

Interactive comment on “Composition and Vertical Flux of Particulate Organic Matter to the Oxygen Minimum Zone of the Central Baltic Sea: Impact of a sporadic North Sea inflow” by Carolina Cisternas-Novoa

Anonymous Referee #1

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The authors demonstrate their investigations of particulate matter in the water column and in sediment traps in two basins (Gotland Basin (GB) and Landsort Deep (LD)) to estimate its composition and the particle flux in these basins and how it changes dependent if the deep water is oxygenated or not. Thus, the manuscript can be a significant contribution to understand the biogeochemical processes and its spatial variation in the Baltic Sea. However, the ms is difficult to read and to get a ‘take home message’ because it contains many nonspecific verbalization. For example, it is often unclear which depth horizons are meant or why the situation at a certain depth is mentioned

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at this place (e.g. 110m in line 31). The influence of the MPI must be presented more clearly and substantiated. This requires comparison of a defined depth range which is oxygen rich in one basin and low in oxygen in the other. In the GB, the situation after MBI should be compared with a situation during a stagnation period, if possible. Many of these things are present in the ms but not clearly and focused demonstrated. I would expect from the title what is transported from the overlaying layers to the OMZ. Indeed, data for the whole water column of the GB and up to 200m in the LD are given without any Accentuations.

Abstract: The objectives are not clear Line 17-18: “Oxygen (O₂) depletion may improve the efficiency of the biological carbon pump“. Is this sentence the hypothesis of the work? If yes, than it has to be indicated and it needs to be answered at the end of the abstract.

Line 20-21: I would replace “Major Baltic Inflow “by“ salt water inflow” here.

Line 24-32: It is difficult to understand what the comparison of the two depths means for the task.

Line 29: Why “contrastingly”? POC and PN decreased too. Line 33: why “may form”? I think it is a result of this work.

Line 38: sink instead of sank

Introduction: Line 78-80: “ . . .(Tamelander et al. 2017)”. Please replace “On the long term, a decrease in OM downward flux may limit the oxygen depletion.” By “The reduction of nutrient inputs as target by HELCOM can cause a decrease in OM downward flux and limit the oxygen depletion.”

Line79-80. I would delete the last sentence of the chapter.

Line 88: “. . . Carstensen et al. 2014)”. Recommendation: Salt water inflows from the North Sea.

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Line 91-92 “Saltier, denser, O₂-rich North Sea waters entered the western Baltic Sea in December 2014 and reached the Gotland Basin on February 2015.” The sentence could be deleted.

Line 92-95: “At the time of sampling, this MBI also affected the neighboring Faroe Deep; but not the LD, located further northwest.” The sentence can be deleted.

Line 100: Please add the water depth in which the redox lines occur.

Line 99-100: This chapter can be shortened and combined with the chapter before.

Line 118-119: “enriched with OM; specifically with transparent exopolymer particles”.

Line 134-142: A clear objective is missing here.

Methods Line 146: “surface-tethered sediment traps” that’s not true. Traps were also installed in 180 m

Line 147-148: depth of water sampling should be given here.

Line 150: conductivity temperature depth? Suggestion: Temperature, salinity and O₂ concentration were determined at each station using a Sea-Bird (CTD) probe equipped with a oxygen (Oxyguard, PreSens) sensor that was calibrated. . .

Line 155: According to Table 1, there are 3 or 4 depths in which the traps were exposed. That should be mentioned here as well.

Line 174-178: Ammonium has to be measured in an unfiltered sample. However if samples for nutrient analysis are stored frozen and analyzed using an auto-analyzer, then filtration is necessary. Please correct.

Line 191-192: Please add the wave lengths

Line 215-219: Suggestion: “Particle number and area was measured semi-automatically using an image analysis system including the WCIF ImageJ software. Image analysis of TEP and CSP were and conducted after Engel (2009). Additionally,

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TEP and CSP in water samples from the stations where we deployed sediment traps were analysed spectrophotometrically according to Passow and Alldredge (1995) and Cisternas & Novoa et al. (2014) respectively. Why was the additional method applied?

Line 223: For TEP and CSP it should be mentioned that the red and the green channel were used?. Here should only be mentioned that the blue channel was used.

Line 226 and line 233: Please delete the word "directly". When storing samples, there is no direct measurement.

Results Generally: I suggest that the results should be demonstrated for the basins successively (at first for the GB and after it for the LD) and not switched between the basins. In the vertical direction it should be started with the surface then successively the deeper layers whereby the depth of each layer should be defined to understand the results reported thereafter. Information about temperature and salinity is missing in the text.

Line 250-251: Information about the thermocline should be moved to the beginning of the chapter. The traps were exposed for one or two days. The water samples were taken at the same time. I don't believe that there was such a large range where the thermocline was located during this short time.

Line 259: Which depth is meant with surface water?

Line 260: Suggestion for changes: (6 μM at 80 and 140 m, and 0.12 260 μM at 110 m). It could be added already here that the upper (80m) and lower (140m) bounds and 110m the core of the OMZ are. It is mentioned later, but it should be already included here.

Line 270: Because the conditions in water column are reported, it should be mentioned that nitrite had a maximum at 370m (Fig.2).

Line 273-274: To which depth the second nutrient values apply; "the upper 110m" is

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confusing here.

Line 275: 0.22 μ M?

Line 269-276: The individual nutrients should be described one after the other and not switch between them several times.

Line 285: Please insert some data.

Line 286: Do you mean the sum of pico- and nanophytoplankton?

Line 287-288: 92% of what? Recommendation: Picocyanobacteria determined by phycoerythrinfluorescence amounted 92% in GB and 96% in LB of the total picophytoplankton and was 30%

Line 289: “The abundance of larger phytoplankton (>5 μ m) was determined by microscopy”. The sentence can be deleted. It is described in the methods.

Line 293: Filamentous unicellular cyanobacteria. A cyanobacteria filament always consists of more than one cell.

Line 292-293: “Cyanobacteria were 60% less abundant in the GB than in the LD.” It is mentioned 2 sentences before. It can be deleted here.

Line 297: 95% of what

Line 302: which layer is meant with the surface.

Line 316: ...decreased quickly at 10m...” Rather: ...decreased quickly below 10m. ...

Line 318: The units of TEP and CSP should be explained in the methods.

Line 324: “...were only observed...” instead of “...were observed...”

Line 331: What is ESD? Please give the full name.

Line 341: “POC flux slightly increased by 18% from the shallowest (40 m) to the deepest (180 m) depth. Fluxes of PN and CSP were higher at 40 and 60 m and decreased

by 19 and 70% from 60 to 180 m...”. I assume the layers 0-40m and 0-180m are meant.

Line 356: “. . .sediment traps at 110 m and 180 m. MnOx- like were They occurred as single particles and forming formed and with other particles. . . ”.

Line 361: “. . . .ranged from 0.6 to 16.5 mm (media mean 1.8) at 110 m (Table 4).

Line 371-381: These chapters would be easier to understand if data are inserted.

Line 390: Please indicate in the method chapter how the DI has been calculated. Line 300-401: “We assess the potential influence of increased O₂ concentration caused by the 2014/2015 MBI in the GB on the chemical composition and degradation stage of the sinking and suspended OM relative to the anoxic LD.” In my view, this is not clear enough in the ms, including the discussion.

Discussion The discussion involves a lot of repetition of the results.

Line 404-405 “...primary production“. Do you mean phytoplankton biomass? PP measurements were not included in this study.

Line 410-411: “Pico-phytoplankton cell abundance (cell mL⁻¹) dominated the small phytoplankton size fraction < 5 μm (Table 2), suggesting a significant contribution to PP and Chl a concentration. This can not be deduced from the abundance alone.

Line 421-422: “Cell abundance of total phytoplankton (>5 μm) were not significantly different (p=0.74) in the GB and the LD.” Which phytoplankton group refers to this statement. According to Table 3 the cell counts in both basins differed. I am wondering that the differences are not significant.

Line 434: “Our samples were collected right after the peak of the spring bloom. . . .”. That is not right. The spring bloom occurs, for example in the Gotland Basin, from the middle until the end of April (see also publications by B. Schneider et al.). The investigations were carried out in June.

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Line435-437: "...TEP concentrations had not reached the usually higher summer value yet since phosphate remained present in the water column (potentially not limiting the PP)". Please make the relationship more clear.

Line 444: It should be noted at what depth the OMZ was located before the salt water inflow. Recommendation for rewriting the sentence: The MBI changed the vertical distribution of O₂ in the GB by increasing its concentrations in depth below...m and relocation of the oxygen deficient layers from ..m to 74-140 m depth.

Line 452-453: "MBIs can have a major impact on nutrient recycling". Such general statements should be reduced throughout the ms.

Line 480: "... Carbon flux below the euphotic zone...". To the bottom or to what depth?

Line 485-486: "...the estimations based on our results from the GB are higher than the C fluxes predicted by those models." Here it should be taken into account that the measurements are obtained only from a single measurement over one or two days. The question is how representative a single measurement is.

The subsequent paragraphs and chapter should be focused. At the moment it is very diffuse and the message is not clear.

Table 3: It is not clear for me how the filamentous cyanobacteria were counted. Were the single cells in the filaments counted or were it counted as units of 50 μ m or 100 μ m length, as it is usually performed.

Fig.2A: The scale of the x-Axis for salinity is wrong.

In Fig. 4: It seems that only one or two depth are sampled. It should be indicated by zero-values if all depth are investigated and no particle is found.

Recommendation: Moderate revision

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