

***Interactive comment on* “Field-obtained carbon and nitrogen uptake rates of phytoplankton in the Laptev and East Siberian seas” by Sang Heon Lee et al.**

Sang Heon Lee et al.

sanglee@pnu.ac.kr

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Interactive comment on “Field-obtained carbon and nitrogen uptake rates of phytoplankton in the Laptev and East Siberian seas” by Sang Heon Lee et al.

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The authors investigated the carbon and nitrogen (nitrate and ammonia) assimilation rates of phytoplankton in the Laptev and East Siberian Seas in late summer of 2013. Overall, I agree that the data obtained from this study are precious to better understand the biogeochemical and ecosystem processes of the less studied regions in the

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Arctic. However, in my view, the present manuscript is too descriptive, and it contains a number of ambiguous or uncertain issues. For example, below are a few severe weaknesses in this paper. As a result, I am sorry that I cannot recommend this paper for publication in the journal Biogeosciences at the present form. =>We carefully revised our manuscript based on the comments as below.

1) Lack of optical data during observation. Even for the determination of optical depths, the authors used a legacy Secchi disk technique. Please clarify the accuracy of the optical depths determined in this study. If not, the primary production data may not be reliable – an underwater PAR sensor or spectroradiometer should be used for determining the euphotic layers. => It would be better to have radiance or optical measurements for more accurate estimation of euphotic depths or diffuse attenuation coefficients for PAR, $K_d(\text{PAR})$. Since we, however, have no underwater PAR sensor (and/or optical instruments) available due to logistic problems (we missed our luggage from airplanes on the way to the Arctic cruise and received them two months later after the cruise), the light depths were determined by Secchi disc which has been widely and commonly used in various oceans as well as the Arctic Ocean to derive euphotic depth and $K_d(\text{PAR})$ (Son et al., 2005; Tremblay et al., 2000; Lee et al. 2012; Lee et al., 2017a; Lee et al., 2017b). From several previous studies in the Arctic Ocean, we are pretty much confident with the Secchi depth to get the euphotic depth since the comparison of the light depths between the two methods of Secchi disc and underwater PAR sensor were matched quite well. We added this sentence in line 102-111, pages 6-7. In this study, the authors incubated the seawater samples for 4 to 6 hours on deck. However, no information is available for the surface PAR during incubation. Were these irradiance levels constant among stations? Also, the authors assumed 24-h daylight conditions in the summer period (L186–187). Were the light levels also constant at every station throughout the day? Please clarify these optical measurement issues. =>We incubated the seawater samples on deck under natural light conditions with cooled with surface seawater for 4 to 6 hours. So, the irradiance levels were not constant among stations like natural light conditions (as we

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mentioned in line 120-123, pages 7-8). The 24-h daylight assumption for the daily carbon and nitrogen uptake rates was applied to this study for a comparison purpose with previous studies (Subba Rao and Platt 1984; Lee and Whitedge 2005; Lee et al., 2010) in line 221-222, page 13. Actual day lengths were about 20 hours per day during the cruise period. As an explanation for the lower f-ratio values observed in this study, the authors suggested potential light-limited conditions for phytoplankton growth in the study period (L220–222). Unfortunately, the authors did not show any optical or bio-optical data such as photosynthesis-irradiance parameters. => A potential light-limited condition is one of our hypothesis based on lower f-ratio despite with some nitrate available within the euphotic layers and no strong relationship between f-ratio and euphotic water depth-integrated concentration of nitrite+nitrate found in this study. However, the conclusion might be needed for caution as the reviewer's comment. We added more discussion on that in line 257-266, page 15. 2) For a comparison between in situ and satellite remotely sensed primary production in the study area, the authors solely used the mean value in the study area during 1998–2008 reported by Arrigo and Dijken (2011) with a few assumptions. As a conclusion of this study, the authors noted that further careful validation would be required for the use of satellite data (L285–288 and L322–325). It is a shame that the authors did not make any effort to match up their in situ data with satellite-based estimates in the observation period more precisely. => We hoped to do that, but unfortunately no annual primary production estimated in 2013 by Arrigo and Dijken (2011) makes difficult for a direct comparison of annual productions in 2013 between our measured rates and their satellite-based rates. We discussed more on potential reasons for the discrepancy between this study and satellite study in line 319-326, pages 18-19 and line 339-352, page 20. Minor comments: L22: $p > 0.01$. Is this level statistically significant? => We revised it in line 22. L22: Remove "Unexpectedly" from the sentence. It could be common that the data obtained were within the previous reported values. => We removed it. L52: Delete "of primary producers" from the sentence. The words are redundant. =>We deleted it. L58–59: Cite a reference at least for the sentence

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that the Laptev and East Siberian seas are situated on the wildest and shallowest continental shelf in the world. => We added the reference in line 59. L65–66: List the references chronologically. => We revised it in line 68-69. L72: marine ecosystems => We revised it. L97: Lee et al., 2007; 2012; Yun et al., 2015). => We revised it. L98: How did the authors convert Secchi disc depth to light intensity? => We rephrased the sentence in line 102, page 6. L101: NaH_2PO_4 ? L108–109; Did the authors remove particulate inorganic nitrogen? If not, particulate nitrogen (PN) would be a better expression. => We revised it with PN. L115: How about the discrimination factor for $^{13}\text{C}/^{12}\text{C}$? => Actually, we did not consider the discrimination factor for $^{13}\text{C}/^{12}\text{C}$ since it is considered too low. We added some potential underestimated in line 135-137, page 8. L129: Insert “values” between “salinity” and “ranged”. => We inserted it in line 147, page 9. L133: from the surface => We revised it. L139–140: I was a bit confused with the sentence that they were relatively higher in the Laptev Sea than in the East Siberian Sea. How did the authors separate the former from the latter? Where is the boundary between the two seas? Also, in Table 1, please classify the stations into the two seas. => We rephrased the sentence (Laptev Sea=> Western part; East Siberian Sea=> Eastern part) in line 165-169, page 10, since it is not that clear for the separation. L141: the patten of silicate concentration showed opposite... The verb “appear” is an intransitive verb, so it cannot be used for the passive. => We revised it in line 167, page 10. L148: phosphate and nitrate were so low... => We revised it in line 174, page 10. L152: concentrations => We revised it in line 185, page 11. L155: Again, $p > 0.01$. Is this statistically significant? => We revised it in line 188, page 11. L169: at the surface => We revised it in line 203, page 12. L172: rates were => We revised it in line 206, page 12. L266: these production levels => We revised it in line 316, page 18. L267: mean production estimates => We revised it in line 317, page 18. L344: The “2” in CO_2 should be subscript. => We revised it in line 413, page 24. L381: The “13” should also be subscript. => We revised it in line 458, page 25. L427: at the productivity measurement stations => We revised it in line 529, page 28. L452: Use subscript for the number of NO_2+NO_3 , NH_4 , PO_4 , and SiO_4 . => We subscripted

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them. L439, 441, 453, and 454: The unit of chl-a concentration would be mg m⁻². => We revised them all. Fig. 4: Insert a space between “20” and “_m”. => We revised it.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-234/bg-2017-234-AC1-supplement.pdf>

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