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Interactive comment on "Influence of timing of sea ice retreat on phytoplankton size during marginal ice zone bloom period in the Chukchi and Bering shelves" *by* A. Fujiwara et al.

Anonymous Referee #3

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Review of "Influence of timing of sea ice retreat on phytoplankton size during marginal ice zone bloom period in the Chukchi and Bering shelves" by Fujiwara et al.

This paper describes a remote sensing study of the Bering/Chukchi Sea region focusing on the relationship between timing of sea ice retreat and phytoplankton size structure. The paper also utilized reanalysis data for variables not available through remote sensing. I found the approach used by the paper to be interesting, even if the results generally conformed to previous observations. Still, I believe that the authors have demonstrated the promise of retrieving phytoplankton size structure from satellite and using this variable to better understand the dynamics of phytoplankton blooms. I think that the paper is well organized and suitable for publication in Biogeosciences

after some modifications are made to the paper. I found nothing major wrong with the paper, but lots of small things that need to be addressed.

The paper needs extensive editing of grammar and spelling. I started to make corrections, but they very soon became too numerous for me to continue. There are also some minor issues of interpretation and consistency that need to be addressed:

Page 3, line 5. Food webs are simple, not short.

Page 5, line 5. The statement that "Since phytoplankton grazers efficiently use the high phytoplankton biomass produced during bloom periods for their growth and production. . ." directly contradicts the statement on Page 2, line 23-24 that "The high primary production in the region is not completely consumed by the grazers in the water column due to low grazing pressure".

Page 8, lines 10-13. The statements that ". . .but ABPM retrieves optimal values of chla normalized productivity (PBopt) from aph(λ) instead of from SST and chla. . ." and ". . . because PPeu by ABPM is independently derived from temperature. . ." are inconsistent. The first indicates that PPeu is independent of temperature and the second states that PPeu is derived from SST. Both statements cannot be true.

Page 9, lines 17-19. The substitution of annual median PPeu for missing pixels seems dangerous considering that PPeu increases significantly over the course of the 16-year study. The authors should attempt to evaluate the consequences of this correction.

Page 11, section 3.1. The authors describe the accuracy of FL estimated from satellites by comparing it to in situ values. They conclude that the performance is acceptable, but only focus on a few metrics. They never give the slope of the relationship, which differs greatly from the hoped-for 1.0. This metric should be added. Also, the satellite overestimates FL by 40% at high values, hardly a "slight overestimate." The authors should be more candid about the shortcomings of the algorithm. I was actually encouraged that there was a significant relationship between the satellite-derived and in situ

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FL. I wish the authors had tried to explain why the slope differed from 1.0, rather than just glossing over this point.

Page 12, lines1-6. It would be useful here to have some vertical profiles of FL and PP to see how they vary with depth.

Page 14, line16-17. The authors need to clarify that over most of the Arctic Ocean, omission of the SCM results in only a small error in PPeu (Ardynya et al. 2013, Arrigo et al. 2011).

Page 14, lines 20-21. I don't understand the basis of their assumption that just because there is a good relationship between surface PP and PPeu, then surface FL can be used to infer FL throughout the water column. I see no reason why this should be true.

Page 16, lines 1-5. This statement is true mainly because the dates of CMAX and ice retreat vary by such a small amount in the northern Bering Sea.

Page 16, lines 15-18. This statement is very vague and seems out of place in this paper. Add specifics or make its relevance more clear.

Page 17, lines 11-13. I don't understand the logic of this statement, assuming that nutrients are mixed uniformly throughout the water column on the Chukchi Shelf in winter. Surface nutrients will remain high until phytoplankton draw them down. This could happen early because of under-ice blooms or because of early ice retreat.

Page 17, lines19-22. Sufficient light can [penetrate first year ice for phytoplankton to grow only if ice has melted and ponds have begun to form.

Page 18, line 7-8. This statement is true mainly because the dates of CMAX and ice retreat vary by such a small amount in the northern Bering Sea.

Page 19, line 19 and Page 20, lines 17-18. The idea that FL controls APP is probably wrong. Both of these variables are controlled by nutrient availability. More nutrients leads to both larger cells and higher production.

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