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Reply on RC2

Tianfei Xue et al.

Author comment on "Mixed layer depth dominates over upwelling in regulating the seasonality of ecosystem functioning in the Peruvian Upwelling System" by Tianfei Xue et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-113-AC2, 2021

Dear reviewer,

We would like to thank you for your valuable feedback and your supportive and constructive comments. Your review has helped to improve the clarity of our manuscript.

The authors investigate the mechanism associated to the seasonal variability of phytoplankton biomass in the upwelling system off Peru (PUS) based on a regional biogeochemical coupled model. Their focus is on understanding the apparent "paradox" associated with the fact that there is an out-of-phase relationship of seasonal surface chlorophyll concentrations and upwelling intensity, which is a unique feature of the PUS compared to the other Eastern Boundary Upwelling systems as they illustrate. Their model result indicates that minimum chlorophyll in austral winter during the upwelling season is mostly associated with the enhanced vertical dilution and stronger light limitation of phytoplankton biomass growth due to the deeper mixed-layer at that season. They estimate all the tendency terms at seasonal timescales of the tendency phytoplankton biomass (i.e. tendency of the phytoplankton biomass) to quantify second-order processes (like reduced phytoplankton growth due to enhanced upwelling of cold waters and lateral advection). They then discuss their results in light of previous works and extend the discussion to implications for understanding net offshore export of phytoplankton biomass. This led us to hypothesize that mixed layer processes along the coast of certainly important for understanding ecosystem functioning.

The paper is interesting and has a sound methodological approach. It provides a synthesis of previous works dealing with this seasonal paradox and extends them nicely through a more in-depth analysis of the processes at work (i.e. detailed budget of phytoplankton biomass) and the broader scope though comparison with other EBUS. It is also well written a pleasant to read.

I have only minor comments mostly related to details in the methodology that I consider worth addressing considering that some results presented here are certainly somehow model-dependent.

R: In reply to your comments, we will put additional effort into clarifying the methodology and note where results might be particularly model dependent. Also, we will extend our discussion with regard to climate change.

Please find our point-by-point responses below:

L58-60: *QuickSCAT only cover the periods 1999-2008 so which wind forcing is used to cover the period 1990-2010? please clarify if this in an hindcast run or a climatological simulation.*

R: Thanks for pointing this out. In the revised manuscript, we will clarify that this study is based on a climatological simulation. We employed the QuickSCAT wind forcing average over the period 1999-2009. We will rephrase the sentence in the manuscript to (L46-48):

"We use a climatological simulation of the three-dimensional regional ocean circulation model CROCO (...) coupled with the biogeochemical model BioEBUS (...) for this study."

L70-73: *please indicate that the BioEBUS model was first used to simulate the Peru biogeochemistry by Montes et al. (2014) Montes L, B. Dewitte, E. Gutknecht, A. Paulmier , L Dadou, A. Oschlies and V. Garçon, 2014: High-resolution modeling the Oxygen Minimum Zone of the Eastern Tropical Pacific: Sensitivity to the tropical oceanic circulation. J. Geophys. Res.-Oceans. 119, doi:10.1002/2014JC009858*

R: Thank you for pointing us to the first study that employed BioEBUS in the Peruvian system. We will include the reference in the Methods section when introducing BioEBUS.

L78-79: *"In this study, we use monthly output of the final five years for our analyses (years 26-30)" It is not clear with which forcing the spin-up is done and if is this is a repetitive selected year. Earlier it is mention that the simulation covers the period 1990-2010 so to which actual years correspond years 26-30?*

R: We apologise for not having been more clear about the model set-up. We used the same climatological forcing for the spin-up and output years. No actual years correspond to years 26-30 of the simulation. We will rephrase the sentence in the manuscript to (L78-79):

"We run the model in total for 30 climatologically forced years and use the last five years for the analyses."

Section 2.2.: *It is not clear if all the terms associated to BIO are calculated on-line or off-line. If this is off-line, the use of monthly-mean mixed-layer depth for the vertical integration could yield errors that would be worth estimating. Mixed-layer depth can vary sharply at high-frequencies.*

R: We will clarify in the manuscript that the fluxes (in mmol N m^{-3} s^{-1}) associated with BIO were saved as output from the model for each grid box as monthly averages and later (offline) integrated over the MLD using the croco-tools, a collection of Matlab scripts that are provided on the CROCO website for pre- and postprocessing purposes (https://www.croco-ocean.org/download/croco-project/). Only the physical tendencies and the tendency of the phytoplankton biomass are available integrated over the MLD as standard output diagnostic from CROCO-BioEBUS. To be consistent, we integrated all tendency terms off-line over the MLD.

Indeed, the offline integration of the fluxes over the MLD may introduce a bias due to fluctuations within a month. To estimate the difference of the on- and offline calculation, we compared the model output of the monthly mean of the online integration of the tendency of the phytoplankton biomass with the offline integration (fig. R2.1). The on- and offline calculations match fairly well, with a slight underestimation of the tendency of phytoplankton biomass if the term is calculated offline.

Figure 1: *"Spatial distribution of the seasonality of surface chlorophyll" what is seasonality exactly? Amplitude of the annual cycle? Seasonality should be defined somewhere.*

R: Seasonality here represents the amplitude of the annual cycle. The green shading in fig. 1a shows the spatial distribution of the magnitude of the amplitude of the annual cycle of the surface chlorophyll. We will clarify this in the manuscript and figure caption.

L126, Figure 2: *The region for averaging the data for the other EBUS is not defined (?). It should be indicated in the text of the caption for clarity. Please also indicate the results for the Chile EBUS.*

R: We pick the same regions as in Chavez and Messie (2009). We will add fig. R2.2 in the appendix illustrates the regions we use to average over.

Our study is conceptual and meant to focus on processes rather than comparing regional details. Testa et al. (2018) find that the paradoxical correlation between upwelling and surface chlorophyll is not observed in the Chile EBUS, with low surface chlorophyll in austral winter when MLD is deep and upwelling is weak. The seasonality observed off Chile might resemble the Californian system but it would require a more in-depth analysis to provide conclusions. Therefore we here follow Chavez and Messie (2009) and do not include the Chile EBUS.

Caption of Figure 2: *"upwelling (estimated based on winds from QuikSCAT, in Sv" Do you mean from Ekman transport or Ekman pumping, or from both?. Please provide details on how upwelling intensity is calculated.*

R: The upwelling values (estimated based on winds from QuikSCAT, in Sv) are digitized from Chavez and Messie (2009). The 'upwelling' is calculated as a combination of Ekman transport and Ekman pumping Chavez and Messie (2009) section 3.2.

L154-156: *"In other words, more nutrients only have a strong local positive effect if concentrations are low / would be low otherwise." This sentence is not clear; please rephrase*

R: We will rephrase the sentence in the manuscript to (L156-157):

"Nutrient enrichment would only stimulate higher productivity if the region was nutrientlimited".

L163-165: *"In the model, surface chlorophyll and nitrogen concentrations together with upwelling intensity and MLD all display a 40-60% seasonal variability" what is it meant by "40-60% seasonal variability"? Please clarify and rephrase.*

R: Here, "40 - 60% seasonal variability" refers to the amplitude of the annual cycle relative to the annual mean. We will rephrase the sentence to:

"Over the course of the year, surface chlorophyll, surface nitrogen concentrations, upwelling intensity and MLD vary by 40 - 60% relative to their annual mean values."

L194-195: *"We separate biological processes (e.g. primary production, grazing from zooplankton, natural mortality, exudation, sinking) and physical processes (mixing, advection and entrainment) that affect the integrated biomass (Fig. 4b)." The detailed equation should be provided along with details on the method for integrating vertically within a seasonally varying mixed layer. How do you calculate entrainment for instance?*

R: Thanks for pointing out that we missed to explain how we calculated the entrainment. We will add the full equation of the MLD budget of phytoplankton biomass and an explanation of how we calculate entrainment to the revised manuscript. We calculated the entrainment as the residual following the calculation of entrainment for physical variables in the CROCO code, as the difference between the MLD-integrated biomass tendency and the sum of the biological and physical fluxes integrated over the MLD of the previous month. All biological and physical fluxes (except for entrainment) were saved monthly from the model in the unit of mmol N m^{-3} s^{-1}, and we integrated the terms off-line over the MLD using the croco-tools (see our response to the comment above). Further, we calculated the tendency of the MLD-integrated phytoplankton biomass as the difference of the MLD-integrated phytoplankton biomass between months, and interpolated the MLD-integrated biomass tendency to the same time points as the other biological and physical terms before calculating the entrainment.

L197-198: *"Most biological and physical processes decrease from the start (t1) to the end (t2) of the decline phase (Fig. 4cd)." Biological processes should balance physical processes so when the former increase the later should decrease? We understand from figure 4b that physical processes were multiplied by -1? Could you please clarify and provide details in the text of the caption.*

R: We apologise for the confusion. Fig. 4 is a complex figure, and we find that we have not provided sufficient detail to make it easy to understand. We will more details to equ. 1 in the manuscript to define each of the fluxes shown in the figure (equ. R2.1). In addition, we will split up fig. 4 into two figures (see fig. R2.3 and fig. R2.4), and will add a more detailed explanation to the manuscript.

The net biological flux (the sum of all biological fluxes) is positive ("biological gain", fig. R2.3b), thus supporting a biomass increase. In contrast. the net physical flux (the sum of all physical fluxes) is negative ("physical loss"), therefore supporting a biomass decrease. The time point t_1 marks the seasonal maximum of the MLD-integrated phytoplankton biomass and t_2 the minimum at the end of the decline phase. At t_1 and t_2 the net biological and physical fluxes balance (fig. R2.3b,c) and the tendency of the mixed layer phytoplankton biomass is zero (fig. R2.3a). In between t_1 and t_2 (fig. R2.3b), the net biomass supply by biological fluxes decreases more quickly than the net biomass removal due to physical fluxes, resulting in an imbalance of the fluxes and the decrease of biomass between t_1 and t_2.

To check what terms of equ. R2.4 mostly drive the decrease of the biomass between t1 and t2 (fig. R2.4a), we integrated the change of each term over time (that is, the derivatives) between t_1 and t_2 (shown in fig. R2.4b,c). Therefore, in fig. R2.4b,c, if a bar is positive (red), it means that the change of the term during the decline phase (t_1 - t_2) promotes an increase of the phytoplankton biomass (mostly reduced grazing pressure and reduced downward mixing). If the bar is negative (grey), it means that the change of the term during the decline phase (t_1 - t_2) is opposing an increase of phytoplankton biomass. The opposing terms that act to reduce phytoplankton biomass are the ones that contribute to the seasonal paradox (decline of biomass despite the increased supply of nutrients due to upwelling). These terms are mostly reduced primary production and reduced convergence due to advection.

L272-273: *"As we just argued in the previous paragraphs using the differences of the seasonalities of MLD and upwelling in the Peruvian." Connect this sentence to the next one?*

R: We rephrased the sentence to (L272-273):

"As we argued in the previous paragraphs based on the differences of the seasonalities of MLD and upwelling in the Peruvian system, the upwelling of nutrient-rich waters happens when growth conditions are the worst, in particular, light availability is lowest due to deep mixed layers."

The discussion on the impact of global warming is a bit frustrating since it is only based on the implication of a reduced mixed-layer depth in the future. It could be extended to the expected changes in the tendency terms discussed in the paper.

R: We agree that extending the expected changes to other tendency terms we discussed in the paper would be nice. In the paper, we refer to these terms as MLD-driven (dilution, light limitation) or upwelling-driven (temperature-limitation, advection). Projections of the change of MLD in response to global warming suggest that MLD will shallow with increasing stratification (fig. 7 in Echevin et al., 2020). It appears not as clear how upwelling will change in a changing climate (Rykaczewski et al., 2015; Oyarzun and Brierley, 2019). In the Peruvian system, upwelling appears to more likely decrease than increase (Echevin et al., 2020). Thus, we will add to the discussion the effect we expect from a weakening of upwelling on the upwelling-driven terms. Also, as we do not have any model results with respect to climate change, we will condense this section on potential implications for climate change to a paragraph, and include it in the final section that we will rename to "Conclusions and potential implications".

Figure C4: *"The correlation coefficient (R^2=0.81) is shown for the decline phase" the correlation uses only 5 points so it is certainly associated to a low level of confidence?*

R: We specifically select the five points for the decline phase here to address the role of advection during the phase that constitutes the seasonal paradox. As evident from fig. C4, the intensity of upwelling appears not to be the key driver of phytoplankton advection during the whole year (the magnitude of the concentration of phytoplankton biomass in the MLD plays a role as well). However, there is a rather strong correlation from April to August during the decline phase that we focus on. We would like to highlight that upwelling and advection are correlated in the decline phase, which is why we used only these five months for the correlation. We will mark in the manuscript and clearly note in the figure captions what time period we use to calculate the correlations over to avoid confusion.

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Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-113/bg-2021-113-AC2-supplement.pdf