

Comments to the manuscript submitted to "Biogeosciences"

entitled

"Extracellular enzyme production in the coastal upwelling system off Peru during different upwelling scenarios: a mesocosm experiment"

By Ch. Spilling et al.

After reading the manuscript, I have the following ideas about the experiments carried out:

Originally, the influence of upwelling water on the biogeochemical processes in surface water was apparently to be investigated. However, due to the El-Nino event, no upwelling water was available. Therefore, water from the OMZ was used to simulate the effect of nutrient-rich water upwelling into the surface layer. The OMZ water was filled into the deeper layers of the mesocosms and was kept there during the duration of the experiment. The 0 – 10 m layer above was sampled. Consequently, from my point of view, the influence of the OMZ on the overlying water layer was investigated. This is a very interesting subject. To what extent effects of upwelling water can be inferred from the effects of OMZ water could be part of the discussion. Upwelling water reaches the water surface directly and mixes with surface water during further transport and does not layer underneath. However, the biogeochemical properties could be compared using data from the literature.

The results part, especially the first chapter "Nutrients" is difficult to read. As emphasized in the title, the focus is on two scenarios. The description of the individual mesocosms does not make the two scenarios clear. You have to look constantly at the figures in order to assign the respective mesocosm to the corresponding scenario. Additionally, constantly different points in time are described in the course of the experiment (sometimes you are at the OMZ water treatment, then at the time before and then again at the end). That's even more confusing. For each scenario, 4 parallel mesocosms were examined. My suggestion would be to describe the general trend of each scenario. Boxplot charts for each sampling time and each treatment would perhaps better illustrate the trends in the two scenarios than the line charts.

In the time course of the parameters in the scenarios, a fixed structure should be maintained - the course before the addition of OMZ water, then the course in scenarios 1 and 2. In the discussion, the influence of different variables on the enzyme activity in the surface water could then be considered the addition of OMZ water and the individual OMZ treatments. In my view, such a structure would make for an interesting manuscript.

Further Comments:

Title:

The word „ production“ should be replaced by the word “activity” (in the abstract too) , because that is what was measured. From my point of view, no upwelling scenarios were examined.

Introduction:

Page 3 line 53: "The ongoing warming..."

Comment: What is meant: Warming due to climate change or warming of upwelling water during transport. Please formulate exactly.

Page 3. Line 45-46: "The fate of biomass..."

Comment: I would move this sentence to the end of the chapter (line 52) and continue with lines 63ff. The chapter describing the OMZ (line 53-62) could be placed before the objectives.

Material and methods:

Page 5, line 93 and throughout the entire ms: Please replace deep-water by OMZ-water, because, in the ocean, water from 90 m is not deep water.

Page 5, line 99: "... And sampled every second day...."

Comment: Please change into "..... and sampled every second day over a period of 50days..."

Page 5 line 103: "... on day 5..."

Comment: the words can be deleted, they are not necessary here.

Page 5, line 103: "... from 90m depth..."

Comment: According to Bach et al. 2020 water was taken from 70 m depth.

Page 7 , line 136-142: "To measure **inorganic nutrients**, total dissolved nitrogen (TDN) and phosphorus (TDP), the samples were first filtered through pre-combusted (5 h, 450°C) Whatman GF/F filters (pore size 0.7 µm). The filtrate was collected in 50 mL acid-cleaned high-density polyethylene (HDPE) bottles and placed directly into a freezer (-20°C). Later the filtrates were thawed at room temperature over a period of 24 hours and divided in two. The first half was used to determine inorganic nutrient concentrations as described above. From the other half we determined the TDN and TDP concentrations. "

Comment: I would move this paragraph to the beginning of the Nutrient chapter

Page 9, line 192: Which extraction agent was used? Please provide a reference for the chlorophyll extraction and measurement

Page 12-13: Measurement of enzyme activity: how many parallels (pseudo-parallels) are measured for each sample?

Page 13, line 261-264: Microtiter plates were obviously used for the enzyme activity measurements, as has been described several times. I don't see how it can be done in 20ml Subsamples. Please correct the error.

Results:

According to Bach et al. In 2020 scallop larvae were added to the treatments. It should be mentioned whether the addition of these organisms affects the enzyme activities.

Discussion:

Page 23-24: line 499-502: " The hydrolysis rates of AP were relatively low compared with most published data, probably linked to the clear surplus of PO₄³⁻. It is worth to note, however, that we were most likely not measuring the maximal potential hydrolysis rates as substrate addition was relatively low (100 nmol L⁻¹) and would likely have been higher with more added substrate.

Comment: For the LAP activity, it was described in the "Methods" that the substrate saturation concentration was determined before. I assume that was done for the AP activity as well. If so, then higher substrate loading should only marginally increase the AP activity.

In summary:

The authors should consider whether upwelling water effects were actually simulated or whether the manuscript should be geared towards OMZ effects. The manuscript should then be focused accordingly. The current manuscript is not well focused and sometimes difficult to read.