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Interactive comment on “Control of primary production in the Arctic by nutrients and light: insights from a high resolution ocean general circulation model” by E. E. Popova et al.

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Overall Comments

Comment:

“The constant comparison of modeled results to the literature throughout the results section makes it difficult to focus on the results of the model. I would prefer the authors to put much of the literature review into the introduction or the discussion.”

“The length of the paper is tiring. There is so much literature on AO PP that it's not necessary to review it all here. Just make your observations and directly reference any

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papers that support it."

Response:

As suggested by the reviewer, we moved the review-style comments from the Results section to the Introduction. The Results section of the manuscript has been shortened and the over-detailed description of the productivity observations reduced to relevant citations only.

Comment:

"Also, to limit the comparison to Pabi et al. results until the discussion, where a simple difference plot between model and satellite would be enough. As the satellite results are also just a model with large errors, it is confusing to keep refereeing to it as absolute observations, and I am not sure that it adds anything to the paper. What is interesting is the investigation of possible futures based on current model results."

Response:

We fully agree with the reviewer that satellite-derived estimates are just another model and should not be referred to as observations. However, we prefer to retain comparisons with these estimates for each of the provinces separately, rather than moving everything into the Discussion for two reasons. Firstly, the two types of model are quite different and the discrepancies highlight important issues (both with our model and with satellite-derived algorithms) which are different for the various provinces (e.g., levels of productivity in the areas affected by riverine input which are mentioned in section 3.3.3). Secondly, satellite based models are presently the only means of obtaining high resolution estimates in time and space allowing, for example, estimation of the MIZ contribution to pan-arctic productivity. Nevertheless, in order to take into account the reviewer's comment that satellite-derived estimates are not measurements, we have re-enforced the first paragraph of section 3.3 about issues associated with these estimates and changed the reference wording throughout the Results section.

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Major Questions

"Q: The light field is important in the spring, but the summer is nutrient not light limited so the averaged UML short wave radiation might not be a helpful metric. Light is also related to sea ice area so which metric is actually driving PP? "

Response:

The reviewer is correct in that, on an annual basis, the large-scale features of primary production are not driven by UML-average short-wave radiation because of (1) surface nutrient limitation, and (2) the significant contribution of phytoplankton in the deep (sub-UML) chlorophyll maximum. This metric is very similar to surface short wave radiation and we removed it from the analysis and from Figure 5.

It is not however possible to give a clear answer to the reviewer's question: "which metric is actually driving PP?". Both nutrients and light are controlling factors and the whole paper is devoted to the description of the particular regional features of this co-limitation. An additional complication is that the ice field affects both stratification (hence nutrient supply over the majority of AO) and light levels. Most of the Discussion section describes the complexity of this interaction. To simplify the matter, or at least provide a different perspective, we have introduced a multiple regression analysis. As with any regression analysis, it does not prove causality but does show large-scale tendencies, highlighting the importance of ice (where ice is present) and mixing (where ice is not present).

In order to clarify the reviewer's metric question, and to answer the next question (below), we have rewritten section 3.5.

"Q: Really don't see the link between mixing, nutrients and light and PP on page 34."

Response

It would appear that this question arises because of the first paragraph of the section 3.5 where we stated "a priori" that primary production should depend on short-wave

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radiation and mixing, which may not necessarily be the case. We hope that the reply above, as well as alterations to section 3.5, clarify this issue.

"Q: Multiple regression between model parameters, but wasn't the model PP dependent on light and mixing anyway?"

Response

The model primary production explicitly depends on light but not on mixing. The expression for primary production includes light limitation and nutrient limitation terms. There is no "a priori" reason to assume that vertical mixing (and in particular the maximum depth of winter mixing) should drive nutrient concentration and hence productivity (indeed, this is discussed in section 3.5, showing that it is not the case in, for example, the Chukchi Sea). Changes to the beginning of section 3.5 have clarified the regression question.

Major Corrections

Comment:

"Section 2.1.4. Defining geographical regions. The regions as defined do not contain consistent biological characteristics. The shelf seas do not extend to 90°N, by using simplistic pie sections to define the regions the authors introduce large errors in the biogeochemistry of an area. The shelf seas are vastly different in terms of nutrient and biological cycling compared to the central basin. The provinces described by Carmack are much better suited to comparing biogeochemical regions"

Response:

We agree that provinces described by Carmack can be a more logical choice than the pie section, however Carmack's provinces are very difficult to introduce numerically as they do not have a clear latitude-longitude-depth boundaries (except for the shelf break). For that reason we have described our results both regionally through the pie sections (section 3.2), and by ecological provinces (section 3.3) where we generally

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follow Carmack's system (e.g. inflow, interior and outflow shelves and alpha and beta ecosystems).

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Comment:

"Section 3.1: The first paragraph is not results, but introduction."

"Section 3.2: The first 2 paragraphs here are also not results, but introduction."

Response:

Both have been moved to the Introduction.

Comment:

"Figure 4: The text does not persuade me of the necessity of this figure, as a reprint of a figure in another paper could the authors just refer to the previous paper. Why do the authors rely on another paper for comparison of their model to observations when the satellite derived ice concentration data is freely available and could be compared within this paper. Also, this section on the model performance should be in a section by itself or in the methods to show that the model is able to reproduce the observations well."

Response:

We agree with this comment. Figure 4 has been removed. We have also removed references to Pabi et al. in the ice comparison and now refer to the data directly. However, we retain ice model-data comparison in the same section since it has direct implication for the productivity and we believe that it logically belongs here.

Comment:

"Page 14: Line 25: Given the short time that the central Arctic basin is ice free, does it really have a higher UML averaged short wave radiation than areas which are ice free throughout the summer? If the calculation is in fact the maximum UML averaged

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SW radiation on a per day basis I understand, but is that value really physiologically important?"

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Response:

The reviewer is correct, there was an error in the original calculation of this field. After recalculation this metric is very similar to the short-wave radiation and thus does not add much to the analysis (see earlier remarks). It has been removed from Figure 5 and the main text. Since it was solely a diagnostic field, its miscalculation had no effect on any of the results.

Comment:

"Page 18: Authors should also add that the vertical distributions of biomass used in the satellite model are not accurate and introduce errors. i.e compare the exponential profile used in Pabi with in situ profiles in any Arctic paper discussing PP."

Response:

We added the following text:

"In addition, the vertical distribution of Chl-a in the AO is characterised by a strong sub-surface maximum (e.g. Hill and Cota, 2005). Exponential decline of the Chl-a below the UML utilised by Pabi et al. 2008 is likely to have introduced significant errors in the estimate of depth-integrated primary production."

Comment:

"Page 19: Does the model calculate production as a function of light available for photosynthesis? This means that even although PP is most likely nutrient limited, the increased light availability due to lower modeled ice concentrations will produce higher PP for the under ice region."

Response:

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The reviewer is correct, taking into account importance of the subsurface chlorophyll maximum, increasing light would increase production even if it is nutrient limited in the UML. We removed the sentence in question.

Comment:

"Page 19: There are significant differences in the nutrient and mixing regime between the Amerasian and Eurasian basin which should be discussed in terms of PP in the ice regime."

Response:

We are not sure that Amerasian and Eurasian basins as divided by the Lomonosov ridge are the most natural primary production sub-domains for the AO ice province. From our perspective, Western versus Eastern ice province might be a better choice. The Western province is characterised by thicker ice that leads to lower mixing but generally higher nutrients in the nutricline (mostly of Pacific origin). The Eastern province has thinner ice with lower summer concentrations that lead to more intense mixing, a better light regime but at the same time lower nutrient concentrations in the nutricline as this region is more influenced by nutrient-poor Atlantic waters. Based on the above zonation we have added the following text:

"Both in the model and observations, the western domain of the ice province (northern parts of the Baffin, Beaufort and Chukchi sectors) is characterised by older and thicker ice with lower summer concentrations (Figure 3c,d) than the eastern domain (northern parts of Greenland, Barents, Laptev, Kara and East Siberian sectors). The anticipated consequences of these differences are a less favourable light regime and decreased vertical mixing in the western domain. Although the model shows differences in light level (Figure 5a, b), the UML dynamics are similar throughout the whole ice province (Figure 5c, d). However, the western domain is significantly influenced by the inflow of nutrient-rich Pacific seawater and is characterised by higher nutrient concentrations than the eastern domain where Atlantic inflow occurs (Figure 5 a,b). Thus, relative to

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the eastern domain, the modelled primary production in the western domain is limited by light to a higher degree and by nutrients to a lesser degree. Although these two factors provide opposite influences on the productivity that nearly cancel each other out, the impact of nutrients prevails and primary production in the western ice province is about a factor of 2 to 3 higher than in the eastern province."

Comment:

"Page 21, Line 22: Not just comparable but actually not statistically different, as 132 +/- 10 and 127 +/- 8 g C m-2 y-1"

Response:

Corrected.

Minor

Comment:

"Page 3, Line 2 – A record minimum of 4.2 million km². This statement would make more of an impact if the authors include the average ice extent from beginning of satellite records."

Response:

We have added: "compared to 1979-2000 mean of 7.0km²:

Comment:

"Page 3, Line 8: check spelling of absorb."

Response:

Corrected.

Comment:

"Page 3, Line 10: There is no evidence to suggest a 60% increase in AO PP over the

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last decade "

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Response:

The reviewer is right, it is an incorrect quotation. The new text reads:

"annual production in 2007 exceeding the 1998-2002 mean by 23\%"

Comment:

"Page 11, Line 8: This sentence is confusing, "nutrients available for primary productions and mixed-layer depth?"

Response:

Typo; now reads:

"nutrients available for primary productions in the mixed-layer depth"

Interactive comment on Biogeosciences Discuss., 7, 5557, 2010.

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