Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-35-AC2, 2020

Biogeosciences Discussions
Open Access

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.

Lennart Thomas Bach et al.

lennart.bach@utas.edu.au

Received and published: 30 June 2020

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.

22) In their paper, Bach et al. describe the results from a mesocosm experiment offshore of Peru. The aim of the experiment was to compare upwelling effects on plankton communities in two different water bodies, but the authors did not achieve enough dif-

ferences in biogeochemical properties of the water bodies to make any assumptions on the treatment effects as they stated in the abstract. Instead they decided to provide a descriptive manuscript and discuss the dynamic of the phytoplankton bloom over the course of the experiment. Although I do appreciate the detail and the fairness of the description (especially in chapter 4.1), I also think that the paper is lacking focus and would recommend some shortenings. I would recommend that the authors focus on the productivity and export processes (chapter 4.3) and leave out phytoplankton-zooplankton interactions, which are supposed to be described in detail in another paper in the special issue. Chapter 4.3 is definitely the most interesting from the scientific point of view and also well written in comparison to the other parts of the manuscript. Now the paper is extremely long and contains several stories, which do not come together.

REPLY: We thank the reviewer for the valuable comments. We removed section 4.2 (Plankton succession section) and section 4.5 as suggested by reviewer #2. We kept section 4.4 (C:N:P:Si stoichiometry in the mesocosms) since stoichiometry is an interesting parameter to discuss and is not covered in another manuscript of the special issue. The discussion is now more focussed and shorter.

23) Language 1. The authors should carefully read the paper and get rid of the jargon and odd phrases. Some examples (there are many more in text): Second, the high primary production fuels secondary production.

REPLY: Changed to "…it sustains one of the largest fisheries in the world, making the Peruvian upwelling system an area of outstanding economic value." (Lines 77-79)

24) Language 2. Our paper kicks off a Biogeosciences special issue.

REPLY: Changed to: "Our paper is the first in a Biogeosciences special issue about the 2017 Peru mesocosm campaign." (Lines 116-117).

25) Language 3. using a manual kitesurf pump so that the sediment material was

sucked through the hose - first, it's a kite pump, not kitesurf pump, second, a kite pump is actually nothing else than a manual air pump (although I appreciate the authors' hobby).

REPLY: It is a fantastic hobby indeed :-D We changed this to "air pump". (Line 242)

26) Language 4. The water columns enclosed at the beginning of the study were temperature stratified - should be: thermally stratified

REPLY: we changed temperature to "thermally" (Line 379).

27) Language 5. Dinophyceae became about as dominant as in the other mesocosms when Cryptophyceae disappeared

REPLY: We changed this part to: "The A. sanguinea bloom was delayed by ∼10 days in M3 and they remained absent in M4 throughout the study. Cryptophypheae benefited from the absence of A. sanguinea and were the dominant group in M3 and M4 in the ∼10 days after the OMZ water addition" (Line 351-354).

28) Language 6. Nevertheless, we observed a few temporal trends that were sufficiently clear - temporal trend is something that should be statistically proven and there are methods to detect temporal trends in time series

REPLY: This sentence was deleted.

29) Language 7. The quasi absence of silicoflagellates

REPLY: This sentence was deleted.

30) Language 8. key mechanism muting phytoplankton growth

REPLY: Agreed, this part was weird. We changed it to: "It appears that self-shading due to high biomass is a key mechanism that constrains phytoplankton growth when integrated over the water column. This constraint may enable an equilibrium between production and loss processes as reflected in the relative constancy of chl-a, POCWC and POCST (Figs. 5A and 8A, E; see next section for further details on export). Indeed, the orni-eutrophication demonstrates that when limiting nutrients are added to a layer with high light intensity, phytoplankton can break this equilibrium and grow rapidly (Fig. 5A)." (Line 742-748)

31) Thus, the shift in PON:TPP in the mesocosms was triggered by ecology whereas it was arguably triggered by a physiological response in the Pacific.

REPLY: This section was deleted to shorten the manuscript.

32) Regime shifts. I have an impression that the authors don't entirely understand the concepts of alternative stable states and regime shifts. There are methods to detect the occurrence of a regime shift from data, but no such method has been applied. For example, "Overall, biogeochemical pools and fluxes were surprisingly constant in between the ecological regime shifts." - the authors probably mean "between alternative stable states"? I don't see any second regime shift to compare with. If the biogeochemistry was not different between the alternative stable states, how the ecological regime shift should have occurred? There are two ways of dealing with this problem: (i) carefully rewrite parts of the text that refer to the regime shifts, or (ii) perform a proper statistical analysis (plethora of methods exist from rather simple "signal to noise ratio" to more sophisticated Monte Carlo models). Personally, I don't think that we are witnessing a regime shift here, but rather a phytoplankton secession that is a result of competition.

REPLY: Agreed. We do not refer to regime shifts in the revised version. Instead, we refer to changes in community composition.

33) Statistics. The manuscript is very descriptive in it's nature, but it does contain some statements that can and should be proven by a proper statistical test, especially considering that the title refers to "factors controlling plankton productivity" etc. What are these factors? Do they significantly affect plankton productivity? For example, the authors write on the page 18: "Nevertheless, we observed a few temporal trends that

were sufficiently clear (and consistent with other datasets) so that we are confident that they were “real” and outside the noise of the measurement.” - temporal trends can be determined from these data, so it should be tested if they are “sufficiently clear” (or as one might have said “significant”). Another example: "Interestingly, there was a tendency of decreasing POC:TPP during periods of chl-a increase” - it is possible to test statistically if time series are correlated (e.g. using cross-correlograms)

REPLY: The sentences the reviewer is referring to (and similarly vague sentences) were removed from the revised version of the manuscript. In general, it is important to emphasize that we determined many corresponding parameters in our time-series. This allows us to confirm a trend in a certain parameter by checking for consistency with related parameters. For example, in section 4.2.1 (formerly 4.3.1) we describe a strong POC increase and argue that this increase is “real” (i.e. significant), as it coincides with an increase of PON, POP, Chla, dinoflagellate abundance and a decrease in DIC, PO43-, DON. We argue that these “mechanistic” insights are more powerful in confirming/rejecting a trend than statistical approaches, which are often based on certain choices (see below). We carefully explored the possibility to use statistical tools for the detection of temporal trends (and in fact use some of them like moving averages for data exploration). One option is to use regression analyses. However, the outcome (i.e. significant or not significant) depends on choice of the applied regression model (linear or non-linear) and on the segment of the time-series that is explored (e.g. in the example above the POC increase due to the dinoflagellate bloom takes place over ∼10 days (Fig. S1) but it is difficult to clearly determine the exact time frame due to limited data on dinoflagellate abundances). These choices include a certain level of arbitrariness, which can influence the outcome. Thus, we generally put more trust into a “mechanistic” explanation of an observed trend. As suggested by the reviewer, we also explored the possibility to apply cross-correlograms to detect trends. However, this tool is used to detect periodicities in the time-series which may be hidden by the noise of the measurements. Periodicities can be caused e.g. by tidal, diel, lunar, seasonal cycles etc. None of these external forcings applied to our time-series as tidal shifts are excluded in mesocosms, the temporal resolution was too low to detect diel cycles, and the experiment too short for lunar/seasonal cycles. Nevertheless, we checked our data for periodic auto-correlations with the acf function in R, but could not find significant autocorrelations in the dataset for time-lags longer than 1 or 2 sampling days (sometimes also over longer timescales when there was very little change in a measured parameter from sampling day to sampling day (e.g. PO43-)).

34) Other comments: "The nutrient concentration between the collected water bodies were relatively small" - the authors decided to exclude treatment effects from the analysis based on this assumption, but in the results section they report that OMZ source water at the station 3 contained 4 umol/L NOx and from station 1 only 0.3 umol/L NOx. Is this a small difference? In fact, the authors describe that low concentrations of NOx in the mesocosms with the OMZ from the station 1 lead to the decrease of the NOx concentration following the water addition. As the OMZ water contained 2.5 umol/L PO4 at both stations, the treatments must have differed in N:P ratios after OMZ water addition. If this is the case, I would argue that the authors need more solid arguments to ignore the treatment effects than simply their personal judgement. One possibility is to statistically prove that the treatments did not affect water chemistry.

REPLY: The source water had the above-mentioned NOx concentrations but it was diluted afterwards when mixed with the mesocosm water (see Section 2.3). We added a table to the revised version that summarizes treatment-specific information (such as NOx addition, Table 1). NOx-, NH4+, and N:P were significantly different between the treatments after the OMZ water addition (this information was added to section 3.2 and Table 1). However, differences were small (e.g. N/P =1.5 vs. 2.9) and the variance among the mesocosms was large due to variable initial conditions (as discussed in section 4.1). We may still be able to reveal some significant differences between the two treatments in some parameters (e.g. chl-a) when applying more sophisticated statistics but our impression was that we can learn more from this dataset when focussing on temporal developments. Our decision not to discuss treatment differences has no influence on the conclusions drawn in this manuscript. This is because we are focussing on temporal changes within individual mesocosms and also interpret the results individually when trends in mesocosms differ from each other. It is also important to note that some manuscripts to be published within this special issue will have a look at treatment differences (we emphasized this in the revised version of the manuscript) (Line 650-652).

35) Line 527: diatoms - change into Bacillariophyceae for consistency.

REPLY: We changed diatoms to Bacillariophyceae. (Line 521)

36) The authors discuss the results from flow cytometry and microscopy. These methods should be described in the method section. Also grazing experiment of Paracalanus is not described in the method section, but discussed later in text.

REPLY: The zooplankton grazing experiments are not discussed anymore in the manuscript (all zooplankton data was removed from the manuscript). We only refer once to the imaging flow cytometry and microscopy datasets in the discussion to provide the information that the Dinophyceae dominance (estimated with CHEMTAX) is due to the bloom of Akashiwo sanguineum. The flow cytometry/ microscopy datasets will be described/discussed in detail by Avy Bernales et al. in a specialized phytoplankton paper so it may be unnecessary to add a full section on flow cytometry and microscopy only to provide this information.

37) Line 717: these principles come back to the papers by Margalef and Reynolds, which would be proper citations

REPLY: Section 4.2 was deleted.

38) Line 724: migratory - change into “motile” Paragraph from the line 792 onwards is full of jargon and describes the effects of enclosure rather than sampling.

REPLY: Section 4.2 was deleted.

39) Line 808: physical processes - change into “transport processes” Paragraph from 873 onwards can be omitted

REPLY: We changed “physical processes” to “transport processes” (Lines 687-690) and deleted the mentioned paragraph as suggested by the reviewer.

40) Line 898: it’s not a surprise, dinoflagellates like Akashiwo and Alexandrium often have a long lag phase.

REPLY: We agree that it is probably not surprising for plankton ecologists but there is debate around this topic in the “biological pump” community (see the referenced articles by (Laws and Maiti, 2019; Stange et al., 2017)). Therefore, the information is relevant in the context of export fluxes.

41) Line 969: it is not contradictory to this study. Hillebrand et al. is based on species specific population growth rates (u) and not on communities.

REPLY: This paragraph was deleted.

42) Lines 1000-1002: this is important for interpretation and should come much earlier in the manuscript, when you describe the mesocosms and experimental design.

REPLY: The classification of the experiment into distinct phases 1-3 is based upon combining all information discussed in this manuscript. These phases were not designed beforehand so we think this classification needs to go into the synthesis section 5.

43) Paragraph from the line 1042 can be omitted, it doesn’t bring anything new to this study

REPLY: We deleted this paragraph as suggested by the reviewer.

References

Laws, E. A. and Maiti, K.: The relationship between primary production and export production in the ocean: Effects of time lags and temporal variability, Deep Sea Res. Part I Oceanogr. Res. Pap., doi:10.1016/j.dsr.2019.05.006, 2019.

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