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Biogeosciences Discussions

Interactive comment on "Nutritive and photosynthetic ecology of subsurface chlorophyll maxima in the Canadian Arctic waters" *by* J. Martin et al.

J. Martin et al.

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We thank referee #1 for critical comments that helped clarify some sections of the text (Introduction and Discussion), emphasize what is original in this work and better explain the choice of methods and approaches.

1) Comment: *This manuscript assumes that subsurface chlorophyll maxima (SCM) in the Canadian Arctic waters have a large role in new production in the water columns, and reports nutritive and photosynthetic characteristics of the SCM. However, vertical maximum of nitrate uptake rate does not occur at SCM in oligotrophic tropical and*

subtropical oceans (e.g. Raimbault et al., 1999; Aufdenkampe et al., 2002; Kanda, 2008).

Response: In fact the introduction hypothesizes but does not "assume" that SCM have a large role in new water-column primary production. This hypothesis was formulated on the basis of recent observations published in Martin et al. 2010. These authors characterized Arctic SCM and compared them with those of warmer oceans. The comparison revealed striking differences between these oceans and the Arctic. In the latter, SCM are located relatively high in the euphotic zone, occur at or near the nitracline, and correspond to maxima of phytoplankton biomass (whereas in tropical oceans the SCM is at the bottom or below the euphotic zone, above the nitracline, and does not correspond to a maximum of phytoplankton biomass). For these reasons one cannot take for granted that the SCM and the vertical maximum of nitrate uptake do not match in the Arctic Ocean.

2) Comments: *The authors determined biological productivity only at the SCM and surface waters, but information of vertical profile of the biological productivity is lacking. Hence I cannot evaluate the validity of their assumption and judge their results properly. Accordingly, I am afraid the conclusions the authors reached are not solid. . . The authors estimate vertical profile of primary production and nitrate uptake on the basis of the results of uptake-irradiance parameters in the SCM and surface waters. Verifying these estimated profiles, the authors should show observed vertical profile of primary production and nitrate uptake at some stations and compare the modeled uptake rates with observed ones.*

Response: One of the most original aspects of our work is the use of comprehensive light-gradient incubations with 2 samples (one representative of the upper mixed layer and the other from the SCM) instead of static 24-hr incubations of 6-7 discrete

samples (on deck or in situ) as previously done by others in the Arctic (e.g. Simpson et al. in prep; Brugel et al. 2009; Fouilland et al. 2007; Smith et Harrison 1991; Harrison et al. 1982; Platt et al. 1982) and elsewhere. We recognize that our approach was not fully explained in the introduction. Firstly, it should be noted that the 24-hr incubations advocated by the referee are not a "gold standard". These 24-hr incubations are subject to artefacts (e.g. unrealistic photoinhibition of surface samples, nutrient depletion) and bottle effects. Secondly, sampling at 6-7 depths provides better vertical coverage but it still is a discrete approach that poorly resolves the structure of SCM layers. Thirdly, light-gradient incubations are the only means to separate the light-dependent and light-independent components of nitrogen uptake (see reply 11). Finally, the results of 24-hr incubations provide "biogeochemical" estimates of primary production at a given station, but these cannot be extrapolated beyond the day of sampling since incident irradiance in the Arctic fluctuates greatly at short time scales.

Using a dynamic approach yielding parameters that assess physiological state, acclimation and responses to irradiance and nutrient additions seemed to be the most promising means to understand the ecology SCM communities, assess their productivity and generate parameters for remote-sensing approaches and numerical models. With these parameters it is possible to "reconstruct" primary production by combining continuous calibrated fluorescence profiles (thus resolving the SCM without the aliasing caused by discrete sampling) with a continuous record of incident irradiance and light attenuation in the water column.

For the reasons exposed above we believe that a direct comparison of our productivity estimates with discrete 24-hr incubations is of limited value. We attempted to do it nevertheless, but realized that most of the papers mentioned above reported vertically-integrated rates only but not the vertical profiles. The only profiles shown by Harrison et al. 1982 for open waters of the Canadian Arctic indeed show a pronounced

subsurface maximum of primary production and nutrient uptake (see their Fig. 2, stn. 93). We also found the following statement in Tremblay et al. 2006 (LO: 51), who used 24-hr incubations at 7 depth for Baffin Bay: *"Moderate RPINO3 , VNO3 and f-ratios in the nitracline were associated with a persistent layer of the centric diatom Chaetoceros socialis, which drove most of the new production after the early (surface) bloom".*

3) Comment: *Furthermore, SCM has been well studied in the ocean, and light, nutrients, and temperature are well recognized to be important for the SCM communities (Lalli and Parsons, 1993). I receive the impression that their finding is lacking novelty and remains local interest.*

Response: Indeed we recognize that light, nutrients and temperature are important for the phytoplankton and were studied numerous times in the world ocean. However we disagree that SCM have been "well" studied from a global perspective. Lalli and Parsons 1993 did not consider the Arctic at all. Most of the available knowledge is derived from temperate or warm oceans and the paper of Martin et al. (2010) and the present one actually demonstrate that Arctic SCM are unique and do not function like tropical or subtropical ones. That's novelty! We also believe that investigating the ecology of a 2500 x 3000 km portion of the Arctic Ocean, the most understudied ocean on Earth and yet the most perturbed by climate change, is not a matter of "local" interest. We have slightly modified the conclusion (or discussion) to better place the results in a larger context.

4) Comment: *Introduction One of the objectives in this study is to establish contemporary parameters for use in ecosystem models and remote-sensing algorithms (P6448 L9-11). However, by only reading this introduction, I cannot understand the necessary for obtaining the parameters. Therefore it is difficult to understand why the authors needed to conduct experiments for photosynthesis or nitrogen uptake–irradiance*

curve (P6451 L7- P6452 L5).

Response: See answer #2 above.

Materials and Methods

5) Comment: *P6450 L18: Information of vacuum pressure is lacking.*

Response: The pressure (< 250 mmHg) is now provided in the text.

6) Comment:*P6452: What is N2?*

Response: The text now specifies that N2 is the Brunt-Väisälä (or buoyancy) frequency.

7) Comment: *P6452 L6: Recent study demonstrated that f-ratio has been overestimated by nitrification near surface (Yool et al., 2007). Influence of nitrification on nitrate uptake and f-ratio should be addressed.*

Response: We agree that nitrification may introduce a bias in the estimation of the *f*-ratio (as stated by Raimbault et al. 1999 and Yool et al. 2007) and have added a caveat to the text. Since bacterial nitrification is inhibited by light (Horrigan et al. 1981), Martin et al. (2010) surmised that nitrification should play a lesser role for Arctic SCM located relatively high in the euphotic zone (which receives light 24-hr per day). These SCM also track the nitracline, where ambient nitrate concentrations are relatively high and must strongly dilute small inputs by nitrifyers.

Results

8) Comment: This comment has been merged with comment #2.

9) Comment: *Table 1: Incubations of surface waters were only conducted in late summer-fall. The authors demonstrate that plankton community properties did not only vary between at the surface and the SCM (Fig. 2) but also with the seasons (Fig. 5 and 6). This sampling bias might influence on interpretation of obtained data.*

Response: We agree that our representation of vertical phytoplankton structure is partial due to the lack data for surface waters in the spring. On the other hand, as mentioned P6467 L1-6, Palmer et al. (2011) observed a continuous and rapid acclimation (within 4 to 10 days) of the phytoplankton during the initiation of the growth season. While acknowledging the limitation that this lack of data can provide, we assume nevertheless that this observation may be appropriate for a large part of the production season.

10) Comment: *Table B1,B2: It is difficult to understand the difference between Table B1 and B2. What is the difference of correlation coefficient for nitrate uptake between Table B1 and B2?*

Response: Table B1 and B2 represent statistical analysis on two different data sets. As mentioned in Materiel and Methods (P6449 L2-5) and in the result section (P6454 L13 and P6455 L20), NO3/NH4 experiment at SCM depth represent one data set (Table B1) and surface/SCM experiments another one (Table B2).

11) Comment: *P6460 L11-19: The authors hypothesized that the primary productivity of SCM communities was limited by irradiance due to their position in the lower euphotic zone near the nitracline and that SCM depend principally on NO3 and mediate a large share of water-column new production. To examine these hypotheses, why the authors need to discuss (1) the relative importance of dark versus light dependent uptake for different N sources and (2) stoichiometry of C and N uptake at low irradiance? There seems to be a jump in logic.*

Response: This paper's objective is to evaluate carbon fixation and nitrogen utilization by photosynthetic primary producers. To do this it is necessary to consider the degree of coupling between the two processes at the SCM. The reason is that C fixation and N uptake may not be entirely coupled under low light conditions (P6447 L12-19) since a portion of the N uptake may be independent of light energy (uptake by bacteria or storage in diatom vacuoles). Light-independent NO3 uptake cannot be assumed to support new, photosynthetic primary production unless we assess/discuss N uptake by bacteria with respect to the light-independent portion of uptake (D) and C:N uptake ratios. This is another justification for using light-gradient incubations, which allow separate the light-independent from the light-driven portion of uptake (24-hr incubations cannot do that). We modified the explanations already given in the introduction and the discussion in order to clarify the rationale.

Aufdenkampe, A.K. et al. (2002) Biogeochemical controls on new production in the tropical Pacific, Deep-Sea Res. II 49, 2619-2648.

Kanda, J. (2008) Vertical profiles of nitrate uptake obtained from in situ 15N incubation experiments in the western North Pacific, J. Mar. Syst. 71, 63-78.

Lalli, C.M., Parsons, T.R. (1993) Biological Oceanography -an introduction, Elsevier.

Raimbault, P. et al. (1999) Carbon and nitrogen uptake and export in the equatorial Pacific at 150° W: Evidence of an efficient regenerated production cycle, J. Geophys. Res. 104, 3341-3356.

Yool, A. et al. (2007) The significance of nitrification for oceanic new production, Nature 447, 999-1002.

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