

Interactive comment on “Factors controlling plankton productivity, particulate matter stoichiometry, and export fluxin the coastal upwelling system off Peru” by Lennart Thomas Bach et al.

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We thank both reviewers for their insightful comments, which helped to improve the manuscript. Please find our point by point responses in the following. Please note that line numbers in our responses refer to the revised version of the manuscript.

Reviewer #1

1) General Comments: The overall quality of this paper is good because it provides new insight for production processes of a very important region of the oceans. Nev-

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ertheless, there is a basic concept that needs to be carefully considered, mainly that Production is phytoplankton biomass mg C m^{-3} and Productivity is carbon production in $\text{mg C m}^{-3} \text{ d}^{-1}$. Please revise because biomass was measured and change it accordingly, or explain. This is a crucial point.

REPLY: We thank the reviewer for the kind words. We changed “productivity” to “production” throughout the manuscript.

2) I think the Discussion is too long. It takes 21 pages out of 54. I cannot suggest how it can be shortened. Maybe move some ideas to the description of the results? Or make new comparative Figures for stoichiometric results?

REPLY: We thank the reviewer for this suggestion, which is consistent with the feedback we got from reviewer #2. We therefore removed section 4.2 (“plankton succession”) from the discussion. This aspect will be covered in more specialized manuscripts within this special issue. Furthermore, zooplankton results, methodology, and the zooplankton figure were removed as this aspect was not covered in the discussion any longer. We also deleted more speculative parts of the discussion in sections 4.3 and 4.4. The discussion is considerably shorter in the revised version of the manuscript and more focussed on the biogeochemistry.

3) Finally, authors refer in the Discussion in Line 677 that. . . there was little potential to detect treatment differences, especially in light of the large differences in the starting condition that induced considerable variance between replicates. Therefore, they decided to focus on the analyses of temporal developments of ecological and biogeochemical processes rather than on detecting treatment differences. Maybe just to make it clear, this point about variance should not be left aside and they should add a paragraph about Plankton patchiness, insufficient replicates??

REPLY: We revised the mentioned text to indicate that a higher number of replicates may have helped to detect treatment differences due to increased statistical power. However, we also noted that this is unfeasible due to the enormous costs of in situ

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mesocosm experimentation.

4) Abstract 1. The phytoplankton communities were initially dominated by diatoms but shifted towards a pronounced dominance of the mixotrophic harmful dinoflagellate (*Akashiwo sanguinea*) when inorganic nitrogen was exhausted in surface layers. It is not clear if the phrase refers to the mesocosmos enclosed waters or the natural Pacific surface layers.

REPLY: We added the word “mesocosm” to clarify (Line 52).

5) Abstract 2. It is not clear why the increase and dominance of one dinoflagellate species is not considered a bloom?

REPLY: We added the word “bloom” to indicate that *A. sanguinea* formed a bloom (Line 56).

6) Abstract 3. Authors state that numerous biotic and abiotic factors modify productivity and biogeochemical processes. It is not clear why they simplify at the end only to nutrients and light?

REPLY: The reasons why we focussed on light and nutrients are described and justified in the discussion line 687-699. This description would be too long for the abstract but we think there are no open questions in the abstract at this point of the abstract, since we emphasize nutrients and light but do (at this point) not exclude any others.

7) Abstract 4. Mesocosm study revealed key links between ecological and biogeochemical processes. . . Please expand and be specific here.

REPLY: This sentence is to close the abstract and refers to the observations described in sentences before. We modified this sentence slightly to link it better to the previous text.

8) Introduction 1. Eastern boundary upwelling systems (EBUS) are hotspots of marine life. References from other EBUS are missing. Please add.

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REPLY: The cited reference is a review of all 4 major EBUS (North-west Africa, Benguela, Humboldt, California). So we think that it is an appropriate reference for our statement. Nevertheless, we also refer to the review by (Thiel et al., 2007) in the revised version to add a reference specific to the Humboldt system.

9) Introduction 2. Moreover most self-references for the coastal upwelling system off Peru are reiterated in lines 69 to 86. Lines 100-101 ...the observed patterns of productivity and export in the Peruvian upwelling system (and elsewhere). . .change elsewhere at least to Peru's Chile Current or give references.

REPLY: The reviewer asks for more "diversity" with respect to references that support our more general statements. To account for this request, we added a reference by (Bakun and Weeks, 2008; Daneri et al., 2000; González et al., 2009; Thiel et al., 2007). As our study region is in Peru, we think it is well justified to refer to the widely used term "Peruvian upwelling" in our study. We think this is more appropriate for our case than e.g. Humboldt system, since using the term "Peruvian upwelling" reminds the reader where our study took place.

10) Introduction 3. Lines 103-4 . . .climate change (Add here the reference of Gruber, 2011 through warming up, turning sour, losing breath) and alterations in productivity could disrupt one of the largest fisheries in the world (maybe a different reference here).

REPLY: We moved the Gruber reference to the previous part as suggested by the reviewer. The second statement is now supported by referring to Bakun and Weeks, 2008 (Lines 101-104).

11) Methods 1. Figure 2 is very unclear. Please improve

REPLY: We simplified the figure and added frames to better distinguish the individual subplots.

12) Surrounding Pacific as in line 183 should say surrounding Pacific water. Line 238 says Pacific surface waters. Please keep this nomenclature. Therefore, later in the

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results and Discussion the term “Pacific” would be better understood. Pacific is an Ocean so this is confusing specially referring to Redfield ratios.

REPLY: We changed “Pacific” to “Pacific water” throughout the text as suggested by the reviewer.

13) Results 1. The legend of Figures needs to be homogenized. It’s difficult to understand them when different denominations are used: surface and bottom waters versus uppermost and the lower water column

REPLY: We changed the term “uppermost” to “surface” and specified the depth range when necessary.

14) Fig 3 The black or white lines on top of the contours show the depth integrated water column average- I don’t see the white lines.

REPLY: We thank the reviewer for pointing this out. There are either black or white lines for the water column average. The “color” differs due to visibility reasons. We indicated that black lines are used in subplots A, B, D and white lines in subplot C.

15) Figure 4. Inorganic and organic nutrient concentrations . . .Add to legend mesocosmos in colour lines. Line in black Pacific is the control water?

REPLY: The legend is shown in subplot G. The legend also indicates that the black line represents data from the surrounding Pacific water.

16) Very interesting to compare NO_3^- + NO_2^- and DON opposite behavior in control and mesocosmos REPLY: The differences are due to the differences in the sampling strategy. While the mesocosms are a lagrangian system (same water mass sampled every other day), the Pacific water changes from sampling day to sampling day due to advective processes. Thus, temporal trends in the two systems cannot really be compared.

17) Figure 5. Chlorophyll a concentration. . . please describe what is the black line

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REPLY: We thank the reviewer for pointing towards this. The black lines were removed from the plots as they were in there by mistake.

18) Fig 10 Brown blob is not a very nice representation of the dinoflagellate Akashiwo. At least add the flagella

REPLY: We added flagella and changed the shape of Akashiwo to more closely resemble its natural appearance.

19) Discussion First, 4.3.1. Productivity is a rate. . . Please change to biomass production. See my General Comments. Line 1051 Altogether, our study revealed some important factors controlling plankton productivity. Authors measured Chl-a. This needs to be clarified in the whole manuscript.

REPLY: We changed productivity to production throughout the text as suggested by the reviewer.

20) Second, I don't understand why Orni-eutrophication was not included in the Abstract, I think it is a very interesting result: Orni-eutrophication during the last 10 days enabled rapid phytoplankton growth through the relief from N-limitation and high light intensities in the uppermost meters. Bird defecation triggered intense phytoplankton blooms in most mesocosms in the uppermost part of the water column where light was plentiful. N inputs through these excrements were directly utilized and converted into organic biomass whereas the defecated P remained unutilized and sank through the water column directly into the sediment traps. Line 702 what seabirds typically add to the water column of the Pacific in this region (Otero et al.).

REPLY: We did not pick up on orni-eutrophication in the abstract as this effect is amplified profoundly through the mesocosms (because the birds had a structure to sit on). Thus, it is an artificial effect that helped us to understand some processes but the orni-eutrophication itself is not a key outcome of our study.

21) Third, Export flux: I think authors should again compare their results with other

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sites in the HCS such as i.e. Gonzalez et al 2009, Carbon fluxes within the epipelagic zone of the Humboldt Current System off Chile: The significance of euphausiids and diatoms as key functional groups for the biological pump. Line 902.

REPLY: We thank the reviewer for this suggestion. The comparison of vertical fluxes in the mesocosms with those at the surrounding Pacific will be addressed in a specific paper in this special issue by Ursula Mendoza et al. They had sediment traps installed next to the mesocosms in the Pacific and will investigate in- and outside fluxes and compare them with measurements from other regions. However, we refer to Gonzalez et al. 2009 in the introduction as it is a relevant paper for our study.

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