



EGUsphere, referee comment RC3
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Comment on egusphere-2022-91

Anonymous Referee #3

Referee comment on "Physiological flexibility of phytoplankton impacts modelled chlorophyll and primary production across the North Pacific Ocean" by Yoshikazu Sasai et al., EGUsphere, <https://doi.org/10.5194/egusphere-2022-91-RC3>, 2022

This paper is a nice update on a line of work that aims to bring a modern representation of physiological plasticity in phytoplankton into the mainstream of ocean biogeochemical modelling. I have followed FlexPFT from a distance for a number of years, and it is useful to have a concrete illustration of how it behaves, and how it behaves differently from standard models, in a realistically complex 3D ocean simulation. I have a number of comments about how the discussion and expression of results could be improved, but I would class these as minor revisions.

- The review of evolving representations of phytoplankton physiology in the Introduction is especially nice.
- line 111: W per m², not W per m³
- line 145-47: The role of parameter tuning in the comparison of the two model formulations is potentially very important. If the tuning of InFlexPFT had been done differently—for example, leaving mu_max the same, or lowering it further—would there have been a different pattern of similarities and differences between the 3D model runs? Which model shows a higher or lower growth rate at a particular point in space and time could be as much a matter of specific parameter choices as the structure of the equations. I would appreciate some comments on this point in the Discussion—what differences between Flex and InFlex are truly inherent and not contingent upon particular parameter choices.
- line 191: the success of FlexPFT at reproducing chl patterns seems to be largely a matter of dynamic range. Van Oostende et al. 2018 (<https://www.sciencedirect.com/science/article/pii/S0079661117302586>) also addressed this challenge in the North Pacific and found a solution by extending a standard

2-phytoplankton NPZD-style model to 3 phytoplankton compartments. So perhaps the poor relative performance of InFlexPFT is really highlighting the limitations of a 1-phytoplankton model. I think this requires some discussion (in the Discussion). If one is going to improve on inflexible plankton models by adding state variables, why add them in the form of flexible physiology instead of additional fixed-response phytoplankton compartments / functional groups? There is more at stake than simple statistical performance; to me the real issue is whether we think that the ocean achieves its wide dynamic range through acclimation and plasticity, or competitive exclusion.

- line 245: is there any way to make this comparison with observed variation in C:N more quantitative, or at least more specific? FlexPFT seems to show about four-fold variation in C:N over a vertical profile, if I am reading the results correctly—based on the references given in these lines, does this seem like roughly the right amount of variation, or too much?

- line 369: this feels like a weak comparison. What fraction of global PP _should_ the North Pacific account for? There is no additional information here, relative to Fig 9, on whether FlexPFT is a quantitative improvement over InFlex. Surely there are published estimates somewhere of North Pacific PP?