Review of "A missing link in the carbon cycle: phytoplankton light absorption under RCP scenarios" by Asselot et al.,

General Comments

Asselot et al., quantify the impact of considering phytoplankton light absorption on the carbon cycle and climate under a multi-centennial future projections using an Earth System model. They find that resolving this additional process leads to increases in chlorophyll, sea surface and atmospheric temperatures, plus increases in atmospheric CO_2.

The experimental design and key results appear to be robust. The justification for considering multi-centennial timescales and the light absorption could be clearer in the introduction though, with some useful context only appearing at the end of the manuscript. The explanation of why phytoplankton light absorption leads to the key is not currently robust – there are clarifications and more analysis needed on the role of temperature in the ecosystem model and, in my view, over interpretation of minimal biological carbon pump changes that needs addressing. One of the most interesting outcomes of the study for me is that the impact of light absorption has a non-linear dependence on the scenarios but unfortunately I don't think the explanation for this is as robust as it can be.

Specific Comments

Lines 20 – 35: Cael et al., (2023) is a recent addition to the literature on observations that should be cited.

Line 47: "Following RCP8.5 scenario" – may be better phrased as something like "Under a scenario of anthropogenic emissions,…" to better differentiate it from the 1% atmospheric CO_2 increase experiment discussed in the previous experiment.

Line 63: "long timescale" – be more precise, do you mean centennial or millennial for example? Why is a >2100 timescale important to consider?

Section 2: This seems like a subsection of the Methods rather than its own individual section

Lines 83 – 84: the analysis is quantitative here in at least you quantify the net impacts. You don't quantify the components or drivers of those net impacts, but I don't think you need to frame that as qualitative!

Lines 132 -133: This could be more precise. For example, you can back out the percentage of POC remineralised from the e-folding depth (or the net flux from the curve if it's a double exponential) which gives a more intuitive metric here. This also needs to take into account the bottom depth of the euphotic layer which I think is different here than in Ward et al., (2018) so 590m may actually be a few hundred meters deeper. I am not sure where the <590m figure comes from DOM remineralisation as this is dependent on advection vs. remineralisation timescale – I think perhaps 590m gives the wrong impression of what's happening, maybe a more approximate number might help.

Lines 142 / 187 – 188 / Appendix B: What is the logic behind the choice of PFT cell sizes? Notably, the zooplankton size class is less than 10 times bigger than the phytoplankton which contrasts with the optimum grazing prey length ratio of 10 times smaller. This means the zooplankton type is not grazing optimally on the phytoplankton, e.g., the proportion of the prey biomass available to the grazer (eqn. 20 in Ward et al., 2018) equals 0.8471 here.

Line 142: "species" is not appropriate given the trait-based model, "group" or "type" might be better.

Lines 161 – 163 / 325 / Appendix A:

- I am struggling to see the suggested effect of SST on chlorophyll around 20 degrees C on Figure A1. Arguably, the upper part of the distribution of chlorophyll begins to decrease around 20 degrees but the lower part of the distribution decreases from 10 degrees.
- I don't think you can conclusively conclude on the relationship with SST because Figure A1 also includes other factors that may be co-varying with SST, e.g., nutrient availability. To do this, I think you'd need to plot this with a constant nutrient concentration or vary temperature whilst keeping nutrient concentrations fixed.
- The net effect of temperature dependence is quite complicated. Nutrient uptake and grazing *rates* increase with temperature, however net nutrient uptake can be limited by nutrient availability leading to disproportionate effects depending on location. For example, the temperature effect of grazing is more likely to dominate in areas with lower nutrient availability. This effect needs to be factored into the explanation of why the impacts under RCP8.5 are less pronounced.

Line 169: the six oceanic layers should appear in the ecosystem section as this is a departure from Ward et al., (2018)

Lines 190 – 191 / 349 – 350: I think the authors are correct in their assertion that the ecosystem will spin up rapidly with the initial biogeochemical state. However, the ecosystem will have an impact on the biogeochemistry via a different uptake of nutrients and carbon and because this impact is broadcast to the deep ocean via sinking particulates it's likely there is a much longer drift in the biogeochemistry. It would help to have an additional experiment to quantify this drift and its impact on the simulations. The alternative approach is to perform a second coupled biogeochemistry-ecosystem spin-up to allow the biogeochemistry to adjust.

Section 4.1.1:

- The variation in b values of around 0.01 reported in Table 1 is incredibly small given the observed spatial variability in the ocean (0.4 to 1.4: Henson et al., 2012; Marsay et al., 2015) and projected future values with temperature dependent remineralisation (~0.25; Laufkotter et al., 2017). The percentage of POC sinking beyond 1000m, an indication of carbon sequestration, calculated from a Martin Curve with the min/max values in Table 2 ranges from 20.8% to 21.4%. Overall, this suggests a very minimal change in the Biological Carbon Pump in response to the light absorption.
- "…we compute [vertical POC fluxes] via a Martin curve…" – I'm totally sure what this means, did you fit a power-law curve to the vertical profile of POC fluxes predicted by

the model? If so, what did you use as the normalisation depth and does this include POC generated in the upper 6 depth levels? Generally, this is not as straight forward as suggested because an exponential curve has linear attenuation whilst a power-law has non-linear attenuation (Lauderdale & Cael 2021).

- is the exponential decay function normalised to the bottom depth of the euphotic zone of the model (assuming this is the bottom of the sixth depth level where light penetrates)?
- The authors seem to suggest the change in remineralisation is occurring in the surface, where I assume the adjusting ecosystem is driving that change, rather than changing the attenuation of POC fluxes across the water column. It would help to see a vertical profile of POC fluxes to confirm this. If this is true and the changes in b reflect this, then this is slightly conflating concepts of POC attenuation, as measured by b, and changing export efficiency (the ratio of export at some reference depth to production: f-ratio, see Henson et al., 2011).

Line 229: “more labile” – this infers POC has different reactivity in the model, is this true?

Figure 4: It might help to have some indication of how big these changes are relatively, i.e., compared to the overall final-preindustrial change, though I appreciate the comparisons are focused on the final state with and without the light absorption.

Figure 6 and Section 4.1.3: The spatial patterns in SST differences for RCP8.5 look to be different to the other scenarios. There is greater warming at the poles compared to smaller warming in the other scenarios which is an interesting feature that doesn’t seem to be discussed in the text.

Lines 286 – 289: It’s not clear here whether the quoted changes in the carbon pumps is from the previous paper or this study.

Lines 375 – 378:

- It would help to give a sense of this change relative to the overall change in the carbon cycle to support your suggestion that phytoplankton light absorption leads to major carbon cycle uncertainties.
- “This study highights a highly uncertain feedback on the carbon cycle that is missing from 50% of the CMIP6 models” – this is a crucial point for justifying this study which is left to the very end of the manuscript! This would be really beneficial to mention in the introduction.

References

Cael, B.B., Bisson, K., Boss, E. et al. (2023) Global climate-change trends detected in indicators of ocean ecology. *Nature* 619, 551–554. https://doi.org/10.1038/s41586-023-06321-z

Henson et al., (2011) A reduced estimate of the strength of the ocean’s biological carbon pump. *Geophysical Research Letters.* 38, L04606. doi:10.1029/2011GL046735

Lauderdale, J. M., & Cael, B. B. (2021). Impact of remineralization profile shape on the air-sea carbon balance. *Geophysical Research Letters*, 48, e2020GL091746. https://doi.org/10.1029/2020GL091746

Laufkötter, C., J. G. John, C. A. Stock, and J. P. Dunne (2017), Temperature and oxygen dependence of the rem- ineralization of organic matter, *Global Biogeochemical Cycles*, 31, 1038–1050, doi:10.1002/2017GB005643.

Ward et al., (2018) EcoGEnIE 1.0: plankton ecology in the cGEnIE Earth system model, *Geoscientific Model Development.* 11, 4241–4267, https://doi.org/10.5194/gmd-11-4241-2018