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Comment

## ***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.**

### **Anonymous Referee #2**

Received and published: 9 September 2010

Overall Comments: 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. 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. 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

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any papers that support it.

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? Q: Really don't see the link between mixing, nutrients and light and PP on page 34. Q: Multiple regression between model parameters, but wasn't the model PP dependant on light and mixing anyway?

Major Corrections

Section 2.1.4. Defining geographical regions. The regions as defined do not contain consistent biological characteristics. The shelf seas do not extend to 90oN, 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

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.

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 reply 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.

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?

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.

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.

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.

Page 21, Line 22: Not just comparable but actually not statistically different, as  $132 \pm 10$  and  $127 \pm 8$  g C m<sup>-2</sup> y<sup>-1</sup>

Minor

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

Page 3, Line 8: check spelling of absorb.

Page 3, Line 10: There is no evidence to suggest a 60% increase in AO PP over the last decade  
Page 11, Line 8: This sentence is confusing, “nutrients available for primary productions and mixed-layer depth?”

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

**BGD**

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