

Biogeosciences Discuss., referee comment RC1
<https://doi.org/10.5194/bg-2021-357-RC1>, 2022
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Comment on bg-2021-357

Anonymous Referee #1

Referee comment on "Significant nutrient consumption in the dark subsurface layer during a diatom bloom: the case study on Funka Bay, Hokkaido, Japan" by Sachi Umezawa et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-357-RC1>, 2022

Umezawa et al. "Nutrient consumption by diatoms in the dark subsurface layer of Funka Bay, Hokkaido, Japan".

In Umezawa's manuscript, the nutrient decline in the twilight zone is reported. They attributed the decline does not occur by physical processes, occurs by diatom consumption, that is biological processes. Very exciting manuscript. They did laboratory experiments, and the result supports the nutrient consumption by diatoms below the euphotic layer. I believe this manuscript will be published from Biogeosciences after significant revisions. To support the authors' idea more strongly, some results and discussion are necessary.

1) Effects of the physical processes.

The authors denied the vertical mixing and horizontal mixing are the main reasons to decline the nutrient concentration at 30–50 m depth of this area. However, the water density on 15th March is lighter than that of 4th March. The authors did not show the horizontal distributions of density and nutrient concentration. So the subduction processes cannot be denied. When the authors have much data, the nutrient(nitrate)-density plot would help discussion on it. I am concerned about the possibility that the low-nutrient water formed in the euphotic layer in the other area subducted at the observation station. For example, such a phenomenon occurs in an anticyclonic eddy.

The other processes possible process is the nutrient diffusion process. I don't know the diffusion of this area, > several tens $\mu\text{mol N m}^{-2} \text{d}^{-1}$ usually occurs in the ocean. The nutrient diffusion occurs with physical disturbance, but diapycnal nutrient flux must be considered. The observations were snapshots, and the authors may not observe the diffusion processes, but the authors must show the nutrient flux at 50 m and 30 m depths are balanced on 4th and 15th March based on the slope of the nitracline and pycnocline, and this process is not the major nutrient decline process.

2) Impact of the biogeochemistry and primary production

The authors concluded "This consumption could result in reduced new production in the subsurface layer after the bloom, when this layer would once again become part of the euphotic zone, if the diatoms sank to deeper layers." However, I cannot agree with this without the evidence that the diatoms are not increased in this layer. Diatoms have some unique modes of nutrient uptake (Martin-Jezequel et al. 2000). Is the observed nutrient uptake of diatoms in the dark condition not linked to the growth? When the authors have the time-series chlorophyll a concentration data in the laboratory experiments, please show the data and discuss that they did not fix carbon. In the case of cyanobacteria, they grow up in the twilight zone (Sohrin et al. 2011). In addition, the dark condition in the laboratory may be different from the dark condition of the field. Even though the PAR is less than 0.1% at the surface, it was not completely dark in the ocean. Many exciting discussions may be possible: nitrate uptake (new production) may occur in the twilight zone but not contribute to the primary production/ new production is underestimated when the nitrate uptake is not measured in more dark layers.

Martin-Jezequel, V., M. Hildebrand, and M. A. Brzezinski (2000), Silicon Metabolism in Diatoms: Implications for Growth, *J. Phycol.*, 36(5), 821-840, doi:10.1046/j.1529-8817.2000.00019.x.

Sohrin, R., M. Isaji, Y. Obara, S. Agostini, Y. Suzuki, Y. Hiroe, T. Ichikawa, and K. Hidaka (2011), Distribution of *Synechococcus* in the dark ocean, *Aquat. Microb. Ecol.*, 64(1), 1-14.

3) The structure of the manuscript

It was just an opinion, but I am familiar with the manuscript which divides results and discussion. I believe the authors can divide them. However, if the authors considered the present style is better, this is not mandatory. This is the option, too, but the title of the manuscript should be a more appealing one. The present title only attracts local interests. For example, when the authors consider the observed phenomenon possibly occurs everywhere under diatom blooms, the title can be revised as "Significant nutrient consumption in the dark subsurface layer during a diatom bloom: the case study on Funka Bay, Hokkaido, Japan".

Minor comments

Abstract

L14: Times of observations are necessary. Technically, the authors' observation is not time-series, because the observation was conducted randomly.

L21 "We believe that this is the first study to present observational evidence for the consumption of the main nutrients by diatoms in the dark subsurface layer during the spring bloom." In my opinion, this sentence is not important. Instead of this sentence, the authors should add why they considered the nutrient decline does not occur by physical processes.

Introduction

L26 "Si: NO₃⁻ ratio". Yes, this is not wrong and described in Harrison et al 2004, but Si(OH)₄:NO₃ or Si: N ratio is more appropriate. This is just opinion.

L31–32: References are required.

L34–35: "From time-series observations in the bay, it is possible to examine the temporal changes of biochemical parameters within the same identified water mass while the water is in the bay." I cannot understand this sentence clearly. What means "while the water is in the bay"?

L37–39: "A massive spring bloom dominated by diatom species occurs in March every year before the Oyashio water flows into the surface of the bay, and it lasts until late March or early April, when Oyashio water occupies the surface of the bay (Odate 1987; Maita and Odate 1988)." I cannot understand this sentence. Please clarify. Can the author divide it into two sentences?

L58–61: I could not find any meaning in these two sentences. I cannot see any discussion of VOIs in this manuscript. In addition, the reference is under consideration. So I considered these two sentences should be removed.

Method:

L72–73: "Observations in Funka Bay have been reported elsewhere (Shimizu et al. 2017)." What did the authors want to describe? Some information on the observations conducted in 2019 was described in 2017? Describe the details or remove the sentence.

L78: How do the authors calculate the analytical precision? This is very low. Did the authors measure the nutrient concentration of not-frozen samples? If the results are frozen samples, the precisions are too good, in particular, silicate.

L82: Where *Thalassiosira nordenskioeldii* come from? Algae collection?

L95: "We set the initial concentrations of nutrients at 23 times those of the first dark incubation." Why did the authors set so high initial nutrient concentration? The environments are very different from the field observations.

* Did not the authors conduct the microscopic observations? Is *Thalassiosira nordenskiöldii* the dominant species of the observations? This is very important. Because the other species is dominant, the authors' incubation experiments are meaningless.

Results and Discussion

L102–106: Please define the water masses at the materials and methods. When the authors defined in the materials and methods section, the results will be simpler.

L106: "The revised classification result" This is unclear. Did the authors revise in this manuscript or revise in Ooki et al. 2019?

L115–119: This is not a result. Please define in the materials and methods section.

L123: "the original data in supplementary information of Ooki et al., (submitted)." Is it right? I can see the supplementary information of this manuscript.

L124: "The data for chl-a are taken from a related article." What does it mean? I think it is acceptable to share the data with other manuscripts.

L150–168: These paragraphs were "discussion". These discussions can be put after the results section because the results after this paragraph are not contained the results of the discussion. For me, this style is hard to follow.

3.3.2 This section (results of incubation experiments) should be shown before 3.3.1.

L270: Villareal et al. reported *Rhizosolenia*, and not *Thalassiosira*. Do the authors have any evidence on the vertical migration of *Thalassiosira*? If not, this discussion is speculative. In addition, the authors' names are wrong: Wirtz and Lan Smith (2020) are correct.