

(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 #1

The manuscript ***Physiological flexibility of phytoplankton impacts modeled biomass and primary production across the North Pacific Ocean*** by Y. Sasai and colleagues assess the importance of optimal nutrient uptake, photoacclimation and variable stoichiometry on the emergence of large scale chlorophyll and primary production patterns in the ocean, using the North Pacific as a testbed. To do that, the authors designed an experiment based on the comparison of two strategic NPZD biogeochemical (BGC) models coupled to a state of the art, eddy-resolving model of ocean physics (OFES2). One of the BGC models implements optimal uptake kinetics ([Smith et al 2009](<https://doi.org/10.3354/meps08022>)), whereas the other incorporates interactive, optimal photoacclimation (Chl:C ratios) and variable stoichiometry (C:N deviating from Redfield ratios) as proposed in the context of the FlexPFT theory of [Smith et al 2016](<https://doi.org/10.1093/plankt/fbv038>)). The experiments revealed clear deviations between predictions from both models, with a clear gain in accuracy when using the more complex FlexPFT model (bulk properties like Chl and fluxes like PP closer to observations, better resolution of the deep chlorophyll maximum (DCM), etc.). The authors conclude recommending the adoption of similar approaches by the BGC community.

The topic, approach and results are of great appeal and the manuscript is already an important contribution. The authors designed a clean test of their hypothesis about the emergence of surface and vertical gradients in phytoplankton growth and biomass, and the result clearly support their ideas. However, the manuscript is not easy to follow as it stands, especially the combined ***Results and Discussion*** section. The narrative in this section is quite descriptive, and it fails to provide a clear picture of the ability of each model to reproduce observed patterns. As commented below, these issues seem to arise in part from the lack of motivation and rationale for model assessment in ***Methods***. There are other aspects missing like the limitations of the current model (***e.g.*** what about nitrogen fixation?), alternative explanations to the emergence of DCM ([Cullen 2015](<https://doi.org/10.1146/annurev-marine-010213-135111>); there may be more recent reviews), or about the performance of similar BGC models in simulating Chl and PP in the Pacific. Together, these issues led me recommend a major revision of the manuscript. I provide some major concerns, and a long list of minor issues and suggestions below. I hope the authors find both lists useful.

We thank Reviewer #1 for valuable comments on our manuscript. The major revised points are 7 items below. The individual responses (fine characters) to the Reviewer #1's comments (bold characters) are described after the list of major 7 items.

1. Title changed “Physiological flexible of phytoplankton impacts modeled chlorophyll and primary production across the North Pacific”. Because phytoplankton biomass changed chlorophyll.

48
49 2. Introduction

50 We added the aim and objective in the last paragraph of Introduction.

51
52 3. Methods and Materials

53 We revised the two subsections “2.2 Formation of Phytoplankton Growth in the
54 Biological Model” and “2.3 Observed Data” in section 2.

55
56 In subsection 2.2, we changed from simple phytoplankton growth rate (InFlexPFT)
57 description to complex phytoplankton growth rate (FlexPFT) description.

58
59 In subsection 2.3, we revised the description of observed data to compare the model
60 results. The observed year and modeled year are not same, but they are compared to
61 confirm the reproducibility of the model climatological averaged field (e.g., season).
62 We revised the first paragraph of subsection 2.3.

63
64 4. Results

65 We changed section title from “Results and Discussion” to “Results”.

66 We changed three subsection titles.

67 Title of subsection 3.1 is “Comparison of Surface Chl Pattern”.

68 Title of subsection 3.2 is “Comparison of Vertical Distributions of Chl and PP
69 along Two transect Lines”.

70 Title of subsection 3.3 is “Vertical Profiles of PP at three stations and PP pattern”.

71 We added the quantitative description in section 3, everywhere.

72
73 5. We added the “Discussion” and revised the “Conclusion”.

74
75 6. Figures

76 Observations points (three stations and two lines) are correctively shown in Figure 1.
77 Two transect lines (JMA and JODC observation lines) are shown in Figures 1c, 1g,
78 and 1k in summer map. Three time series stations (Station K2, Station S1, and Station
79 ALOHA) are shown in Figures 1d, 1h, and 1l in winter map.

80
81 The numbers of figures have also been changed because the order of descriptions of
82 Figures 8 and 9 in the body text has been changed. Figure 8 (old) changes Figure 9
83 (new), and Figure 9 (old) changes Figure 8 (new).

84
85 7. Appendix A: NPZD model

86 We added the grazing of phytoplankton by zooplankton equation, $G(P)$, in Appendix
87 A.

88
89 -----
90 ***Major comments***

91
92 **1. The manuscript is in general well written and structured, but there are two**
93 **sections that in my opinion deserve another thought, namely *Methods and**
94 **Materials* and the *Results and Discussion* (see next three points). Although the**

description of the models is in general easy to follow (see however some minor suggestions below), the fact that the text moves from a complex model to a simpler one is not an optimal choice. I recommend the authors to present first their general approach with the components that are common to both models, and then detail first the simpler model featuring just optimal uptake followed by the more complex model featuring also photoacclimation and variable stoichiometry. I am aware that this might read as a minor issue, but I think it is quite important to ensure that readers realize that, despite their names, FlexPFT is something more complex than InFlexPFT. It is not clear whether one model is a nested version of the other or not (in the sense of a simpler formulation or the result of setting from variables as a constant). For instance, there is certain temptation to just check Table 1 and conclude that InFlexPFT results in reduced Chl and PP when compared with FlexPFT just because μ_{max} is lower in the former. There is also some confusion about whether the model implements only photoacclimation or photoacclimation and variable stoichiometry, and about whether one or both of them are simple NPZDs or not.

Thank you for your comment. Following your suggestion, we have revised the two sections, “Methods and Materials” and “Results and Discussion”. “Methods and Materials” section was revised including minor comments. “Results and Discussion” section was divided into “Results” and “Discussion (New section)”. Please check the attached file.

2. Merging *Results* and *Discussion* has certain risks. In my opinion, that section of the manuscript needs to dig a bit more into the results and provide more quantitative tests that enable readers to assess the relative merits of each model and to frame the results in the context of similar work. The text reads well, but it lacks any quantitative comparison except toward the end, when the authors comment on the huge variability of available NPP estimates and provide large scale estimates for the overall production of the North Pacific. The manuscript would benefit from a more systematic assessment featuring regional averages (say, at the biome scale?) and some kind of statistical metric or test.

Thank you for your comment. Following your suggestion, we have revised the “Results” section based on the above and the minor comments below. Please see the attached pdf file.

3. There may be other things to say about the choice of the data for the comparison, and about how model output was preprocessed (for instance, how did you process Chl profiles, was there any attempt to mimic the way the ocean color satellites operate?). Since the simulations were forced using JRA-do reanalysis data, one would expect that the target for the models would be to reproduce or match available data.

Since the 2000s, sea surface chlorophyll data has been accumulated for the last 20 years, and it is possible to analyze the seasonal variability. On the other hand, the vertical profiles of chlorophyll and PP in-situ observed data are mostly snapshots of limited places

(time series stations, etc.) and observation lines, and there are few that are spatiotemporally aligned like nutrients and temperature such as WOA database. Since the 2000s, comparable data have been published in the North Pacific and used in model validation. The observational data used for comparison in this study is not sufficient, so it is necessary to prepare publicly available data in order to analyze variations over several days to decade.

New Lines 200-205: We have revised the text to clearly state, “The last 20 years (2000-2019) average of model results were compared with satellite data, in-situ observations, and the climatological data (Chl, nitrate, and temperature). Although the model and observation periods differ somewhat, using the satellite and in-situ observation data observed during the simulation period (2000s), we compare whether the horizontal and vertical patterns of climatological seasonal variations can reproduce the patterns captured by the satellite and the snapshot observations. Especially, we focused on the Chl and PP patterns, which strongly reflect effects of the different assumptions about how growth rates depend on light and nutrients.” at the beginning of the Observational Data section.

It is not clear what was the aim and objectives of the study, and perhaps that explanation is the only thing missing. The objectives and the rationale for choosing some data and patterns over potential alternatives needs to be justified. The models seem to be doing more than decent job, but the authors need to clarify to what extent some of the apparent biases observed both in surface and subsurface fields reflect are due to biases in simulated physical and chemical conditions or to differences on the phytoplankton model.

New Lines 80-90: We the text to read, “Most of biogeochemical models have similar structure, with nitrogen as the main currency for a simplified food-web, which generally includes phytoplankton and zooplankton, and a regeneration network with detritus, dissolve organic nitrogen, and various nutrients (i.e., Fasham et al., 1990). Whereas the more complex biogeochemical models have become more common (e.g., Follows et al., 2007, Totterdell, 2019), simple phytoplankton growth (fixed stoichiometry, without photoacclimation) models are still applied widely. In this study, we focus on the acclimative growth response of phytoplankton as incorporated in these models. To evaluate the performance and implications of this acclimative response of phytoplankton growth to varying light and nutrient conditions across the North Pacific Ocean, we compare modeled chlorophyll and primary production from an inflexible phytoplankton control model (InFlexPFT), which assumes fixed C:N:Chl ratios (fixed stoichiometry), to a recently developed phytoplankton model (FlexPFT, Smith et al., 2016), which incorporates photoacclimation and variable C:N:Chl ratios. We apply these two phytoplankton models in a 3-D eddy-resolving ocean circulation model of the North Pacific, to assess each model's performance compared to observations of chlorophyll and primary production.” In the last paragraph of the Introduction.

4. Finally, a key aspect that the authors need to make clear earlier in the paper is the feasibility that proposed and discussed mechanisms may be actually working in the field. There is room to discuss alternative mechanisms currently ignored by the two models assessed. For instance, interactions between grazers and phytoplankters,

potential biases in export and recycling, the metabolic diversity of phytoplankton (*e.g.* nitrogen fixers, picophytoplankton), etc.

In this study, we compared the two models, each with only one phytoplankton type: the FlexPFT incorporating variable C:N:Chl ratios and photoacclimation, and the InFlexPFT assuming constant composition without photoacclimation. With the FlexPFT, a single phytoplankton type adjusts its growth rate (i.e., acclimates) depending on available nutrients and light conditions. On the other hand, the InFlexPFT does not account for this physiological flexibility, and therefore either light or nutrient limitation tends to reduce growth rates more with the InFlexPFT compared to the FlexPFT. Also, we ignored other biological processes (e.g., interactions between grazers and phytoplanktons, export and recycling) of BGC model.

New Lines 480-482: We have revised the text to read, “In addition, we will proceed with research on introducing flexible physiology to the growth of multiple phytoplankton, as well as associated food quality effects on predation by zooplankton, and the uncertainty of other biological processes, such as nitrification, grazing, mortality, export and recycling.” in the last paragraph of the Conclusions section.

5. As a bonus question, although it does not seem central to the study at hand, the formulation of zooplankton grazing was quite intriguing for two reasons the deserve further comment;

1. the numerical response seems to be nonstandard and deserves further comment, as well as the closure term

We added zooplankton grazing equation, $G(P)$, and explain this equation in Appendix A.

2. Eq (A2) in L429 includes a quadratic mortality term for phytoplankton. That effectively means that phytoplankton dynamics follow logistic growth, which seems redundant with the formulation of phytoplankton growth as a function of available nutrients, and underwater light and temperature conditions, and with the common assumption of a population controlled though grazing by zooplankton. Perhaps I am missing something?

In Eq. (A2), the time-varying formula for P is the growth rate, respiration, mortality, extracellular excretion (a part of growth rate returns N), and grazing by Zooplankton. Since this formula changes only μ contained in the section 2.2, it does not overlap with the growth rate formula pointed out in the comment. Our previous explanation was difficult to understand; therefore, in the revised manuscript we have added a symbol μ_InFlex or μ_Flex after μ in appendix.

Minor comments

Abstract

L001 - active voice? [Light and nutrient conditions ...]

New Line: We have revised from "...biomass to changing light and nutrient conditions..." to "... biomass to changes in light and nutrient availability...". Phytoplankton biomass is passive response.

L002 - define photoacclimation?

New Line 2: We have defined "photoacclimation" in the second sentence of the revised abstract as follows: "...photoacclimation, i.e. the dynamic physiological response of phytoplankton to varying light and nutrient availability (variable chlorophyll: carbon ratios)"

L002 - at the end your model features both photoacclimation and variable stoichiometry, perhaps it is worth highlighting it

New Line 2: Yes, it is highlighting message in our manuscript. As mentioned in the comment above, we have added the definition of "photoacclimation", and we now state clearly that the FlexPFT accounts for both photoacclimation and variable composition.

L003 - break the sentence at the comme (it is already a bit twisted), and perhaps join with the next one?

New Lines 4-5: We revised from "... their application and testing against oceanic observations remain limited." to "... their application and testing against the observed flexible response of phytoplankton communities remains limited.".

L004 - as commented above, I recommend to go from simple to complex [say optimum nutrient uptake PFT to full FlexPFT], and provide a one sentence description with general details about the two models

New Lines 6-9: As mentioned in the comment, we revised "We compare modeled chlorophyll and primary production from an inflexible control model (InFlexPFT), which assumes fixed carbon (C):nitrogen (N):chlorophyll (Chl) ratios, to a recently developed flexible phytoplankton functional type model (FlexPFT), which incorporates photoacclimation and variable C:N:Chl ratios.".

L007 - mention OFES2 by name [and acronym]?

OFES2 is not mentioned in abstract, but it is described in the body text (Section 2).

L010 - [...] subsurface Chl maxima *in the subtropical gyre* [to provide context]. Otherwise please detail where exactly that happens (especially the overestimation of Chl). As commented above, a figure detailing the magnitude of deviations with satellite data would be very useful.

New Line 12: We add "in the subtropical gyre" after "subsurface Chl maxima".

As a general comment about the abstract; data used for validation is not mentioned at all. Readers might just assume you are testing your model against "oceanic observations" [L003], which may be too vague

New Lines 10: We added the "(e.g., satellite imagery and vertical profiles of in-situ observations)" after "We coupled each phytoplankton model ... and evaluate their respective performance versus observations". As the [L003] comment, we revised it. The term "oceanic observations" was ambiguous, so we revised it to "observed flexible response of phytoplankton communities".

Introduction

L035 - not sure if there is something else besides pursuing efficiency and simplicity ;) ...

Yes, I removed "for the sake of computational efficiency and simplicity."

L077 - if InFlexPFT is a typical NPZD, then call it NPZD, or state here too that it is an NPZD implementing optimal uptake kinetics as per Smith et al 2009 [L135ff)?

New Lines 84-90: As the [L077] and [L080] comments, we revised it as follows:
"In this study, we focus on the acclimative growth response of phytoplankton as incorporated in these models. To evaluate the performance and implications of this acclimative response of phytoplankton growth to varying light and nutrient conditions across the North Pacific Ocean, we compare modeled chlorophyll and primary production from an inflexible phytoplankton control model (InFlexPFT), which assumes fixed C:N:Chl ratios (fixed stoichiometry), to a recently developed phytoplankton model (FlexPFT, Smith et al., 2016), which incorporates photoacclimation and variable C:N:Chl ratios. We apply these two phytoplankton models in a 3-D eddy-resolving ocean circulation model of the North Pacific, to assess each model's performance compared to observations of chlorophyll and primary production."

L080 - perhaps deter giving the full name and details of OFES2 to MatMet?

We moved the sentence of full name and details of OFES2 to "Methods and Materials". We revised from "... coupled physical-biological model of the North Pacific, consisting of the OFES2 including..." to "... coupled physical-biological model of the North Pacific, consisting of the physical ocean model (OFES2, namely the Ocean general circulation model For the Earth Simulator) coupled with a simple nitrogen based Nitrate-Phytoplankton-Zooplankton-Detritus (NPZD)..." in the first paragraph of the Section 2.1.

Methods and Materials

L085 - I understand you extensively modified the simple NPZD to implement either OU kinetics or FlexPFT. I mean, perhaps it is worth mention it and state that the default configuration consisted in a simple NPZD? [or maybe just provide those details later when talking about the BGC component of the model]

In this study, we only changed the term of phytoplankton growth rate in the simple NPZD model. I don't think it is necessary for readers who understand the BGC model, but readers who are not very familiar with the BGC model or who will conduct research to discuss the uncertainty of phytoplankton growth rates and other biological processes (e.g., grazing, mortality, export and recycling, and nitrogen fixation) in the future. I think a short NPZD model description would be helpful for them.

L089 - I would put all details about the configuration of the experiments together [L101]. The sentence in L93 is especially intriguing and disconnected from the rest of the explanation [L104].

New Lines 103-104: We revised from “The last day of 1979 is used for the initial physical fields for this simulation.” to “The last day of 1979 is used for the initial physical fields for performing coupled physical-biological model simulation.” in [L93].

New Lines 118-119: We revised from “Two NPZD models are incorporated after the last day of 1979 of the OFES2.” to “Two NPZD models are incorporated after the last day of 1979 of the physical fields in the OFES2” in [L104].

L113 - Eq (1): perhaps $Q(I,T)$ instead of just Q ?

In Section 2.2. Yes, you are right. Q and f_v are functions of I , T , and N . In the explanation of the formula, we changed Q and f_v to $Q(N,I,T)$ and $f_v(N,I,T)$.

L119 - Eq (2): I really did not like the symbol $\mu_{\text{Flex}}(I,T)$, it seems potentially confusing ... what about just μ_{max} , $S(I,T)$, $F(T)$ or $\mu_{\text{Flex}}^*(I,T)$ [where I would suggest the former] ... otherwise it may lead users to think you need some functional or to iteratively solve the equation?

Thank you for your suggestion. We changed the symbol “ $\mu_{\text{Flex}}(I,T)$ ” to “ $\mu_{\text{max}} S(I,T) F(T)$ ” in Eqs. 5 and 7 in Section 2.2.

L124 - Please detail how do you determine maximum affinity [if it is optimized on a daily basis, etc]

New Lines 178-180: These parameter values were determined by tuning the model to reproduce the seasonal and spatial variability of N and Chl in the near-surface of the North Pacific. We revised “Parameter values, μ_{max} , V_0 , A_0 , and α (Table 1) used in Eqs. 1 to 7 for the phytoplankton growth rate were tuned, separately for each coupled model, to reproduce the seasonal variability of N , and Chl in the near-surface of North Pacific.”. after explanation of equations.

L131 - please detail how the optimal value of θ is updated [I mean, that it is not a constant]

Formula θ is so complicated that we will not go into details here, but just cite two papers (Pahlow et al., 2013 and Smith et al., 2016). The optimal value of θ is calculated and applied only when irradiance I is greater than the threshold irradiance; otherwise, when light levels are insufficient to justify the respiratory cost of chlorophyll synthesis, the model assumed that no new chlorophyll is produced. In the latter case, θ is set to a constant value (no photoacclimation).

New Lines 174-176: We have revised by adding: “The optimal value of Chl:C ratio in the FlexPFT is applied when irradiance I exceeds the threshold irradiance, below which the respiratory cost outweighs the benefits of producing chlorophyll (Pahlow et al., 2013 and Smith et al., 2016).”

L137 - ideally, it would be nice to see how one can go from Eq (7) to Eq (1) [if that is possible], Otherwise it may be worth stating whether the models are truly nested or they just feature different terms for nutrient the dependent growth [though they propagate to the other terms in FlexPFT]

In the InFlexPFT, N-limitation and light-limitation have independent (multiplicative) effects on growth. In the FlexPFT, the trade-off between light- and nutrient- acquisition is built into the formulations, resulting in inter-dependent effects. Therefore, there is not clear way to migrate simply from the InFlexPFT growth equation to the FlexPFT growth equation. Here, we will keep the difference in the formula of growth and leave any such derivation for future work.

L153ff - perhaps explicitly include formulas for Chl and PP [$\text{Chl} = P \cdot \theta$, $\text{PP} = \mu_{\text{Flex}} \cdot P \cdot 1/Q$], etc]

New Lines 193-198: We have added “ $\text{Chl} = P \cdot \theta/Q$ ” and “ $\text{PP} = \mu_{\text{Flex}} \cdot P \cdot 1/Q$ ”. etc.

L155 - I also miss some details here about what kind of outputs were compared to observations. In principle, since JRA-do is a reanalysis dataset, you might expect a direct match between simulated fields and observations at sea.

Many studies of physical fields using OFES2 output have been carried out. In particular, the reproducibility of OFES2 has been reported in Sasaki et al. (2020) and others. Here, we want to discuss the difference in phytoplankton biomass and production depending on the growth formula, so we keep it to the minimum verification for physical fields (temperature and nutrients).

L165 - perhaps follow the order physics, chemistry, biology? Again, missing some rationale for the choices and the way the model was evaluated

Verification of model results requires data (physical, biogeochemical fields) on various spatiotemporal scales. Verification of modeled temperature and nutrient distribution, which are functions of growth, is carried out only in comparable cross-sections (2 lines). In this study, we discuss seasonal variability of climatological values in the biological fields as an example. Impacts not considered this time (circulation, mixing, etc.) will be discussed in the future.

Results and Discussion

-> ***general comment*** as an author myself I can understand the preference for pooling the two sections, as a reader I am not such a fan.

We divided this section into “Results” and “Discussion”.

L174ff - it seems that the physical component of the model was evaluated elsewhere; if that is the case it would be better to explicitly state so, but it would be ok to go beyond the ability of the model to reproduce major circulation features to mention at least its skill in reproducing temperature and nutrient fields.

New Lines 225-226: We added “In addition, the seasonal variability of T and N fields in the near-surface over the North Pacific are also well reproduced (not shown).” between “The ed-resolving ocean ... mesoscale eddies, and upwelling events.” and “These physical processes ...”.

L185 - there is certain paradox here since the initial focus of the manuscript on phytoplankton biomass and productivity mutates here on a large section devoted to two sections devoted to chlorophyll (which, needless to say in the context of a photoacclimation paper, is not biomass)

New Lines 229-238: We revised the description of results section in the first paragraph before the 3.1 section.

From “Here we focus on the different assumptions about how phytoplankton growth rate depends on ambient nitrogen concentration and light intensity. First, the reproducibility of seasonal and horizontal Chl distributions is described. Next, we compare the results of the two coupled physical-biological models in terms of Chl and PP along two vertical transects (north-south and east-west, respectively) in the North Pacific, and discuss the reasons for the differences. Finally, the difference in PP as calculated by these two models over the North Pacific is also discussed.”

to “Here we focus on the different assumptions about how phytoplankton growth rate depends on ambient nitrogen concentration and light intensity. First, the reproducibility of seasonal and horizontal Chl distributions is described. As the Chl concentration in the FlexPFT is calculated from $P \times \theta / Q$, and reflects the changes in θ and Q , we examine how variations in C:N:Chl ratios impact the surface Chl pattern. Next, we compare the results of the two coupled physical-biological models in terms of Chl and PP along two vertical transects (north-south and east-west, respectively) in the North Pacific,

and discuss the reasons for the differences. Especially, the role of photoacclimation in the formation of SCM and the growth rate on the variable C:N:Chl ratios of phytoplankton. Finally, the difference in PP as calculated by these two models over the North Pacific and the comparison with limited PP vertical profiles are discussed. The extent to which the different growth rate (InFlexPFT vs FlexPFT) affects the estimated PP is described.”.

L185 and 200 - perhaps the titles of these sections should somehow detail that they refer to model to data intercomparisons (w.r.t. the section starting on L321)

We changed two section’s titles.

1. New Line 261: “3.2 Comparison of Vertical Distributions of Chl and PP along the Two Transects Lines”
2. New Line 386: “3.3 Vertical Profiles of PP at Three Stations and PP Patterns” .

L190 - please provide some quantitative summary of deviations between models and obs

New Line: We added quantitative discussion in “Results” section (see attached pdf file).

L200ff - the narrative here becomes a bit difficult to follow to me ... perhaps an alternative structure [grouping results per biome for instance], or just a diagram or table summarizing the main findings [obs, simulated patters, most important regulating factor, etc] might make the section easier to follow

We revised the “Results” section. See attached file.

L277 - why? Is it possible to partition the amount of variation due to each to I, N and T?

It is possible to separate and present the limitation factors. However, here we examine the differences by simultaneously plotting the three factors (I is different symbol, N is horizontal axis, and T is color) that control the growth rate. In addition, we explain the effect of the C: N ratio in the FlexPFT.

L327 - horizontally and vertically? Could you develop a bit more what you mean?

New Lines 408-410: We added “At the gyre boundary, in addition to the surface, primary production is greater compared to other regions. Because the nutricline depth (close to the base of the euphotic layer) and the light intensity are optimal for the spring production.” after “... the spring bloom occurs both horizontally and vertically.”.

L346ff - I think this paragraph belongs to the previous section

New Lines 386-426: We changed the order of this section and the previous section. Old Figure numbers 8 and 9 changed to new figure numbers 9 and 8.

Conclusions

L375 - perhaps *acclimation* instead of *adaptive response*?

New Line 469: We revised from “adaptive response” to “acclimation”.

L409 - perhaps the IA abbreviation can be omitted here to detail instantaneous acclimation

We deleted “IA” as the comment.

Appendix A

L429 - please note comment above about Eq (A2)

Same response to Major comment 5.

L435 - please detail the type of numerical response (*i.e.* no need to force interested readers to go to Sasai et al 2016). My excursion to that paper suggest it is a Gompertz function with a threshold ... did not seem entirely standard (I mean, a commonly used formulation).

We added G(P) equation (A6) after A5.

Figures

Figure 1 - nice maps! Some suggestions doubts to comment in the text;

- add transect lines to panels c and k?

We added transect lines to Figs.1 c and k.

- what happened in the Gulf of Alaska and at Bering Sea?

In these regions (low Chl), the modeled nitrate concentration in the surface layer is depleted (not shown). Possibility, in the shallow Bering Sea (East side), nutrient supply is little by the modeled physical processes (e.g., circulation or tidal mixing between marginal sea and open ocean) or is no river inflow (not include). Therefore, the physical model needs to be improved.

- the distinct shape of the gyre suggests there may be underlying biases in ocean physics propagating to chl [what about simulated MLD?]

The model MLD reproduced its seasonal variations. Comparing OFES2 with WOA, the climatological MLD in the model tends to be deeper (25 – 50 m in March) in the boundary

of two gyres (not shown). In this study, we are comparing by climatological value, so we would like to discuss the Chl biases due to the mixed layer in future.

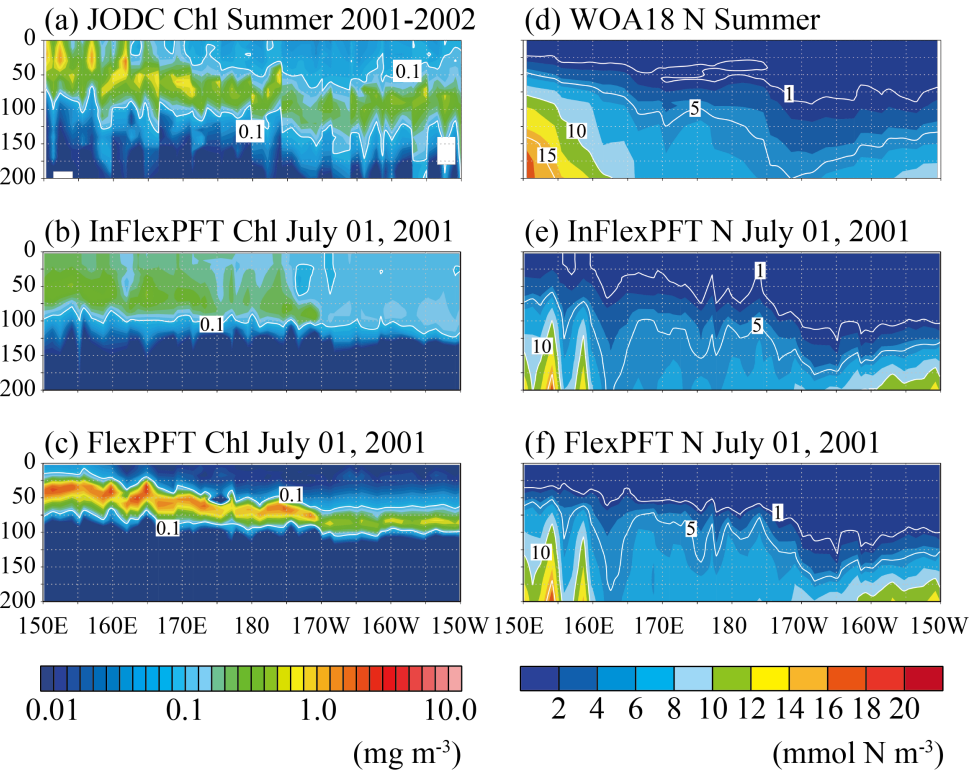
Figures 2 and 3 - again nice figures and amazing results

- physics, chemistry, biology? [order of columns]

It is shown in order to emphasize the difference in reproducibility of the vertical distribution of chlorophyll. The reproducibility of temperature and nutrient distributions depends on the physical fields of the model. Temperature and nutrient environments are important for biomass such as chlorophyll, but the formula is controlled by BGC model, and even if the temperature and nutrient environment is the same, the bias becomes large.

- why not directly comparing data for 2002/2003? [it would be nice to check whether the model reproduces small scale heterogeneity]

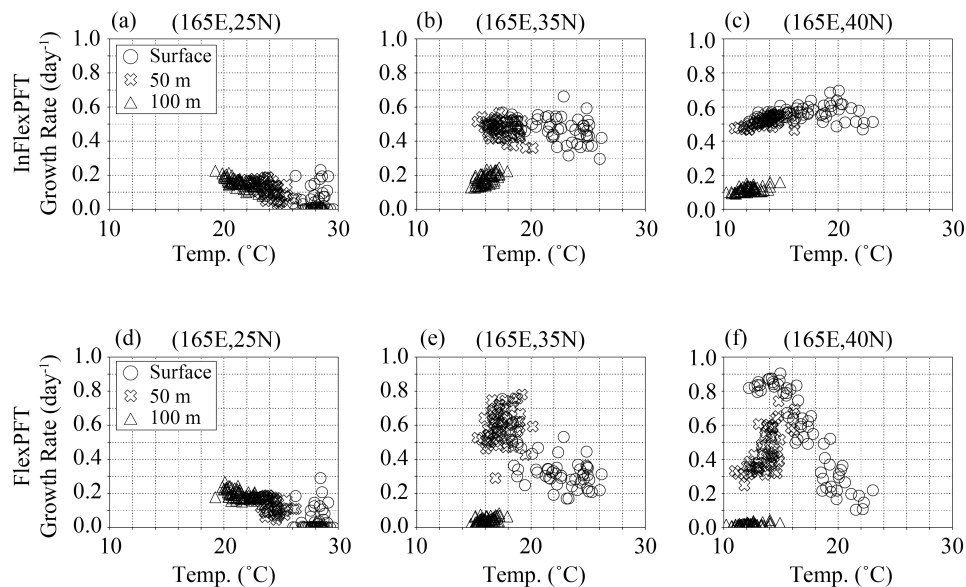
In the data of JODC, the observed location, depth, and time are different. Perhaps it is capturing variations of chlorophyll on small scale. We have checked the similar patchiness of daily mean chlorophyll (Simulated date is July, 1st, 2001) pattern in the model along the same transect line (See attached file). Its chlorophyll pattern in the FlexPFT model reflects the effect of small variability of vertical nitrate pattern (1 mmol N m⁻³). Even in the InFlexPFT model, a smaller scale pattern can be reproduced, but the SCM is not clear. This is future study to investigate the impact of smaller scale process on the chlorophyll distribution.



Figures 6 and 7

- I like the figures but still feel they fail to clearly convey whether N and I are more important than T ... How would an equivalent figure with T in the abscissa look like? How can you partition which variable contributes more variability?

T-limitation is also important around 20 degree C, which is reference temperature in T-limitation equation, (in the subtropical region) for growth rate. Especially, in the subsurface layer (50m in below figure), Figures 6e and 7e show the higher growth rate compared with the that of surface layer (same N concentration and strong light intensity). Figure (e) (165E, 35N), where is the boundary between two gyres, shows the high growth rate around 20 degree C in the subsurface layer (50m). At other locations, the effect of T-limitation for growth rate is smaller than the N- and light-limitations.



In the east-west line, the similar pattern shows below. As the locations in Figures (d) and (e) are close to (165E, 35N), and the FlexPFT growth rate is the highest around 20 degree C.

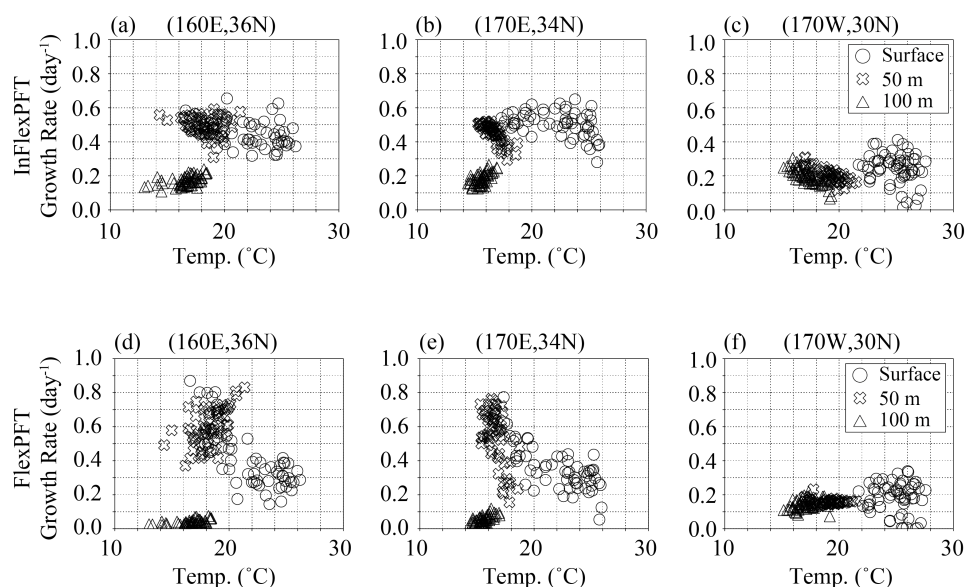
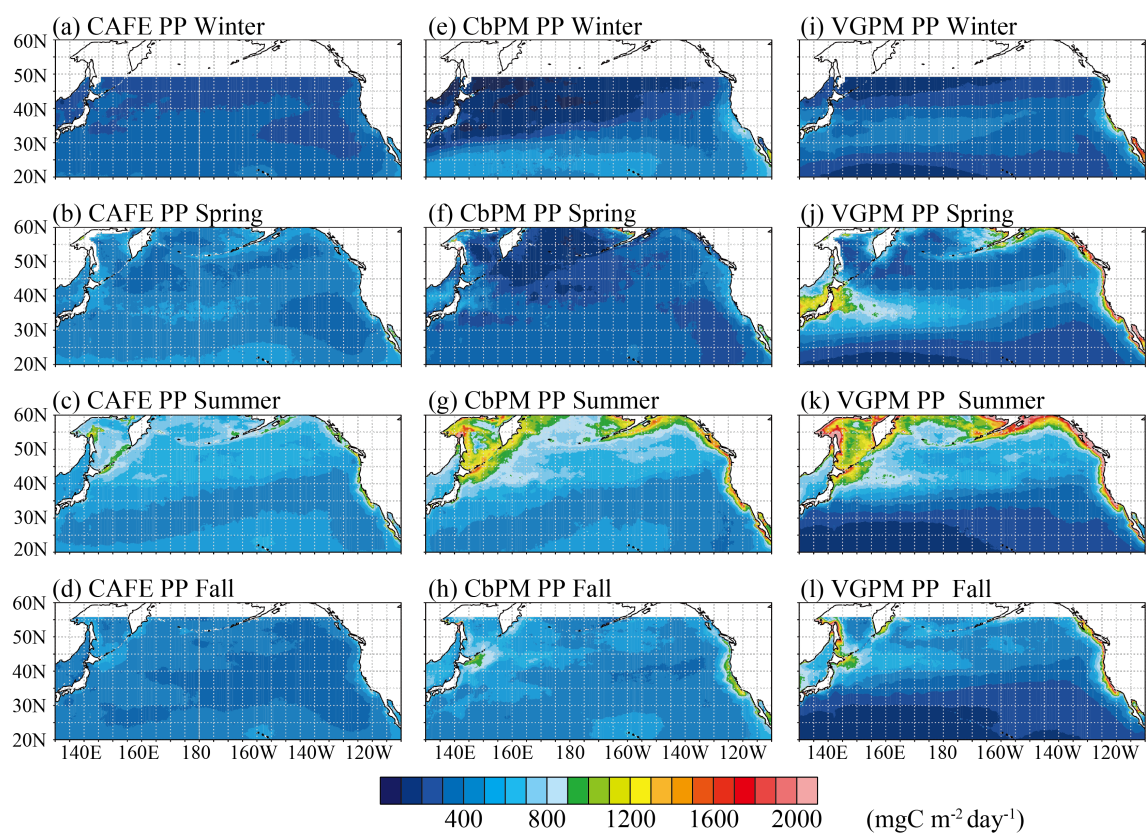


Figure 8

I think the results for FlexPFT would compare well with satellite based NPP products. Indeed, it would be great if, beyond biases in InFlexPFT the authors can show that actually the simpler model fails to capture large scale gradients [or at least, that is the impression I got].

Satellite based NPP has a large range depending by the formula of NPP to estimate (e.g., Kulk et al., 2020). Attached figure (below) shows the seasonal variability of NPP distribution map with three different (CAFE, CBPM, and VGPM) estimates using satellite data for comparison of modeled estimation. CAFE is the Carbon, Absorption, and Fluorescence Euphotic-resolving net primary production model, which is an adaptable framework for advancing global ocean productivity assessments by exploiting state-of-the-art satellite ocean color analyses and addressing key physiological and ecological attributes of phytoplankton (Silsbe et al., 2016, <https://doi.org/10.1002/2016GB005521>). CbPM is the Carbon-based Production Model, where inorganic carbon is fixed by photosynthetic processes (Behrenfeld et al., 2005, <https://doi.org/10.1029/2004GB002299>). VGPM is the Vertically Generalized Production Model, which is a chlorophyll-based model that estimate net primary production from chlorophyll, available light, and the photosynthetic efficiency (Behrenfeld and Falkowski 1997). These data are from Ocean Productivity web site (<https://sites.science.oregonstate.edu/ocean.productivity/>).

Due to the large variability of three satellite based NPP map, only the differences between the models are shown in this study.



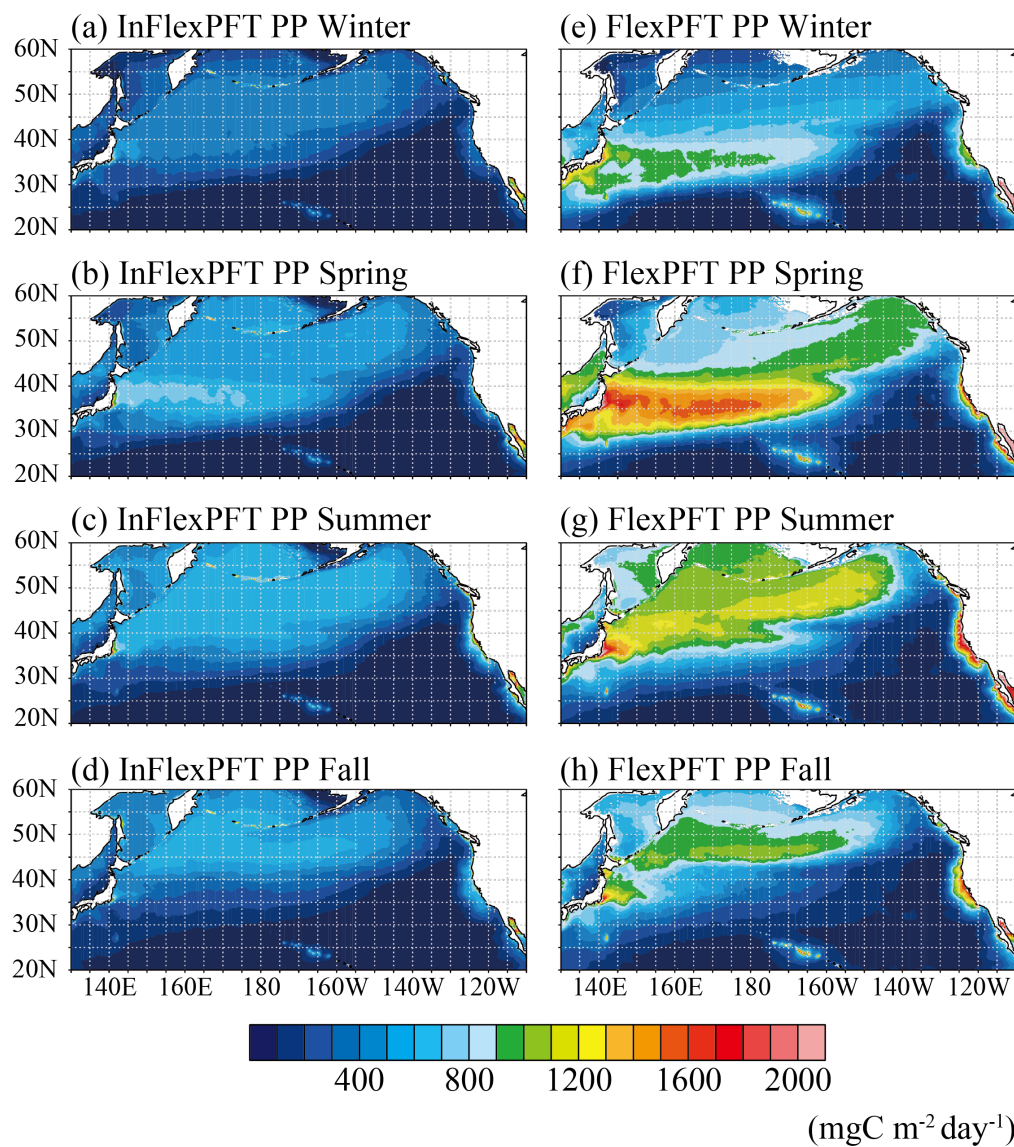


Figure 9

Is it possible to complement these profiles with a time series plot? [perhaps the monthly climatology at each site]

New Line 387: Modeled data shows the daily mean during 2000 to 2019. We added “modeled daily mean” in the first sentence of section 3.3.