

## ***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***

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We thank the referee for her helpful observations, comments, and suggestions. Detail answers to the comments are provided below and in the attached pdf.

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

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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 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 (O<sub>2</sub>) 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. AR: We will add information to the abstract, about the oxygen conditions at each depth where we deployed a sediment trap in GB and LD (ex: oxygenated surface, the core of the oxygen minimum zone and deep water oxygenated by the inflow) to clarify why we mention specific depths. We explicitly add the hypothesis and rephrase the objectives for clarity. We answered the hypothesis at the end of the abstract when we expose the differences in POM and export between the two studied stations, and we propose a mechanism to explain those differences. Information about oxygen conditions at depths where sediment traps were deployed in each station was added in L22-25. We explicitly add the hypothesis to the abstract (L25-27) Line 20-21: I would replace “Major Baltic Inflow “by“ salt water inflow” here. AR: This was change to “a major oxygen-rich saltwater inflow” (L21) Line 24-32: It is difficult to understand what the comparison of the two depths means for the task. AR: This paragraph was modified for clarity and as mentioned above, the oxygen conditions at each depth are mentioned (ex: oxygenated surface, the core of the oxygen minimum zone and deep water oxygenated by the

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inflow) Line 29: Why “contrastingly”? POC and PN decreased too. AR: We agree this was confusing “contrastingly” was deleted Line 33: why “may form”? I think it is a result of this work. AR: We said “may form” because, as we explain in the next line, our results suggest that MnOx-like particles aggregate with POM, this is what we propose. However, we think that to ensure that MnOx and POM aggregate and the specific composition of those aggregates, we will need to investigate aggregate formation and composition further. Line 38: sink instead of sank AR: This has been fixed. 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.” AR: The sentence has been replaced according to the reviewer suggestion. Line 79-80. I would delete the last sentence of the chapter. AR: The last sentence of the paragraph “However, to fully suppress hypoxia enhanced ventilation would be necessary the bottom waters of the Baltic Sea” is to emphasize that the bottom-water oxygen concentrations, are not only controlled by the nutrient loads, but also by physical factors like the frequency and intensity of the saltier water inflow, which had a decadal variability and it is modulated by meteorological forcing (e.g., Carstensen et al. 2014). This is explained in the next paragraph. Line 88: “. . . Carstensen et al. 2014)”. Recommendation: Salt water inflows from the North Sea. . . . AR: The sentence has been modified according to the reviewer recommendation. Line 91-92 “Saltier, denser, O<sub>2</sub>-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. AR: We modified this paragraph and combined those sentences as “At the time of sampling, this MBI had reached the Gotland Basin, but did not affect the LD, located further northwest.” to emphasize that the MBI oxygenated the deep waters of GB, but not those of LD (L97-98). Line 100: Please add the water depth in which the redox lines occur. AR: We added the redoxcline depth from literature (between 120

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and 150 m depth; L102). In the results, we define the redoxcline depth determined from the O<sub>2</sub> and H<sub>2</sub>S concentrations during our sampling. Line 99-100: This chapter can be shortened and combined with the chapter before. AR: We think that provide background information on the chemical reactions occurring in the redoxcline, and how previous MBI had altered redox conditions, is important to frame our idea that changes in oxygenation enhance the formation of MnOx that aggregate with POM and alter POM distribution and export Line 118-119: “enriched with OM; specifically with transparent exopolymer particles”. AR: This has been fixed (L122). Line 134-142: A clear objective is missing here. AR: This paragraph has been modified to clarify the objectives of the study (L138-146) Methods Line 146: “surface-tethered sediment traps” that’s not true. Traps were also installed in 180 m AR: As explained in the section “Sediment trap design and deployment”(L158-164) and in more detail in Engel et al. 2017 and Knauer et al. 1979, our traps consisted of 12 particle interceptor tubes (PITs) framed in five PVC crosses. Crosses with PITs tubes are attached at four depths: 2 at 40m, 60m, 110m, and 180m. The entire array is attached to the flotation gear that consists on a polypropylene line attached to two different types of flotation spheres. The entire flotation array is secured to the surface spar (a large yellow buoy) on which the flashlight and positioning systems are mounted. Line 147-148: depth of water sampling should be given here. AR: Water sampling depths are in table 2; this was added to the text (L152) Line 150: conductivity temperature depth? Suggestion: Temperature, salinity and O<sub>2</sub> concentration were determined at each station using a Sea-Bird (CTD) probe equipped with a oxygen (Oxyguard, PreSens) sensor that was calibrated. . . AR: This paragraph has been modified according to the reviewer recommendations (L153-154) 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. AR: The sediment trap depths were added to the text (L158). 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. AR: We thank the reviewer for this observation. The paragraph was corrected

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(L176-181). Line 191-192: Please add the wave lengths AR: The wavelengths were added to the text (440/685 nm; L195) Line 215-219: Suggestion: "Particle number and area was measured semiautomatically using an image analysis system including the WCIF ImageJ software. Image analysis of TEP and CSP were and conducted after Engel (2009). Additionally, TEP and CSP in water samples from the stations where we deployed sediment traps were analysed spectrophotometrically according to Passow and Alldredge (1995) and CisternasâAËY RNova et al. (2014) respectively. Why was the additional method ap- ËG plied? AR: We modified the sentence according to the reviewer suggestion (L219-222). The additional spectrophotometrically method was used to measured TEP and CSP concentration in the water column. Since the spectrophotometric method is less labor intensive, it allows for sampling of the water column with higher vertical resolution (between 9 and 12 depths; L223) 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. AR: We modified the TEP, CSP (L219-222) and MnOx (L230-232) image analysis section according to the reviewer suggestion. Line 226 and line 233: Please delete the word "directly". When storing samples, there is no direct measurement. AR: We deleted the word "directly" 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 rage where the thermocline was located during this short time. AR: The organization of this paragraph was changed according to the reviewer suggestion, i.e. Temperature, salinity and O2 conditions were discussed first in GB and then in LD. Information about temperature and salinity were added to the text. We include the thermocline

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information based on the measurements made during the deployment of the sediment traps. The depth range presented correspond to the initial and end depth were the temperature had a rapid decreased, this information was added to the text. Line 259: Which depth is meant with surface water? AR: We added to the text that surface water mean upper 10 m. Line 260: Suggestion for changes: (6  $\mu$ M at 80 and 140 m, and 0.12 260  $\mu$ M 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. AR: We added the information about upper, lower oxycline and OMZ core in this sentence (L278-279). Line 270: Because the conditions in water column are reported, it should be mentioned that nitrite had a maximum at 370m (Fig.2). AR: We added the maximum of nitrite at 250 m and nitrate at 400 m to the text (L291-292) Line 273-274: To which depth the second nutrient values apply; "the upper 110m" is confusing here. Line 275: 0.22 $\mu$ M? Line 269-276: The individual nutrients should be described one after the other and not switch between them several times. AR: We modified the nutrient section for clarity 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% . . . . AR: We modified this paragraph according to the reviewer suggestion (L310-311) Line 289: "The abundance of larger phytoplankton (>5 $\mu$ m) was determined by microscopy". The sentence can be deleted. It is described in the methods. AR: This sentence was modified (L313) Line 293: Filamentous unicellular cyanobacteria. A cyanobacteria filament always consists of more than one cell. A: We thank the reviewer for this observation, "unicellular" was deleted 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. AR: We deleted this sentence Line 297: 95% of what AR: Sorry, we do not understand this comment Line 302: which layer is meant with the surface. AR: With surface we referred to the upper 10 m of the water column. This was added to the text. Line 316: . . .decreased quickly at 10m. . ." Rather: . . .decreased quickly

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below 10m. . . AR: We thank the reviewer for this observation, the sentence was fixed  
 Line 318: The units of TEP and CSP should be explained in the methods. AR: We add  
 the units for TEP and CSP to the method section (L225-228) Line 324: “. . .were only  
 observed. . .” instead of “. . .were observed. . .” AR: This has been modified. Line  
 331: What is ESD? Please give the full name. AR: We added equivalent spherical  
 diameter to the text 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. AR: Yes, since we collected discrete samples at 40,  
 60, 110 and 180m, the assumption is that at each depth the sediment trap collected  
 the particles formed and sinking from the euphotic zone. 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. . .”. AR: This has been modified. Line  
 361: “. . .ranged from 0.6 to 16.5 mm (media mean 1.8) at 110 m (Table 4). AR:  
 We referred to the “median” as a measure of central tendency. The word “median”  
 was fixed in the text Line 371-381: These chapters would be easier to understand if  
 data are inserted. AR: We added the POC: PN ratio range to make the paragraph  
 easier to follow. Line 390: Please indicate in the method chapter how the DI has been  
 calculated. AR: We added the DI calculation to the method section (L240-244) 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. AR:  
 We replaced PP by phytoplankton biomass Line 410-411: “Pico-phytoplankton cell  
 abundance (cell mL-1) dominated the small phytoplankton size fraction < 5 $\mu$ m (Table  
 2), suggesting a significant contribution to PP and Chl a concentration. This can not  
 be deduced from the abundance alone. AR: We deleted the sentence “suggesting a

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significant contribution to PP and Chl a concentration” Line 421-422: “Cell abundance  
 of total phytoplankton (>5  $\mu$ m) 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. AR: To determine if the total phytoplankton abundances (considering all  
 groups presented in table 3) were significantly different we used the Mann-Whitney  
 U-test. The p-value (0.74) indicates that there is not sufficient evidence to indicate  
 that the medians of those two data set were significantly different. 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. AR: We corrected this paragraph. Line 435-437: “. . .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. AR: We modified this paragraph to make clear that even  
 though we sample in June, the high Chla concentration and the phosphate still present  
 in the water column could indicate that PP was not nutrient limited yet. The presence  
 of nutrients (phosphate) may be an explanation of why TEP concentration was lower  
 than reported before for summer in the Baltic Sea when PP was low due to nutrient  
 limitation. 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. AR: We  
 modified the text according to the reviewer recommendations Line 452-453: “MBIs  
 can have a major impact on nutrient recycling“. Such general statements should be  
 reduced throughout the ms. AR: This sentence was moved to the beginning of the  
 paragraph since was introducing the effect of MBI in nutrient distribution. Line 480: “. .  
 . . Carbon flux below the euphotic zone. . .”. To the bottom or to what depth? AR:  
 For clarification, this sentence was modified to “Our measurement of carbon flux at

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40 m , below the euphotic zone, were. . ." 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. AR: We agreed with the reviewer that our study represent only one discrete measurement; however, the objective of mention the results of previous estimations from modeling studies was precisely to add some context to our results. We re-organize the discussion to make it clearer and more focus. 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 $\mu$ m or 100 $\mu$ m length, as it is usually performed. AR: The filamentous cyanobacteria were counted as single filament as it is usually performed. The word unicellular was deleted from the table. Fig.2A: The scale of the x-Axis for salinity is wrong. AR: We thank the reviewer for this observation; the salinity scale in figure 2a is fixed 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. AR: We added all values to figure 4, included the depths with zero particles.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-360/bg-2018-360-AC1-supplement.pdf>

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