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Biogeosciences Discussions

Interactive comment on "Increasing cloudiness in Arctic damps the increase in phytoplankton primary production due to sea ice receding" *by* S. Bélanger et al.

Anonymous Referee #1

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General comments

The paper is dedicated to a very topical issue, and thus certainly is of interest. However, I find it very poorly substantiated and lacking some important proves of the statements made by the authors. The illustrations are of so small size that they become nearly useless. It is very deplorable because the figures are almost the only presentation of quantitative results in the paper.

Concrete comments

1. The methodology is given unsatisfactorily. Daily PP model (eq. 1): no reference to a more detailed description of this model. It is necessary to provide a derivation of

the model, with an explicit discussion of the boundary conditions, the model sensitivity to vertical profiles of CHL, and other colour-producing agents. The authors neglect the dependence of PmB on SST. What errors might it inflict? Results of verification of the model with in situ determinations are mandatory, but they are absent. The model simulates daily PP rates. The spaceborne Chl, IOPs and cloud and ice concentration data employed are monthly. Calculated spectral Rrs values are monthly. How is all reconciled? The authors refer to an excellent performance of the algorithm retrieving the total absorption and backscattering coefficients. However, they apply the algorithm to gigantic oceanic tracts. Are there proves that the algorithm performs well across all oceanic waters covered by the research? No attempts of this sort are explicitly undertaken by the authors. The analysis of errors of the algorithm performance. Analysis of the PP model sensitivity to total a and bb retrieval errors is absolutely mandatory, and it is absent as well. The authors write that they fill the gaps in monthly means of the IOPs and Chl with monthly climatological values of IOPs and Chl? What errors does this procedure induce? Please, provide the methodology of quantifying PAR(0-) in more detail. Could the authors specify the values of PP in the areas under clouds? What is the ratio (outer limits) between the PP values under cloudy and cloud-free atmospheric conditions? May be you can provide a statistically substantiated table of such ratios for different provinces you are discussing in your paper? It is known that the retrievals within the areas immediately contouring the projection of clouds on the water surface are inaccurate. How did the authors combat this problem? Eq. 2. Again a derivation of this expression is necessary. The expression for the mean cosine of downwelling irradiance employed by the authors has its limits of application; they are not specified. It would be much better to use the expression suggested by H. Gordon. The authors do not provide the reasons of choosing the expression by Sathyendranath et al. The empirical dependence between Chl and aph employed by the authors was obtained for a very limited geographical province. Application of it to the entire area of research is certainly a stretch, and an assessment in depth of errors arising from it are mandatory, but they are absent. 2. In order to draw and analyze trends in PAR and PP

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it is necessary to assess the error bars, and only after that to draw the trends: the error bars might be as high as ± 30% - 50% or even more, and drawing trends is a procedure requiring special investigations. Without such assessments the trend significance determined by the authors (regardless of the sophisticated procedures employed by the authors) is unclear. 3. The statement (p. 13995, bullet 20) that the PP values determined by the authors are more accurate that the ones reported previously is not convincing in light of the comments made above. This statement is particularly astonishing because the authors haven't shown the accuracy of their PP retrievals even for some local areas/seas (although such data are available). 4. P. 13996 The statement that the largest increase in PP occurred in May is too generalized: in pelagic and shelf seas it is different. Your data should allow you documenting this issue in more detail. 5. The choice of the ratio Chl/KPUR as an indicator of ocean optical properties seems inappropriate: both of them are, firstly, interdependent, and secondly, the concentration of Chl is a function of other factors that are not related to hydro-optical properties. The conclusion drawn by the authors are at least debatable. The statement on p. 13999 bullet 15 is unsubstantiated. At least, the authors should clarify why in conditions of decreased PAR(0-) and increased KPUR the situation can only be explained in terms of a change in ocean optical properties (whereas there are plenty of other, non-optical, factors that could be important players) The phrase "In several Arctic sectors. . ." is not concrete and lacks references. On page 14000 the authors declare: CDOM production from microbial activity is. . .delayed' Right, but what is the delay? Please, specify. Because it is my impression that offering the data in Fig.5a, the authors imply the delay of about 1 month. Please, provide quantitative data or references. 6. P. 13999: the authors explain low values of the ratio Chl/KPUR during June-August exclusively by the microbial production of CDOM. Given a very intensive water transport of Atlantic waters into the Barents Sea, and a further transfer of Barents waters to the East and in the White Sea, it appears unrealistic that the effect of CDOM on Chl/KPUR should last so long. It seems to me that this case explicitly shows inappropriateness of the chosen ratio as an analytic tool. Other factors can come in to play. For instance for August,

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the authors do not consider the impact of E. huxleyi blooms in the Barents Sea. 7. P. 14001, bullet 5: The authors' speculations are indeed sheer speculations. Without reliable data, it is better to abstain from such suppositions. 8. P. 14001, bullet 10: the subsurface maximum in Chl and the respective PP additions in the water column are not small. PP is underestimated by 10-11%. It is strange that the authors for their argumentation cite the work by Arrigo and Dijken: the latter, contrarily, state that Chl subsurface maxima are characteristic of the Arctic Basin and their contribution to PP should be accounted for. This is supported by our data as well. 9. It is correct that under-ice phytoplankton blooms can possibly represent a significant portion of the Arctic annual PP. But it should be taken into account that the length of the ice-edge in the Arctic decreases progressively, and the importance of this factor is expected to rapidly drop in the years come. Nothing of this is mentioned in the paper. 10. In the conclusions, the authors enumerate the problems to be solved/ the knowledge that needs to be furthered. The list is incomplete and lacks such factors as nutrients availability, stratification conditions/water freshening, change of the phytoplankton composition in conditions of climate warming, the effect of increasing occurrence of deep cyclones capable of increasing PP, the impacts of NAO, PDO &IPO, and AO on the system of currents, and some others.

11. Summing up, I have to underline that for me as a reader, the paper's results will appear persuading only on condition that the authors revise substantially their paper at least answering the questions and meeting the recommendations posed above.

Interactive comment on Biogeosciences Discuss., 9, 13987, 2012.