

## 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 CO<sub>2</sub> 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 air-sea CO<sub>2</sub> flux has a much larger effect on atmospheric temperature than the air-sea heat flux, making the former the dominant climate pathway for PLA.

### 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.

The Abstract could be more detailed on what has been done and why, for example outlining how the relative importance of heat and CO<sub>2</sub> 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. +x°C, -y°C), and mentioning here or earlier in the Abstract which specific mechanisms are involved for both air-sea CO<sub>2</sub> and heat fluxes (e.g. warming from PLA reduces capacity to dissolve CO<sub>2</sub>, changes in latent heat, etc.). Conversely, the sentence on lines 6-8 on model configuration seems more methodological detail than necessary in an Abstract.

The Introduction could do 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.

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.

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 0.45°C in Asselot et al. (2021) to 0.14°C here). A number of other methodological choices and discussion points, for example the surprisingly low atmospheric CO<sub>2</sub> model baseline of ~170ppm, AMOC strength in HEAT, drivers of HC<sub>org</sub> 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.

#### **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.

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.

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.

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

Line 50: Add "both" before "air-sea heat and CO<sub>2</sub> exchange" for sentence clarity.

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.

Line 75: "the sensitivity of atmospheric CO<sub>2</sub>" 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.

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.

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  $\text{CaCO}_3$  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.

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.

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.  $\text{CO}_2$  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  $\sim 10\text{ppm}$  difference in atm.  $\text{CO}_2$  to '1P1ZLA' versus a max.  $\sim 9\text{ppm}$  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.

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!

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.

Line 168: It would be useful to briefly describe how PLA affects  $\text{CO}_2$  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.

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-CO<sub>2</sub> pathways relatively more impactful.

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.

Line 196: 169 ppm for default scenario atmospheric CO<sub>2</sub> 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 CO<sub>2</sub> 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 CO<sub>2</sub> falls so much in response to modified ECOGEM.

Another similar issue is model-data comparison – if CO<sub>2</sub> 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.

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.

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 pCO<sub>2</sub>, SST, wind speed, seasonality, etc. by repeating some or all of your main simulations with those different baselines, whereas simply showing that higher pCO<sub>2</sub> 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.

Line 230: The change in FCO<sub>2</sub> 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 CO<sub>2</sub> 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 FCO<sub>2</sub> would affect carbon reservoir size over the duration of your runs, if only to demonstrate it's only a minor factor.

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.

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.

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).

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.

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).

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.

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.

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.

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.)

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.

Line 397-398: You should state the mechanism for how PLA increases atmospheric CO<sub>2</sub> here (i.e. PLA = surface warming = lower solubility = more atm. CO<sub>2</sub>) for clarity.

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.

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

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?

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

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.

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.

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

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 CO<sub>2</sub> 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.

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 pCO<sub>2</sub>->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.

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**References:**

K. A. Crichton, J. D. Wilson, A. Ridgwell, P. N. Pearson, Calibration of temperature-dependent ocean microbial processes in the cGENIE.muffin (v0.9.13) Earth system model. *Geosci. Model Dev.* 14, 125–149 (2021).

D. I. Armstrong McKay, S. E. Cornell, K. Richardson, J. Rockström, Resolving ecological feedbacks on the ocean carbon sink in Earth system models. *Earth Syst. Dyn.* 12, 797–818 (2021).