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Comment on bg-2021-113

Anonymous Referee #1

Referee 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-RC1, 2021

Review of "Mixed layer depth dominates over upwelling in regulating the seasonality of ecosystem functioning in the Peruvian Upwelling System" by Xue et al.

This paper investigates the seasonal cycle of nearshore phytoplankton in the Peru Upwelling System (PUS), one of the richest upwelling systems in terms of fish catch. The PUS is particular as the surface chlorophyll seasonal cycle is out of phase with the wind-driven upwelling intensity, suggesting that vertical mixing may generate dilution and light-limitation. The paper is structured in two parts: first, the surface chlorophyll, nutrients, mixed layer and upwelling rates of the PUS are compared to those of three other EBUS (California, Canary, Benguela) using observations. Second, a regional coupled physical-biogeochemical model (CROCO-BioEBUS) is set up for the PUS and used to study the limiting factors of phytoplankton growth and to perform a phytoplankton budget in the mixed layer.

Besides the nicely presented figures and the english that needs to be thoroughly corrected, I was disappointed by the results presented in the paper. First, the authors do not really do justice to a previous paper (Echevin et al., 2008, hereafter EC08) that investigated exactly the same questions using a quite similar modelling approach. The latter is barely cited in the introduction and discussion, even though these authors conducted a comprehensive investigation of the factors driving the seasonal cycle of chlorophyll. As a co-author of this latter work, I was curious to find out whether some new questions or new approaches regarding this paradox were being investigated. Unfortunately, the material presented in this work provides very little new information with respect to the findings of EC08. The authors could have used their model to perform innovative sensitivity experiments (for example EC08 performed several sensitivity experiments to illustrate the impact of iron limitation, temperature, insolation on the seasonal cycle of chlorophyll) but here only one model experiment is analyzed. They claim that they elaborate on the propagation of the seasonal cycle of surface chlorophyll onto higher trophic levels, but very few results are presented in the manuscript. The second part of the paper, which compares different EBUS systems, is not particularly innovative in comparison to previous findings of Messié and Chavez (2005,2015). It seems that the authors were inspired by these previous works but did not manage to expand on the scientific questions. For this reason, I think that the paper in its present state is not worthy of publication in Biogeosciences Discussions.

Detailed comments:

Abstract:

L7: " Intense *upwelling coincides with deep mixed layers*”: is this really unique? Are the layers deep?

L11: *In contrast to previous studies, reduced phytoplankton growth due to enhanced upwelling of cold waters and lateral advection are second-order drivers of low surface chlorophyll concentrations*”: not sure what previous studies were asserting.

L15: what about the role of nutrient enrichment? It could be reduced under climate change. See Chevin et al. (2020).

Introduction

L33-35: The seasonal paradox is also mentioned by EC08. At citing the latter, it would be fair to mention that they used a regional coupled physical-bgc model as in the present study. You should also mention Thomas et al. (2001) and Chavez et al (2005) who noted the seasonal paradox.

L38: Actually EC08 showed that the seasonal cycle of insolation did not play a major role in their model set-up, whereas Guillen and Calienes (1981) assumed it played a role.

L39: I disagree here: this has been at least partly assessed in EC08. Rephrase and explain precisely which hypotheses have not been assessed in the latter and which new ones will be assessed in the present work. Overall, a more accurate review of the findings of EC08 to explain the out-of -phase seasonal cycle of chlorophyll is needed in the introduction.

L41: “*the unique mechanism*”: not clear what this unique mechanism is.

L43: use present tense.

Data and methods

L50: why do you cite ROMS here? Explain that CROCO is the next generation ROMS-AGRIF model.

L54: “which is used in this study”: suppress this part of the sentence. Both grids are used in the study but only the results from the fine resolution one are analyzed.

L54: do you use bulk forcing?

L61: I think the authors should explain why these variables are important for the simulation of chlorophyll. Figure 1 of José et al shows that the PCC is too strong in the model at 12°S, not too weak!

L66: I do not think it can be said that N is a species. I think the authors could have done a better job at proofreading the english in the submitted paper.

L72: Table A1 is useful. Please indicate values in the table instead of “see ref.”

L73: Good to know that the model has been adjusted to fit zooplankton biomass, which to my knowledge, has not been done before: which figure and section is it?

Figure C1: I think showing N at ~100 m is more relevant than at 1000m, as it would be in the depth range of upwelled waters. Besides, N at 1000m is not at quasi equilibrium after 25 years, it is still drifting quite a lot.

L78-79: “deep spin-up”: rephrase.

L79: “where shows..”: rephrase.

L81: I am not sure the title of the subsection is adequate. I suggest “Model diagnostics”.

L82: “analyze with...”: another typo. There are too many of them, the language really needs to be corrected, I am sure the authors can do a better job at it or get some help from a native speaker.

L90: add an equation for J and the limitation terms L(x).

L95: I do not understand equation (3). What is L_{mld} and how is it related to J? Does it play a role in the model equations? Please clarify.

L96: C_{mld} is the concentration or the concentration change? If it is the concetration change, what is ΔC_{mld}? I am lost here.

L98: What is the chain rule?

Figure B3: This is an interesting figure: could you indicate the number of data per 1°x1° grid point? How does it compare to the Boyer Montegut climatology (can be downloaded here: http://www.ifremer.fr/cerweb/deboyer/mld/home.php). It would be nice to add an error bar indicating the model’s MLD internal variability.

Figure 1c: Indicate the meaning of labels B-H in the legend. I believe you should suppress label A, which is misleading, as the model is not a reference. I guess all points would be superposed with A if the model was in perfect agreement with the data. Why do you use WOA and not CARS here?

Figure 1d: I am surprised the model chlorophyll is low: is this really the best fit after parameter tuning?

Could you add an “error” bar to represent the model internal variability of Chl? Also, I think that Pennington’s plot was not exactly the same coastal area as shown in Fig.1a.

L137: “suggested” is a bit weak here. See my previous comment about the introduction.

L138: “the weaker increase...”: you need a reference here.

L141: Are the ARGO MLD estimates reliable off Peru? This should be addressed somewhere and ARGO data should be described in the Data and Methods section.

L142: I think there are other hypothesis supporting the high chlorophyll values in the Canary system. This section comparing the different systems is too long and does not focus enough on the main topic of the paper.

L145-146: I do not agree with the conclusion here: as SST is also strongly correlated with insolation, you can not conclude that the seasonal cycle of temperature drives the phytoplankton growth. For your information, EC08 showed that the temperature effect was negligible in their model set-up (see end of section 3.3).

Figure 2: the regions where data is averaged in a coastal band in the four systems should

be indicated in the supplementary.

L150: The reason for this is well known: the along-shore wind forcing is enhanced during winter, increasing upwelling and vertical mixing, and the lower winter insolation decreases surface stratification and increases the MLD.

L181: “DV contributes...”: in which figure is this shown?

L206: “advection is picking up”: I can not see that, advection seems negligible with respect to mixing (Fig.4d).

L208-209: “the decreasing rate...”: I do not understand this sentence

L202-2011: This paragraph is very difficult to follow and lacks precise references to the figures in the core of the text.

L222: How do you obtain this 60% decrease based on Eq.3?

L229: The weak role of temperature is in agreement with EC08.

Discussion

L258-261: I suggest a closer examination of EC08 findings and expand the comparison with their modelling work, which is very similar to what is presented here. In particular, they relaxed iron limitation in their model and found an Chl increase of 20-80% (depending on the latitude) during winter and spring, which corroborates the impact of iron limitation on the seasonal cycle found by Messié and Chavez (2015).

L272: the sentence seems incomplete.

L274: “and in deep ...”: rephrase

L275: “charge”

L276: I am not convinced by this hypothesis: the residence time of the upwelled water in the mixed layer near the coast is probably quite short as upwelled waters are rapidly transported offshore by Ekman currents. Thus I do not believe in such preconditioning. Unless you can you prove it using the model.

L295: “higher” with respect to what? Clarify.

L314: is this a result of the study? It has not been described. I think elaborating on the seasonal cycle of export and zooplankton could have been interesting.

L320-332: This discussion is very speculative and vague. I do not find it very useful.

L340: Echevin et al. (2020) also investigated the mixed layer evolution under climate change (Figure 7), not only changes in upwelling. I encourage the authors to read the papers they cite more carefully.

L347: Surface chl only slightly increases in the different simulations (2%-17%, Fig.12a).

L355: The propagation of the seasonal variability up the foodweb is not documented in the results sections and only mentionned in the discussion: it is not worth mentioning in the conclusion.

L356: what are the remaining open questions about the interactions behing the mixed layer and upwelling dynamics? Be more specific.

References:

Chavez, F.P., 1995. A comparison of ship and satellite chlorophyll from California and Peru. Journal of Geophysical Research 100 (C12), 24855–24862.

Thomas, A.C., Carr, M.E., Strub, P.T., 2001. Chlorophyll variability in eastern boundary currents. Geophysical Research Letters 28 (18), 3421–3424.