Biogeosciences Discuss., 9, C8435–C8441, 2013
www.biogeosciences-discuss.net/9/C8435/2013/

Biogeosciences
Discussions

Interactive comment on "Plankton ecosystem functioning and nitrogen fluxes in the most oligotrophic waters of the Beaufort Sea, Arctic Ocean: a modeling study" *by* V. Le Fouest et al.

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Received and published: 21 February 2013

We gratefully thank referee #2 for her/his constructive comments with respect to our manuscript results, discussion and conclusion. In order to improve the manuscript with respect to these comments, we amended the manuscript as suggested by the referee wherever it was possible.

1.Abstract

Line 2: "Greater stratification in the Arctic, did stratification really increased everywhere in the Arctic?"

The sentence was confusing. We modified it as follows:

Page 14753 (line 2): "In some areas of the Beaufort Sea, the stronger haline stratification observed in summer alters the plankton ecosystem structure, functioning and productivity promoting oligotrophy (Li et al., 2009)."

Line 7: "functioning of what?"

We modified the sentence as follows:

Page 14753 (line 2): "A one-dimension (1-D) physical-biological coupled model based on the large multiparametric database of the Malina project in the Beaufort Sea was used (i) to infer the plankton ecosystem functioning and related nitrogen fluxes and (ii) to assess the model sensitivity to key light-associated processes involved in nutrient recycling and phytoplankton growth."

Lines-12-14: "This sentence is not clear, is the "respectively" for surface and depth integrated or for primary and bacterial production?"

We modified the sentence as follows:

Page 14753 (line 12): "It contributed to ca. two-thirds and one-third of the simulated surface (0–10 m) and depth-integrated, respectively, primary and bacterial production."

2.Introduction

"The authors cite different study for changes in the Arctic. Some of these studies report results for the Arctic periphery, the central Arctic, the eastern Arctic, etc., which are governed by different processes but in the intro they are all discussed as the AO in general. The distinctions should be made as it brings some contradictions in the text."

We accounted for this comment and gave precisions wherever possible.

Lines 2-3: "I assume you mean earlier light exposure, rather than greater? Also, could the authors explain why an increase in stratification would promote earlier spring blooms in the Arctic?"

We improved the understanding of this sentence and modified the sentence as follows:

Page 14754 (line 2): "In the Hudson Bay, Foxe Basin, Baffin Sea, off the coasts of Greenland, in the Kara Sea and around Novaya Zemlya, earlier blooms are observed in response to earlier light exposure caused by sea ice retreat (Kahru et al., 2011). In some areas of the Beaufort Sea, the stronger haline stratification recently observed mediates the growing contribution of small phytoplankton cells to the planktonic community in summer (Li et al., 2009) suggesting oligotrophy is expanding in this part of the AO."

Line 6: "40% widening: could you precise in what region, that is certainly not everywhere."

We modified the sentence as follows:

Page 14754 (line 17): "In the Barents Sea, the 40% projected widening of the productive time period will probably allow heterotrophic organisms to optimize grazing through growth and reproduction on phytoplankton, and hence alter the carbon quality and quantity exported to the benthic realm (Wassmann and Reigstad, 2011)."

Line 8: "Will grazing and export really be optimized with the greater occurrence of smaller phytoplankton cells (as you mentioned above, Li et al.)?"

The two sentences have been clarified above in order to prevent any misunderstanding.

Line 21: "most oligotrophic water over what region?"

Page 14754 (line 9): "In this context of accelerating Arctic sea ice decline, a better knowledge of the mechanistic processes and biogenic fluxes mediating PP is required, with a particular attention to the oligotrophic season when biogenic fluxes are complex and so far are poorly quantified."

Paragraph starting at line 25: "I do not agree with the authors. An improved physics in

the coupled models is the most important factor to obtain realistic plankton dynamics and production rates (e.g. circulation, mixed layer depth, freshwater balance, sea ice concentration and thickness, snow depth, light transfer through snow and ice, etc, to obtain the appropriate amount of light and nutrient, see Popova et al., 2012, JGR, 117, C00D12, doi:10.1029/2011JC007112. A literature review on AO models is missing here. Also, you should explain why improving the representation of turnover rates in detail, rather than a more simple parameterization would be so important since for this low production period? How your treatment of light related parameters differ from other AO models?"

We agree that a good physics required in coupled models to simulate "realistic" biogeochemical fluxes is a rule for any oceanic system (e.g. Le Fouest et al., 2006). In this sentence, we refer to the ecosystem models themselves and not to coupled physical-biogeochemical models. In summer, within the upper mixed layer, nutrients are principally issued from the remineralization of freshly produced organic matter. Hence biogeochemical equations driving the simulated elemental fluxes between the ecosystem compartments also play a pivotal role. In order to substantiate our statement, we modified the sentence as follows:

Page 14754 (line 9): "The ability of coupled physical-biogeochemical models applied to the AO to simulate realistic plankton dynamics and production rates relies on both the simulated physics (e.g. Popova et al., 2012) and simulated elemental biogeochemical fluxes (e.g. Le Fouest et al., 2011). In summer, nutrients within the upper mixed layer are mostly issued from the remineralization of freshly produced organic matter. Hence biogeochemical equations driving the simulated elemental fluxes between the ecosystem compartments play a pivotal role. The representation in models of key biogeochemical processes and their comparison with measurements is generally limited in the AO by the lack of joint multiparametric measurements, especially nutrients turnover rates and light-related parameters."

3.Observations

"Where were these observations taken?"

Page 14755 (line 17): "Measurements were taken in the Beaufort Sea at the continental edge slope and ice-edge station 345 (71.33°N, 132.56°W) sampled on 14–16 August, 2009 (Fig. 1)."

"Also on Figure 1, modelling site is hardly visible."

Figure 1 will be improved accordingly.

"It should be mentioned clearly that the model was implemented offshelf in the Beaufort Sea as it has implications when comparing with shelf areas or inflow regions such as the Chukchi or Barents Seas."

This is now mentioned on page 14755 (line 17) and 14756 (line 17).

4.Model

"How is DONp determined? I am a bit confused with the different types, DON, DONp, DONl. Could you specify the simulation period."

As mentioned in page 14758 (line 10), "DONp in the model results from the vertical interpolation of DON concentrations measured on 15 August." We also define DONl as labile DON produced by the plankton ecosystem in page 14757 (line 10): "Detrital (i.e. produced by the ecosystem model compartments) particulate and dissolved organic nitrogen (PON and DONl, respectively) close the nitrogen cycle.".

We precise the simulation period in page 14759 (line 3) as follows:

"The coupled model was run for the 14-15 August 2009 period in steady-state mode so that the diffused state variables reached a near equilibrium state (Fig. 4) ("standard" run)."

5.Results

"It would be more instructive to show SZ and detrital PON rather than total PON. From

the results (high bact, high SP, high PP even though low LP) SZ appears to be too low."

We agree with this statement. However, we showed total PON since it was also measured at the station. This was not the case, unfortunately, for SZ.

Line 23 of page 14759: "I do not think the note is appropriate since the interest is to see if the model, forced with observations, reproduces observations, which is obviously not the case. The reasons for that should be stated here, i.e., why is the so much simulated NH4? If SZ is indeed too low, NH4 regeneration should be lower since it is such an important contributor. Is basal mortality too high?"

We agree with this statement. The sentence has been removed. We explain the model-data discrepancy on ammonium as follows:

Page 14759 (line 23): "The omission of lateral advection, which can be significant in late summer near the slope of the Mackenzie plateau (Griffith et al., 2012), and overestimated biological sources of ammonium from the ecosystem model likely explain the high simulated ammonium concentrations relative to measurements within the DCM."

Line 23 of page 14763: "The sentence should say for the run NOT ACCOUNTING for photoammonification, as stated in the paragraph above and in the legend of Figures 7 and 8. So the results are actually better without this process. The rest of the paper should be modified accordingly."

As a result of a typesetting error, the reader should have read "Figs. 5 and 6" instead of "Figs. 7 and 8". We modified the sentence as follows:

Page 14753 (line 23): "A closer match with surface observations was achieved in the run accounting for the photochemical process (Figs. 5 and 6)."

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/9/C8435/2013/bgd-9-C8435-2013-supplement.pdf

Interactive comment on Biogeosciences Discuss., 9, 14751, 2012.