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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.

Anonymous Referee #2

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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 differences 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

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Discussion paper

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. There are also 3 overall problems that I have with this paper, which require more attention:

1. Language. âĂÍThe 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 Our paper kicks off a Biogeosciences special issue 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). The water columns enclosed at the beginning of the study were temperature stratified - should be: thermally stratified Dinophyceae became about as dominant as in the other mesocosms when Cryptophyceae disappeared 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 The quasi absence of silicoflagellates key mechanism muting phytoplankton growth 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.âĂÍ

2. 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 biogeo-

chemistry 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.

3. 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)

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.

Line 527: diatoms - change into Bacillariophyceae for consistency.

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.

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

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.

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

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

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.

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

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

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