would like to thank David Armstrong McKay for his very detailed, very helpful and supporting review.

General Comments:

In general this is an interesting and worthwhile paper that is suited to the scope of Biogeosciences (connecting ecology, biogeochemistry, and climate) and has a clear rationale (to disentangle the effects of PLA on climate), but would benefit from further explanation, clarifications, and details throughout in order to make the methodology and significance clearer. The manuscript could also do with a tidy up in some areas (e.g. paragraph formatting, confusing phrasings), but as this will be dealt with in copy-editing I have mostly focused on clarity and scientific content.

We will add more explanations, clarifications and details to make the manuscript clearer. We will also remove some tables and add figures where needed.

The Abstract could be more detailed on what has been done and why, for example outlining how the relative importance of heat and CO2 exchange has not previously been disentangled (and so provides justification for why this study is useful and timely), and directly mentioning in the abstract that you’ve implemented a new PLA parameterisation (as currently it just says you use ecoGEnIE without being clear on how it’s different from previous usage). Some more detail on the results would be useful in the Abstract as well, for example specifying the direction and magnitude of each pathway (e.g. +xoC, -yoC), and mentioning here or earlier in the Abstract which specific mechanisms are involved for both air-sea CO2 and heat fluxes (e.g. warming from PLA reduces capacity to dissolve CO2, changes in latent heat, etc.). Conversely, the sentence on lines 6-8 on model configuration seems more methodological detail than necessary in an Abstract.

We will seize the suggestion and will revise the abstract including more specific details on the motivation, model design and results.

The Introduction could so with more detail on exactly how PLA affects each pathway – for example, how PLA impacts the biological pump and the carbon cycle, or the relation of sea ice with gas exchange. At the moment several mechanisms are not really explained until they come up in the Results, when ideally they'd be flagged and explained first in the Introduction for the benefit of readers less familiar with PLA or the mechanisms. I'd also like to see some more detail on how the PLA scheme differs from the original ECOGEM setup for those not familiar with how light is treated in these models. The experimental set-up is clear though and capable of disentangling the potential climate impact pathways.

We now admit that it is more comprehensible to introduce the pathways already in the introduction section and will do that in the revised version.

In several places numbers from this study are qualitatively compared with previous studies, when a quantitative comparison would be more informative for the reader. Similarly, the reader is referred to Asselot et al. (2021) several times for explanations of particular mechanisms or methodological choices when it'd be better to briefly summarise those explanations in this paper as a recap for readers who may not have read the former paper or recall all the details.

We agree and will add these estimates and briefly summarize the particular mechanisms and methodological choices in the revised manuscript.

The authors make a reasonable case for why this study is still useful despite the lack of model seasonality (in contrast to their recent study Asselot et al. (2021)), but I feel that the statements that it has no effect on the results to be overstated given that it is clear that there are indeed some effects (reducing the net impact of including PLA from 0.45oC in Asselot et al. (2021) to 0.14oC here). A number of other methodological choices and discussion points, for example the surprisingly low atmospheric CO2 model baseline of ~170ppm, AMOC strength in HEAT, drivers of HCorg vs. Bio biomass differences, whether temperature-dependent remineralisation is enabled, and not using ECOGEM's size class functionality, could also do with more explanation or clarification. I'm also not convinced of the utility of the current sensitivity analyses – see my specific comments for details.
Lastly, some wider implications or significance should be signalled in the Conclusions, for example how these results might relate to current Earth system models' projections for climate change and if they might differ if they included PLA.

We would like to refer to the "specific comments" section. Furthermore, we already conducted additional sensitivity analyses to illustrate the effects of a more complex model with additional size classes.

Specific Comments:

Line 6-8: This sentence on model configuration seems more methodological detail than necessary in an abstract – the extra space can be used to give a little more rationale and results details.

Done

Line 27: Given that this model setup has no seasonality it's arguable whether you're able to analyse "varying magnitude" in this study – unless you mean ESMs more broadly than this study, or you mean the relative magnitudes between the different climate impact pathways? If the latter better to say relative rather than varying, as varying implies some analysis of model variability.

Indeed we referred to the pathways and thus replaced "varying magnitude" by "relative magnitude"

Line 31: "an increase of SST between 0.5-2C" relative to what – presumably no PLA? Need to clarify what exactly these past results are showing.

Yes, we refer to "no PLA"; we changed the sentence accordingly.

Line 37: Is the 0.5C increase global? Also, better to say 1C at end of line to keep consistent units.

The increase in atmospheric temperature is local. The units are changed.

Line 50: Add "both" before "air-sea heat and CO2 exchange" for sentence clarity.

Changed.

Line 57-58: More detail on the effects of PLA on the biological pump in Section 1 would be useful, as at the moment it is not explained until the results. Similarly with sea ice – a brief explanation stating that sea ice acts as an ocean cap that blocks gas exchange (and so lower extent means more gas exchange can occur) would be useful in Section 1.

In the introduction, more details will be provided on the different pathways, including the effects of PLA on the biological pump and consequences of sea ice on gas exchange.

Line 75: "the sensitivity of atmospheric CO2" to what (or do you mean long-term variability?), and how does the organic carbon pump explain it? Need a little more explanation if bringing up a past model usage.

We will revise this part.

Line 76-77: A minor point, but the current norm with the model developers is to spell cGENIE/EcoGENIE as cGEnIE/ecoGEnIE with a small n (as it's Grid Enabled...), so you may want to match that for literature consistency (although not super important). More importantly, in the next line GENIE should be specifically cGEnIE, as the two models have diverged and ECOGEM has been specifically developed for cGEnIE rather than GENIE.

We agree and adopt the spelling cGEnIE/EcoGEnIE.

Line 102-122: A bit more detail for readers not familiar with cGEnIE/ecoGEnIE might be valuable here. For example, on how remineralisation is dealt with (important for biological pump impacts), what ecological processes are temperature or size dependent (e.g. predation, nutrient uptake, photosynthetic rates), how CaCO3 is dealt with (alluded to in Figure 1 but not elsewhere – not so critical with no calcifier PFT available though). These may not be so relevant for this particular study, but unfamiliar readers might want extra context on how this particular model represents biogeochemistry.

We will add more explanation: The surface export is divided between refractory organic matter remineralised close to the seafloor and labile organic matter remineralised in the upper water column. The remineralisation is not temperature-dependent in our model setup. Furthermore, because we do not consider a sediment component, all organic matter reaching the sea-floor is instantaneously remineralised.
The size-dependent ecophysiological parameters are: the maximum nutrient uptake rate, the cell carbon quotas, the grazing and the partitioning of dissolved and particulate organic matter.
The temperature-dependent parameters are: nutrient uptake, photosynthesis and predation.
Calcium carbonate ($CaCO_3$) is represented in the model and its dissolution below the surface is treated as the remineralization of particulate organic matter.

Line 114: Presumably you do not activate temperature-dependent remineralisation as well (as it is not mentioned)? It is not a standard option in ecoGEnIE but is available, and some recent papers (Crichton et al, 2021, GMD; Armstrong McKay et al., 2021, ESD) showed how turning on TDR (as well as ECOGEM) in cGEnIE can significantly alter global export magnitude and patterns. Would therefore be useful to specify which version is being used, as PLA-induced warming would have different impacts on remineralisation depending on it.

We thank the reviewer for pointing out these recent papers. In our model setup, we do not active the temperature-dependent remineralisation. A sentence is added in the manuscript.

Line 116: Based on Table A1 though you only use one size class per PFT in this study despite more being possible, justified in Appendix A by more size classes having less of an effect than PLA in Asselot et al. (2021) and for saving computational time. One size ECOGEM is better than BIOGEM even with only one size class for each (as you still get explicit OM, flexible stoichiometry, more temperature sensitivities, etc.), but you'd still miss out on some extra ecological dynamics in response to warmer surface water (i.e. the combined effect of PLA and ecological complexity). You found a relatively smaller impact of increased ecosystem complexity in Asselot et al. (2021) on atm. CO2 relative to PLA, but given the smaller magnitude changes in this study it might play a non-trivial role, with for example your '6P6ZLA' simulations in Asselot et al. (2021) showing a ~10ppm difference in atm. CO2 to '1P1ZLA' versus a max. ~9ppm difference from your reference simulation here. Of course your focus here is on disentangling the effects of PLA so it does make some sense to simplify other aspects to make those effects clearer (though it'd be good to more clearly state as such in the main text rather than in the Appendix), but if possible it'd make an interesting sensitivity analysis to repeat Bio & BioLA with multiple size classes and see how that affects the net PLA impact. Also, while reducing size classes does save some computational time, from my own experience it'd likely be on the order of hours per run on a standard core, so not entirely prohibitive.

Though this is a bit out of scope of our study, where complexity is not the main issue, we repeated the simulations Bio and BioLA with 6 phytoplankton and 6 zooplankton species. The simulation without PLA is named Bio6 while the simulation with PLA is called BioLA6. The table below shows the quantities of the most important climate variables. The third row represents the differences between the simulations BioLA6 minus Bio6.

	Atm. CO2 conc. (ppm)	Chl. (mgChl/m3)	SST (°C)	SAT (°C)
Bio6	154	0.133	14.99	8.93
BioLA6	159	0.140	15.04	8.97
Difference	+5	+0.007	+0.05	+0.04

Even with an increased ecosystem complexity, PLA increases the atmospheric temperature. However, the effect of PLA is reduced with a higher ecosystem complexity compared to the effect of PLA with a simple ecosystem community. This is due to the higher amount of carbon stored in the living biomass with increasing number of species, thus reducing the effect of PLA on the atmospheric CO_2 concentration and on the climate system. We will add this information in the appendix.

Line 124: It would be useful in this subsection to briefly mention how your new light parametrisation differs from ecoGEnIE's original Ward et al. (2018) light scheme (or cGEnIE's) for readers who may be familiar with cGEnIE or ecoGEnIE but not Asselot et al. (2021), as currently only the new scheme is described. Of course one can go to your recent paper for the full details, but a quick recap would be helpful!

We added the sentence: "In the previous model version of Ward et al. (2018), light was only absorbed by phytoplankton. In our model version of Asselot et al. 2021, a new light scheme is implemented where the absorbed light by phytoplankton is converted into heat and affects the oceanic temperature. Furthermore light absorption takes place throughout the water column and is not restricted to the first oceanic layer."

Line 125-127: This is a reasonable simplification and likely won't alter your overall qualitative findings as you state, but I'm not sure it can be said to not affect your results. Given you describe the significant impacts of short-lived seasonal algal blooms in the Introduction, it stands to reason that an annual mean approach will probably underestimate PLA's impact on production and surface water warming. Given technical limitations it's probably fair not to include this in this paper (though I am intrigued as to why – see next-but-one comment), but it's no doubt a limitation to this study and should be stated as such.

We agree and will add this limitation in the discussion/conclusion section.

Line 168: It would be useful to briefly describe how PLA affects CO2 via these parameters here – presumably warmer surface water from activating PLA reduces solubility and sea ice fraction, but it's worth stating so explicitly for clarity.

We agree and will add a sentence accordingly.

Line 178: How come a seasonal SST cycle was possible in Asselot et al. (2021) but not in this paper (except Appendix B?) if it's effectively the same model code otherwise? Judging by the PLA impact in Asselot et al. (2021) versus the BioLA vs. Bio numbers in this study it seems to have quite large impact on the total warming magnitude. While unlikely to change the qualitative importance of each climate pathway in this study, it can't entirely be ruled out that larger peak warming with seasonality-enabled could make non-CO2 pathways relatively more impactful.

The seasonal cycle is removed for technical issues. In Asselot et al. (2021), we did not have to prescribe the SST in contrast to this study, at least for some simulations to look at specific pathways. The outputs of the SST field are annual means. Thus, it does not make sense to prescribe a yearly-averaged SST field while turning on the seasonal cycle. We decided to remove the seasonal cycle to be consistent with our prescribed non-seasonal SST field.
Enabling seasonality would lead to larger seasonal increase of temperature but it would also lead to larger seasonal decrease in CO2 solubility. Therefore, we don't think that the heat-pathway would overrule the CO2-pathway. We think that enabling seasonality would not change the qualitative importance of each climate pathway but we will add this point in the discussion section.

Line 190: So it is exactly the same scheme (including ECOGEM plus your new PLA scheme), just with this parameter set to 0? Important to be clear on as would affect baseline inter-comparability if they were using different modules. If it is just that parameter then it should be fine.

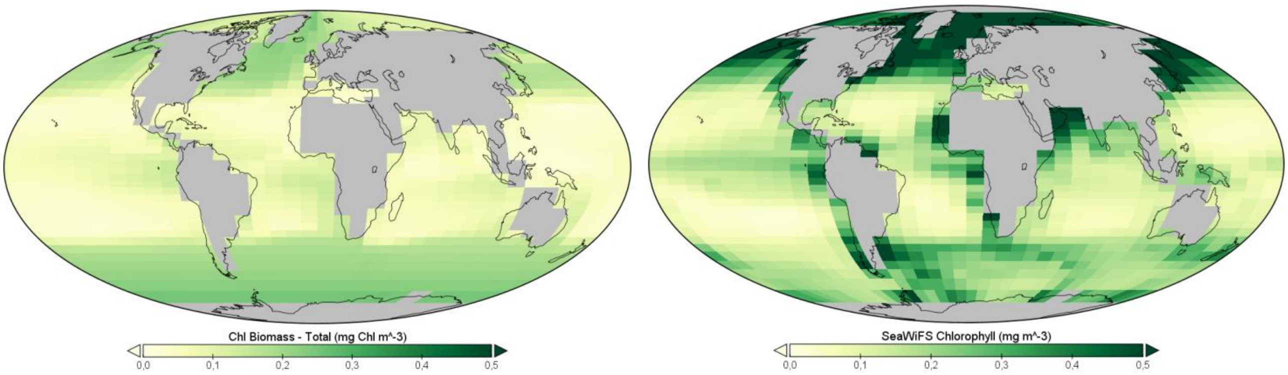
Yes, all the simulations include ECOGEM plus the new PLA scheme. And yes, only for our reference simulation Bio, the chlorophyll absorption coefficient is set to 0, thus chlorophyll does not absorb light at all.

Line 196: 169 ppm for default scenario atmospheric CO2 seems rather low – it effectively means you're looking at an LGM ocean. Looking at lines 175-178 & Asselot et al. (2021), you spin up BIOGEM with 278ppm constant and then restart with ECOGEM+PLA, implying that CO2 falls by ~110ppm during the first ~700 years of your experiments before stabilising. This doesn't occur in the original ECOGEM version, with ecoGEnIE set up to match quasi-modern observations, so must be a result of adding PLA or some other modification you've made to ECOGEM (perhaps allowing deeper PP?). I know you're primarily interested in the relative impact of PLA on different climate pathways rather than overall magnitude of climate impact or state, but it strikes me as an unusual baseline climate state to end up using (even for an EMIC!) versus a more usual pre-industrial Holocene baseline, and might make inter-comparison with other processes and model developments trickier. More importantly the relative importance of PLA's climate impact pathways might also be state-dependent and vary depending on baseline, which could affect your results. At the very least I think this should be flagged here, and it'd be useful to know why CO2 falls so much in response to modified ECOGEM.

Another similar issue is model-data comparison – if CO_2 drifts this much, does adding PLA change ecoGEnIE's match with observations, or was this covered in Asselot et al. (2021)? Some supplementary plots to demonstrate this might be necessary if there are noticeable differences.

We are aware that an atmospheric CO_2 concentration of 169 ppm for the reference scenario is indeed very low. Primary production takes place even in deeper layers of the water column and we also find deeper chlorophyll biomass. Our new model setup allows production until the sixth oceanic layer while the previous model setup only allows production in the surface layer. As a result, in our new model setup, more carbon is stored in the deeper ocean, reducing the atmospheric CO_2 concentration.
Here we are interested in the relative impact of PLA on different climate pathways rather than on the overall magnitude of climate change. We agree that our quantitative estimates would be affected if the atmospheric CO_2 concentration of the reference run would be higher but we assume that the qualitative estimates would be very similar and our conclusions will not change. If the atmospheric CO_2 baseline is higher, also the heat budget would increase. As a consequence, the ocean would be warmer and the CO_2 solubility would decrease, increasing the importance of the CO_2 pathway in the phytoplankton-induced atmospheric warming.
Concerning the second point, in our previous paper (Asselot et al., 2021), we already compared our results with observations. We showed that the AMOC, the primary production, the export production of POC and the PO_4 concentrations match observations. Here, for instance, we only compare the observed and modelled surface chlorophyll concentration.

BioLA SeaWIFS



The global pattern of surface chlorophyll biomass is in agreement with the satellite-derived estimates. The high latitudes show a large chlorophyll biomass while the subtropical gyres indicate a low chlorophyll biomass. However, the model underestimates the magnitude of the surface chlorophyll biomass. This is particularly true in the northern polar region and the upwelling regions. These limited agreements with observations are in line with the results of Ward et al. (2018).

Line 215-218: You're not so much analysing the model's climate variability here as the difference between two different climate baselines (climate variability would imply seasonality, multi-decadal oscillations, etc.). And how do these runs compare with the 169ppm baseline in the main default scenario? A ~110ppm difference would be more significant than the 40ppm discussed here, and given that your scenarios are all around ~170ppm (Table 5) I'm not sure why comparing just 280 and 320 ppm to each other is so useful here.

As also suggested by Reviewer 1, we moved the section 4.1 (climate variability) to the appendix in the revised manuscript. The following information is now available in the table where we compare the results of the reference (169ppm) and a sensitivity model run (280ppm).

	Atm. CO2 conc. (ppm)	**Chl. (mgChl/m3)**	**SST (°C)**	**SAT (°C)**
Exp_1	169	0.09949	15.26	9.31
Exp_2	280	0.1177	16.78	11.92
Difference	-111	-0.01821	-1.52	-2.61

The surface chlorophyll biomass, the SST and SAT are lower in our reference simulation (169ppm) compared to the sensitivity simulation (280ppm).

Line 220-223: And how would this affect the main results, in particular BioLA vs. Bio? A sensitivity analysis would be most useful in determining how sensitive the overall pattern of findings are to certain parameters such as baseline pCO2, SST, wind speed, seasonality, etc. by repeating some or all of your main simulations with those different baselines, whereas simply showing that higher pCO2 leads to greater SAT & SST is somewhat unsurprising. If more detailed sensitivity analyse simulations were conducted (which ideally they should, or the impacts of critical factors explored and discussed in greater detail some other way) then the supplement could be used to avoid manuscript clutter with just a brief summary in the main text.

We find that with an increase of 40 ppm, the changes in surface chlorophyll biomass are lower than the changes in surface chlorophyll biomass due to PLA, indicating that surface chlorophyll biomass is more “sensitive” to PLA than an increase of 40 ppm in pCO2. Yet, due to time constraints, we refrain running all experiments again with different parameters for a more detailed sensitivity study. We also think that our current findings are meaningful as it provides a first glimpse into different pathways even if we cannot answer all potential questions related to it.

Line 230: The change in FCO2 is indeed small, but on long timescales it could still affect the ocean carbon sink capacity quite a bit (after all, if SST had marginal impact on CO2 flux then there wouldn't be worries about the solubility pump and therefore the ocean carbon sink declining with global warming). However, in equilibrium runs with relatively stable SATs/SSTs as in this study this isn't so important, and the maximum SST difference from your main runs yields a far smaller change. Still, it might be beneficial to calculate how much a 0.21% increase in FCO2 would affect carbon reservoir size over the duration of your runs, if only to demonstrate it's only a minor factor.

In this study, the changes in heat budget are small therefore the effect of SST on FCO2 is not important. Between our simulations, the maximum change of FCO2 is 0.21%. The table below indicates that a 0.21% increase in FCO2 slightly affects the carbon reservoirs in the simulation Bio.

Carbon reservoir	**Effect**
Atm. CO_2 concentration	+0.55%
DOC	-0.71%
POC	-0.04%
DIC	-0.32%
Carbon biomass	-0.01%
$CaCO_3$	-0.001%

Line 234-235: But what's the implication of the larger impact of changed wind speed baseline on FCO2 for your results then? At the moment you use the comparison to imply SST's effect is negligible, but don't go in to the implications of your wind speed sensitivity calculation.

Between all the simulations, the wind field is prescribed and identical. As a consequence, the effect of wind on FCO2 is identical for all the simulations. We just modify the wind speed for the sensitivity analysis. In the "model setup" section, we add sentences to explain the wind fields.

Line 237-238: Additionally cGEnIE/ecoGEnIE is rather low resolution anyway and biota can't move between grid cells, so will not resolve some key regional processes even if seasonality was included.

We agree and add this argumentation in the manuscript.

Line 246: The CARB simulation is not described in Section 5.1, presumably because it's not so interesting for examining ocean dynamics (as you mention in outlining the scenarios), but this could bear repeating here even if briefly (much like CARB is only briefly mentioned in 5.2.1 to state atmospheric CO2 is the same as BioLA).

We agree and will add a brief description.

Line 249-251: I don't think this particular mechanism (PLA increasing chlorophyll biomass via changes in OM export) has been described in this manuscript prior to this point, and as readers haven't necessarily read the cited papers it would be therefore useful to explain this process here in a little more detail. Is it because activating PLA brings production closer to the surface and therefore boosts surface nutrient recycling and surface biomass? Also, "shallower downward flux of OM" is slightly confusing phrasing.

We agree and will rephrase this part.

Line 253-255: It would be useful to provide those previous estimates here for the reader to compare the current estimate with. Looking at Asselot et al. (2021), is the comparable number there 0.45C? If so it's quite a big difference, and emphasises the point earlier that no-seasonality may not affect the overall pattern of findings but will still affect your results to some degree (which you've usefully stated here).

We add the values of previous modelling studies. Due to PLA, we find a global increase of SST of 0.08°C while previous studies find a global SST increase between 0.45-1°C. We mention in the manuscript that the overall pattern of findings is not affected but quantitative changes arise. Since we are interested in a qualitative assessment only, we still find that the non-seasonality is an adequate simplification in the model setup.

Line 259-260: The impacts of stronger overturning fit with your results, but why is overturning a whole Sverdrup stronger in HEAT if the CO2->SAT forcing is the same? Needs some explanation, as a ~13% increase is quite a big difference.

In the simulation HEAT the SST is lower than in the simulation Bio and also the sea-ice cover is slightly higher (9.91% versus 9.79% in Bio), so there is more deep water formation in high latitudes inducing a stronger AMOC. We will add this in the manuscript.

Line 268-281: The differences between HCorg and HCorgSol solubility and PO4/chl/SST are explained in this paragraph, but not why the HCorgX scenarios in general have higher chl biomass & SST than the reference run. Presumably as with HCorg vs. HCorgSol it might be explained by higher PO4 in HCorg vs. Bio (which leads to higher biomass and therefore higher SST due to PLA), but an explanation of how these differences versus the default simulation arise (and indeed why PO4 is higher, explaining in more detail the role of the biological pump here) would be useful at the outset of this paragraph to guide the reader along. I'm also wondering to what extent using only one size class each for phyto- and zooplankton might affect these HCorgX simulations, given the role different size classes play in POM vs. DOM production.

The surface chlorophyll biomass in the simulations HCorgX is higher than the surface chlorophyll biomass in the reference simulation Bio due to the higher surface nutrient concentrations; in Bio the surface PO4 concentration is 0.18×10^{-6} mol/kg while in the simulations HCorgX the surface phosphate concentration is $>0.21 \times 10^{-6}$ mol/kg. The higher surface PO4 concentrations in HCorgX in Bio are mainly due to shallower remineralization length scale leading to less export production into the deeper layer (see also Asselot et al. 2021). Higher nutrient concentrations in turn increase the surface chlorophyll biomass enhancing the effect of phytoplankton light absorption. These explanations are added to the revised manuscript.
Please note that we addressed the question concerning the effects of ecosystem complexity, here defined as the number of size classes of phyto- and zooplankton already in the published article of Asselot et al. (2021). We therefore refrain from doing similar experiments since we do not expect fundamentally different results.

Line 289: Would be useful to state what the previous estimate of BioLA vs. Bio CO2 was to avoid the reader having to flick back to Asselot et al. (2021) to compare it themselves.

We will add the value of Asselot et al. (2021).

Line 295-297: What do you mean by more "important" here – higher, I guess? A bit unclear phrasing to me as it stands (similarly in final paragraph sentence.)

Changed.

Line 298: The remineralisation rate would only be higher if temperature-dependent remineralisation (TDR) is activated, otherwise remin. rates follow a fixed profile in cGEnIE & ecoGEnIE (see Crichton et al., 2021 & Armstrong McKay et al., 2021 for discussion of TDR in cGEniE/ecoGEnIE). If the former is the case in this study then you need to state this in the Methods, but if not then higher remin. rates can't be the cause of increased dissolved CO2 – instead it'd most likely be higher production rates leading to more remin. despite remin. being at the same rate. I suspect this might be what you meant here anyway – if so, should be clear on terminology here to avoid confusion.

We agree and will change the sentence accordingly; a temperature-dependent remineralisation is not considered in our model setup.

Line 397-398: You should state the mechanism for how PLA increases atmospheric CO_2 here (i.e. PLA = surface warming = lower solubility = more atm. CO_2) for clarity.

A sentence will be added to explain how PLA increases the atmospheric CO_2 concentration.

Line 332-333: Highest as in least negative, but arguably a tad confusing phrasing as Bio/CARB as plotted have the smallest (negative) bars. Might be better to rephrase sentence starting in line 331 as "A more negative value of net longwave heat flux indicates a greater loss of heat to outer space" along with next sentence accordingly.

We thank for the advice and will rephrase this sentence.

Line 334: Refer the reader here to the next section for details on specific humidity (otherwise it feels like a skipped detail).

We agree and will refer the reader to the adequate figure.

Line 339: This might be my more limited experience of climate thermodynamics relative to biogeochemistry here, but presumably in Bio the same amount of light enters the ocean but is absorbed less close to the surface (as no PLA), so activating PLA is not so much an "additional heat source" as heat absorbed closer to the surface in a way that affects the atmosphere on shorter timescales? If so then on longer ocean overturning and equilibration timescales could total ocean heat uptake in Bio eventually lead to higher outgoing radiation than in this spin-up?

There are no biologically-induced albedo changes and the outgoing radiation is similar – if this is what the reviewer is referring to. Here, we wanted to point out that PLA is an "additional heat source" for the surface of the ocean, where the air-sea heat exchanges occur. We will revise our sentence.

Line 358: How much lower than previous values? Useful to state these things directly so the reader can easily see the difference.

We estimate that phytoplankton light absorption raises the specific humidity by 0.5% while previous estimates indicate an increase of 2-6% (Patara et al., 2012). We will add the previous estimates in the revised manuscript.

Line 362: Confusingly phrased (had to re-read a few times to get it) – might be clearer rephrased as "with lower humidity leading to higher evaporation rates" or similar.

Changed.

Line 374: As mentioned earlier, it's useful to specify what these previous estimates are for easy inter-comparison by the reader. In this case, if the relevant number from Asselot et al. (2021) is 0.45C then arguably in the next sentence ~25% of that value is not such a small difference.

The previous estimates are added to the manuscript.

Line 380-381: Presumably the global net longwave heat flux decreases because of the lower SST.

Indeed, the SST is lower in HEAT compared to Bio, leading to a decrease of the global net longwave flux.

Line 412-417: I'd add that bringing in more plankton size classes (as well as more PFTs) would be useful for more complex ecological dynamics to emerge, which as well as enabling TDR could significantly affect the biological pump pathway. Additionally assessing if there's climate state-dependence, reflecting my earlier comments about the relatively low CO2 baseline. These could of course make nice sensitivity analyses for this present study, but if that's not possible in the time available then they'd certainly be useful next steps (and arguably increasing ecological complexity is a priority ahead of increasing atmospheric complexity with PLASIM). I'd also be interested how this would affect a transient warming simulation – would it likely amplify or dampen global warming? This could be something to draw out as wider implications too.

As suggested by the reviewer, we will add further perspectives on these issues in the final paragraph of the manuscript. Yet, previous studies have already analysed the effect of PLA under a transient warming scenario (Park et al., 2015; Paulsen, 2018). Paulsen (2018) indicates that PLA amplifies SST increase by 0.7K. Additionally, Park et al. (2015) evidence that PLA amplifies future Arctic warming by 20%.

Figure 3: The simulation names might do with being bigger (and/or in top-left corners), as I didn't notice them at first. Also, currently the dotted lines initially seemed to imply that the prescribed pathways are the same in each case, whereas for example the pCO2->SAT prescription in HEAT is from Bio and in CARB is from BioLA. Maybe could add a label above prescribed lines indicating from which setup the prescription is coming from? It'd make the figure busier, but would also make it more self-standing without needing to cross-reference to the text so much.

We will change Figure 3 and the names are clearer in the revised manuscript. However, we would like to keep the figures simple but we will add the information in the figure legends.

Thanks again for your comprehensive review and very valuable comments!

Dr. David A. McKay

27/8/2021

References:

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