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

**Anonymous Referee #3**

Received and published: 8 August 2012

General Comments: The manuscript presents an extensive analysis of  $^{13}\text{C}$ ,  $\text{NO}_3$  and  $\text{NH}_4$  -uptake versus irradiance experiments for the Arctic subsurface chlorophyll maxima (SCM).  $\text{NO}_3$  and  $\text{NH}_4$  – uptake versus irradiance experiments are rarely conducted alongside traditional “P vs E” curves, so the paper includes some novel information on the rates of  $\text{NO}_3$  and  $\text{NH}_4$  uptake compared to carbon fixation for a range of irradiances and locations in the Canadian Arctic. Some observations, such as variability in the f-ratio with light and nitrogen availability, have wider implications for ocean biogeochemistry, but on the whole the paper is of local interest. My main concern is that there is no mention of a spectral correction to account for spectral difference between experimental and in situ light fields, or to account for variability in the absorption spectra of

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phytoplankton. Large errors will likely be present in a significant amount of the analysis presented in the paper if a spectral correction was not performed. I have given more detail on this below.

#### Specific Comments:

A spectral correction ought to be performed in order to account for spectral difference between experimental and in situ light fields, as well as for variability in the absorption spectra of phytoplankton. The importance of spectral corrections is well documented (Babin et al. 1993; Morel et al. 1996; Sugget et al. 2001) and relevant for the Arctic (Shakshaug and Slagstad 1991; Brunelle et al. 2012). With relevance to the current paper, the spectral correction is important, at least in my view, for the following reasons: 1) The initial slope ( $\alpha$ ), light saturation parameter ( $E_k$ ) are spectrally dependent - their magnitudes are experiment-specific (relative) unless the experimental light spectra is accounted for, 2) Large and variable differences between the spectra of the experimental lamps and insitu light field are likely (particularly in coastal, case II, regions) so that experimental parameters cannot be accurately used to estimate in situ primary production without a spectral correction. 3) Phytoplankton light absorption varies with species composition and with depth, contributing significant variability in light-response curve parameters. Unfortunately, these issues are not acknowledged in the paper and, if a correction was not performed, I cannot see how any estimates made by combining experimental parameters and insitu irradiance can be valid. Similarly, if spectra differed between experimental light sources, then parameters obtained from different experiments cannot be directly compared without a correction (e.g. such as comparison of  $\text{NO}_3$ ,  $\text{NH}_4$  and  $\text{C}^{13}$  response curves to estimate the f-ratio). I feel this is a fundamental problem that needs to be addressed as it could potentially undermine much of the data presented in the paper.

Pg 6447 Ln 11: You could also reference Taylor et al. 1986.

Pg 6450 Ln 26: Change "Detail" to "Detailed"

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Pg 6450 Ln 26 – Pg 6451 Ln 1-2: “The depths (Z) of the SCM, pycnocline and nitracline were identified as those where the vertical gradients of in vivo fluorescence, N<sub>2</sub> and NO<sub>3</sub><sup>-</sup> had the highest values, respectively.” Is the maximum gradient in chlorophyll concentration a reliable method for identifying the SCM? Why not use the depth of maximum in vivo fluorescence? Using this criterion, it seems possible that the maximum gradient in Chl-a could be either above or below the actual Chl-a maxima, thus unpredictably over- or under- estimating ZSCM for different profiles?

Pg 6451 Ln 6: “Daily-averaged irradiance at the SCM ( $E_{\text{SCM}}$ ) and a continuous record of incident PAR above the sea surface (Kipp & Konen; PAR-Lite) to estimate  $E_0$ .” Was any correction made for the transmittance of irradiance through the sea surface? If not, in situ irradiance could be quite significantly overestimated due to low solar angles and high surface reflectance in the polar regions (e.g. see Kirk 1994, Sakshaug & Slagstad 1991).

Was there any evidence of carbon fixation in the dark (as would be expected for the <sup>14</sup>C radioisotope method)? If so, how was it accounted for? Since the dark uptake of NO<sub>3</sub> and NH<sub>4</sub> is discussed at length, it would be helpful to mention dark carbon fixation as well.

It would be helpful to mention whether or not the experiments were conducted at the same time each day. Was there evidence of diel variability in photophysiological parameters?

Pg 6452 Ln 2-5. It is unclear what these standard errors represent? Are they errors or variability in the data?

Please use different symbols for the light saturation parameter and initial slope of the NO<sub>3</sub> and NH<sub>4</sub> –uptake vs. irradiance parameters.  $E_k$  and  $\alpha$  are accepted photosynthesis – irradiance parameters, but in this manuscript they have multiple meanings. If there are no standard symbols for “ $E_k$ ” and “ $\alpha$ ” for NO<sub>3</sub><sup>-</sup> and NH<sub>4</sub> –uptake versus irradiance curves, I suggest simply using subscripts,  $E_{k\text{NO}_3}$ ,  $\alpha_{\text{NO}_3}$ ,  $E_{k\text{NH}_4}$ ,

alphaNH<sub>4</sub> to make things clearer. I would also suggest doing this to distinguish DBNO<sub>3</sub> and DBNH<sub>4</sub>.

Section 3.1 General Results in the sampling area. More information on the water column structure in the region would be very helpful here. What is the typical water depth? Is the water column stratified all year? The authors reference Martin et al. 2010 for details, but I feel information in the current manuscript is needed for the reader to put the SCM into context. A figure showing density, Chl-a, NO<sub>3</sub>, NH<sub>4</sub> profiles for a representative station would help greatly.

Pg 6453 Ln 15-18. "... and their vertical positions were significantly correlated with the SCM ( $ZPNM = 0.50 \times ZSCM + 39.23$ ,  $r^2 = 0.12$ ,  $n=201$ ;  $ZPAmM = 0.72 \times ZSCM + 25.75$ ,  $r^2 = 0.20$ ,  $n=96$ )." What is the significance level of these relationships? The  $r^2$  values are very low, with ZSCM only representing < 20% of the variability in ZPNM and ZPAmM. To me, these do not seem like strong relationships?

Pg 6455 Ln 2-5: "On order to assess the contribution of the SCM layer to daily primary production and NO<sub>3</sub>- uptake during 2006, we combined uptake-irradiance parameters with measurements of daily mean irradiance and detailed vertical profiles of Chl-a. ... " Some additional information on how the calculations described in this section were done would be helpful. I have a number specific questions: 1) what were the criteria for defining the surface layer and SCM (how was the 'top of the pycnocline and top of SCM defined?')? 2) Given the non-linear nature of the <sup>13</sup>C and NO<sub>3</sub> -uptake vs irradiance curves, an estimate of daily uptake based on the mean daily irradiance will not be the same as an estimate based on the integrated daily irradiance (i.e. that accounts for the change in irradiance during the day). Is the assumption of mean irradiance likely to lead to a large error? 4) The authors assuming NO<sub>3</sub>- uptake parameters measured on the SCM sample are representative of the region from the top of the SCM to the base of the pycnocline. This assumption consequently assumes no change in the NO<sub>3</sub>-uptake parameters across the nitracline (since the nitracline and SCM are coupled). This assumption is, presumably, backed up by the observation that the parameters

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were not altered by NO<sub>3</sub>- additions. However, this is an important assumption and I feel it would be helpful to make it clear to the reader.

Pg 6456 Ln 2: Change “station” to “stations”.

Pg 6456 Ln 13: Change “was” to “were”.

Pg 6457 Ln 7-10: “. . . a detailed analysis of PBm versus temperature (T) showed a significant, positive linear relationship during late summer-fall (Fig 5. . .).” It looks to me that only the surface (solid triangles) samples show a linear relationship, the values from the SCM (solid circles) do not appear to not conform to this statement.

Pg 6460 Ln 26: “. . . because it can be mediated by heterotrophic bacteria and the portion taken up by phytoplankton is not necessarily constitutive (i.e. not assimilated or, more precisely, not leading to amino acid synthesis) since photosynthesis does not occur in the dark (e.g. N may be stored in cell vacuoles).” Please include references for these statements.

Pg 6464 Ln 18: “. . .because the strong SCM there abolished the separation that different Ek values would otherwise impart.” The wording could do with some work here.

Pg 6464 Ln 21-26: “In the reconstructed profiles (Fig. 3) the depth of maximum productivity occurred at the SCM and the ‘classical’ decrease in primary production with depth was not observed. . .”. Please make clear that the water-column estimates were made only at a minority of stations during Sept-Nov 2006 - they are not representative of the whole region or of changes over time. It would be helpful to identify stations where profiles were estimated on Fig 1.

Pg 6466 Ln 20-22: “When considering spring-early summer only, PBm can be approximated as a function of day of the year and temperature (e.g.  $PBm = 8.417 - 0.0229 DY + 2.742 T$ ). Otherwise PBm can be estimated as a function of temperature only (e.g.  $PBm = 0.178 T + 0.538$ ; Fig 5).” Please include statistics for these relationships so the reader has a clear idea of how much of the variability in PBm is described by

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T and DY. There is rather a lot of unexplained variability in the data (for these relationships as well as others in the manuscript) that is not currently acknowledged. It seems rather misleading particularly when promoting these relationships for remote sensing or ecosystem modeling applications.

Table 1: Please define 'n/d' in the caption. Also, please describe how the correction for ice cover was performed and specify what the Mean and SD at the bottom of the two parts of the table represent.

Additional references quoted above:

Taylor AH, JRW Harris, J Aiken (1986) The interaction of physical and biological processes in a model of the vertical distribution of phytoplankton under stratification. *Elsev Oceanogr Ser* 42: 313-330.

Babin M, JC Therriault, L Legendre, A Condal (1993) Variations in the specific absorption coefficient for natural assemblages: Impacts on estimates of primary production. *Limnology and Oceanography*, 38:154-177.

Brunelle CB, P Larouche, M Gosselin (2012) Variability of phytoplankton light absorption in Canadian Arctic seas. *Journal of Geophysical Research*. 117:17

Kirk JTO (1994) *Light and photosynthesis in aquatic ecosystems*. Cambridge University Press.

Morel A, D Antoine, A Babin, Y Dandonneau (1996) Measured and modeled primary production in the northeast Atlantic (EMULI JGOFS program): the impact of natural variations in photosynthetic parameters on model predictive skill. *Deep-Sea Research* 43:1273-1304.

Sakshaug E, D Slagstad (1991) Light and productivity of phytoplankton in polar marine ecosystems: a physiological view. Pp. 6S-85 in E Sakshaug, CCE Hopkins & NA Britsland (eds.): *Proceedings of the Pro Mare Symposium on Polar Marine Ecology*. Trondheim. 12-16 May 1990. *folur Research* 10(1).

Sugget D, G Kraay, P Holligan, M Davey, J Aiken, R Geider (2001) Assessment of photosynthesis in a spring cyanobacterial bloom by use of a Fast Repitition Rate Fluorometer. *Limnology and Oceanography* 46:802-810.

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