(egusphere-2022-91) entitled "Physiological flexibility of phytoplankton impacts modeled biomass and primary production across the North Pacific Ocean" by Sasai et al.

Response to Reviewer #3

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.

We thank Reviewer #3 for valuable comments on our manuscript. The major revised points are attached pdf file (red letters are major revised parts). Our responses (fine characters) to the Reviewer #3's comments (bold characters) are described below.

- The review of evolving representations of phytoplankton physiology in the Introduction is especially nice.

Thank you for your evaluation.

- line 111: W per m^2, not W per m^3

New Line 125: Yes, it's a mistake. Corrected from W per m^3 to W per m^2.

- 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.

New Lines 183-186: We added the following sentences after the explanation of growth equation in the Section 2.2.
"For example, the potential maximum growth rate, mu_max, is 1.5 (day^-1) for the InFlexPFT, compared to 2.2 (day^-1) for the FlexPFT. Increasing the potential maximum growth rate decreases the surface N concentration in the subpolar gyre, to the point of depleting nutrient during summer, while increasing the surface Chl concentration across the whole gyre.".

- 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.

In this study, we used one phytoplankton incorporated variable C:N:Chl ratios and photoacclimation. One phytoplankton in our model can handle multiple growth rates depending on available nutrients and light conditions. This corresponds to the ratio calculated by the BGC models consisting of multiple phytoplankton compartments (> 2 phytoplankton). Therefore, it is considered that the chlorophyll distribution in our model similar to the BGC model (Van Oostende et al., 2018) for calculating multiple phytoplankton with a fixed ratio was reproduced.

In general, multiple phytoplankton and zooplankton adapt to their environment and survive (e.g., growth, grazing by zooplankton, and export). Where to spend the cost is important in modeling (e.g., increasing compartments of BGC, and simplified BGC). In particular, it is important to reduce the simulation cost for the global BGC model, such as Earth System Model. In this study, we showed how much one plankton can reproduce. As future research, it is possible to introduce flexible physiology to the growth of multiple phytoplankton, or to predation by zooplankton.

- 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?

New Lines 454-462: We added this sentence in the Discussion section (new).
"Observed elemental ratios of phytoplankton and particulate organic matter in surface layers deviate substantially from the Redfield ratio (e.g., Goldman et al., 1979, Garcia et al., 2018b, Liefer et al., 2019). Molar C:N ratios of phytoplankton vary from 4 to 60 (mol C: mol N) between phylogenetic groups. However, our current implementation FlexPFT model includes only one phytoplankton type, which varies its C:N ratio as its acclimates to the varying light, nutrient and temperature levels. Sauterey and Ward (2022) found that nitrogen and temperature mostly determined variations in the C:N ratio of phytoplankton, with variable contributions from other factors across the North Atlantic. By contrast, we find that with the FlexPFT model applied to the North Pacific, nitrogen and light levels are the primary determinants of C:N:Chl ratios, with temperature playing a lesser role. Accurate modelling of Chl and PP patterns requires accounting for these dependencies to resolve the variable elemental composition and pigment content of phytoplankton."

- 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?

We deleted the sentence "On the other hand, … 10% of the estimated global primary production." compared with global estimation of PP.

Direct comparison between 3-D BGC model output and snapshots observed data at time series stations is difficult. In particular, the quantitative discussion is limited to the comparison of the temporal variation of the vertical profiles because it includes the influence of the uncertainty contained in the BGC model and the error due to the observed time and sea area (including depth). In this study, we are focusing on the difference in the reproducibility of the PP vertical profile between two models. The model itself still needs to be improved for quantitative discussion, so this is a topic for the future.