



# ***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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General comments

**1) Comment:** *Coastal polar areas can hold large proportions of ocean productivity. In this sense, this is an important work that sheds light on production at the SCM in a large area in the Canadian Arctic. It would be interesting to calculate the proportion of the Arctic Ocean that was covered by this study and how much SCM production in these areas contributes to global estimates; this would be another valuable contribution. In addition, adequate parameterisation is central in biogeochemical*

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modelling and is usually a major gap, so that information on accurately measured (and also N-based, which is the currency of many models) new and regenerated primary production parameters is an additional value of this work.

BGD

9, C3167–C3177, 2012

**Response:** We would like to thank referee #2 for insightful comments throughout the review. We estimate that the Canadian Arctic represents a substantial portion (ca. 27%) of the Arctic Ocean and modified the introduction to point this out. The goals of our paper were to generate fundamental knowledge on SCM function and to provide parameters to refine large-scale estimations of primary production - but performing these estimations is another work altogether. Although the data presented and the methodology used here do not allow to assess the areal contribution of "Canadian" SCM to total Arctic production, we are confident that a 3-D numerical approach (e.g. N-based model) would be able to do so in the future.

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**2) Comment:** *I think that authors' focus in the introduction and part of the paper on the need to correct satellite algorithms should not be a priority. Arrigo et al (2011) state: "As a result, the combined effect of underestimates in NPP due to omission of the SCM and overestimates in NPP due to high satellite Chl a yields a total error in annual pan-Arctic depth-integrated NPP of <1%." If in the present work authors conclude that for their study area the omission of SCM in the estimation of NPP has a greater impact than the average 8% these authors estimate for SCM omission, this has to be discussed in more detail. In addition, sampling stations in the present work are mainly coastal, which poses extra problems for the satellite estimation of phytoplankton biomass (and production).*

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estimations of coastal NPP introduces other biases (high chl a), but we believe that resolving the regional contribution of SCM to productivity in regions that become more stratified over time is an important step forward that should not be dismissed because other sources of error tend to cancel out in remote-sensing approaches. The point made by the referee is very valid, however, and we have modified the text accordingly.

**BGD**

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## Specific comments

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## Abstract

**3) Comment:** *The authors state that SCM contribute largely to “total water column” production; the depth for which they have estimated production is only given for one station (and it is 71 m. The depth of the water column at the sampling stations is not indicated in the paper.*

**Response:** Actually, 71 m is not the depth of the water column for station 303 but the depth of the euphotic zone. Nevertheless, your comment made us realize that the bathymetry is missing in Fig.1. We added it to the figure.

## Introduction

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**4) Comment:** *Temperature is mentioned in the abstract, results and discussion but does not appear in the introduction or as a question for the work.*

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**Response:** Indeed. We apologize for the oversight and added a presentation of temperature effects to the introduction.

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**5) Comment:** *Sampling: since assumptions are made for the whole water column, the “standard depths” should be explicitly mentioned here (P 6448, L 26).*

**Response:** The depths are 5, 10, 20, 30, 40, 50, 60, 70, 80, 100, 125, 150, 175, 200, 250, 300 m and then every 100 m unless the Arctic halocline was identified. In this case, sampling in the 100–200 m range occurred at every 20 m and at a salinity of 33.1 to capture the nutrient maximum. We added this information to the Sampling section.

**6) Comment:** *The 2.1 “Sampling” section could be combined with some of the analyses that are standard procedures described in the following sections. For example, combine the section on nutrients in paragraph 2.1 with 2.2, and Chl-a and Fv/Fm in 2.1 with 2.3.*

**Response:** Done.

**7) Comment:** *Section 2.5 is entitled “sensor calibrations” but only the reference to Martin et al. 2010 is mentioned. “Calibration” should be omitted from the title. Z of SCM and of the nitracline are not really “transformations” either, and could be added in the Chl-a and nutrients paragraphs, respectively. If N2 is Brunt-Väisälä frequency, this should be stated.*

**Response:** We changed the title of section 2.5 to remove the mention of calibrations.

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We included the Brunt-Väisälä frequency.

BGD

**8) Comment:** *Some of N-uptake parameters present pretty high errors (such as DB, Page 6452, L.4). The errors presented correspond to which of the two methods used to calculate it?*

**Response:** The error on irradiance-uptake parameters is the standard deviation estimated by SigmaPlot for the fitting of the empirical exponential models (Platt et al. 1980 or Webb et al. 1974).

**9) Comment:** *Indicate the software used for statistical analyses.*

**Response:** We used SigmaPlot 11. Added to the text.

Results

**10) Comment:** *3.1. Are the “experimental stations” different in any aspect from the other stations? Why is ZSCM and the relation between ZSCM and Znitracline presented separately for them? Is the error due to subsampling larger than expected? If there is no difference among experimental and non-experimental stations, adding this extra information is not necessary.*

**Response:** Our intent was to demonstrate that subsampling was representative of whole sampling domain. We clarified this in section 3.1.

**11) Comment:** *In P 6454, L. 6-9 NO<sub>2</sub> and urea values are repeated from the table.*

Omit them from the text, which has already a huge amount of numbers and renders reading hard.

BGD

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**Response:** Removed.

**12) Comment:** 3.2 In P 6454, L. 19, 21, 26 and elsewhere: when  $p$  is significant, just indicate  $p < 0.05$  or  $p < 0.01$ . Indicate the exact value when it is not (i.e, P.6458, L22).

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**Response:** Done.

**13) Comment:** I don't see the use of Fig. 2; the significant differences in alfa and Ek (or the lack of them for PBm) were already mentioned in the text.

**Response:** We believe that it is important to visually provide the data distribution of surface and SCM photosynthetic parameters in addition of significance difference in order to ease to comprehension of data summarized in the Result section.

**14) Comment:** Simultaneous surface and SCM production measurements are used to estimate integrated production based on light and Chl-a profiles (again, information on the depth of the samplings stations would be important here), and so estimate the relative importance of the SCM during late summer-fall. Although average values are presented and station 303 is depicted in the figure, since this is central to the hypotheses drawn in the paper it should be presented in more detail. A table with estimates (C and N based) for surface, SCM, and integrated (and maybe the % SCM represents) would be important. This would additionally allow excluding some numbers from the text.

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**Response:** We agree that SCM contribution is an important issue, but the main objective of the present manuscript is to evaluate carbon fixation and nitrogen utilization by photosynthetic primary producers at SCM depth. Here, this was not the central aim to attribute the contribution of the SCM. As mentioned in the comments #1 and #18, the method used in this study can induce some bias. That is why we chose not to push further this analysis when we initially wrote the discussion. These results will be discussed in a forthcoming manuscript using a numerical approach in order to assess SCM contribution.

**15) Comment:** *3.5: Is the correlation significant for SCM data only (and without station NR24)? There is a parenthesis missing in L 24, after ESCM.*

**Response:** Yes, the correlation stands for SCM data only. We added this information and the missing parenthesis to the text.

**16) Comment:** *In fig. 6, grey and white circles are hard to distinguish.*

**Response:** Changed.

**17) Comment:** *P. 6458, L. 7. What is the “station-specific” decrease?*

**Response:** We meant the decrease of the mean f-ratio over the experimental range of irradiances at a given station. Changed.

Discussion

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While reading the article, some questions arose which I did not see answered in the discussion. Authors may consider including them in their revised document:

**18) Comment:** - *Simultaneous surface and SCM production measurements were only performed in fall 2006 (9 samples). How can integration in the whole water column in this season be generalised to other seasons?*

**Response:** We agree that our assessment of the SCM contribution to water-column primary production is only partial due to the lack of surface experiments in the spring. This is why we refer to Palmer et al. (2011) on P6467 L1-6, who observed a continuous and rapid acclimation (within 4 to 10 days) of the phytoplankton during the initiation of the growth season and vertical patterns similar to ours thereafter. 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.

**19) Comment:** - *What is the hypothesis laying behind DY? How could DY affect phytoplankton (other than by light, which did not show correlation with data)?*

**Response:** There was no a priori hypothesis relating to day of the year (DY), but it turned out as an important factor in statistical analysis. We think that DY integrates a complex set of interaction between environmental parameters (e.g., surface irradiance, SCM depth, nutrients availability, stratification). We reworked the text to discuss this issue.

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**20) Comment:** -Was species composition during the different years/seasons/areas analysed? Are the phytoplankton assemblages comparable?

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**Response:** The species composition for 2005 and 2006 is presented in Martin et al. 2010. During late-summer and fall, communities were mainly composed of flagellates with an important contribution of diatoms. In fall 2007, Ardyna et al. (2011) observed similar pattern. Due to their large size (cells > 20 um; Lee Whittlesey 2005, Tremblay et al. 2009) compared to other taxa and because SCM communities also thrive high in the euphotic zone (compared to other ocean; e.g. oligotrophic gyre) diatoms are expected to contribute significantly to SCM productivity. To our knowledge, no taxonomic counts are available for our sampling zone in spring 2008. Nevertheless, Palmer et al. (2011) observed diatoms dominance at SCM under ice with pigment analysis.

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**21) Comment:** - Why was photoinhibition observed at the SCM but not in surface?

**Response:** This phenomenon suggests that phytoplankton at SCM depth are acclimated to low light, while phytoplankton near the surface are able to cope with much higher irradiances. Similar photoinhibition patterns were observed from P-E curves in stratified waters of Baffin Bay and Lancaster Sound (Platt et al. 1982, Gallegos et al. 1983) performed at 50% and 1% light level of surface irradiance where surface incubations showed modest to no-photoinhibition while deeper incubations demonstrated a photoinhibition about 7 times higher. We clarified that in section 4.2 of the Discussion.

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**Response:** Indeed this is what we meant. As mentioned earlier in section 4.1, non-constitutive uptake does not lead to rapid amino acid synthesis (e.g. N storage, incomplete reduction). We clarified the text of this section.

**23) Comment:** *Dark vs light dependent N uptake appears then mostly as a correction factor in the estimations of N uptake due to non-algal assimilation. But then, I don't understand what you mean when explaining this in P. 6461, L. 15-18: DB algae assimilation requires active growing phytoplankton, which all along your paper you showed was the case at the SCM, but now you say that this is unlikely to occur under the limiting light conditions there! If they are actively photosynthesizing, why couldn't they use that C to take up NO<sub>3</sub> in the dark? How can light limitation for phytoplankton production in the SCM be assessed through this (P. 6460, L., 15) anyway?*

**Response:** We meant that DB during incubation presupposes the prior accumulation of energy under conditions where irradiance exceeds the immediate needs of phytoplankton. This is unlikely to occur in algae sampled at the SCM even if these are actively growing.

**24) Comment:** *The last sentence (L. 16-21 in P. 6462) corresponds to the classical succession patterns.*

**Response:** We agree. This corresponds to the classical succession pattern. Nevertheless, we were expecting a larger contribution of nitrate to total uptake (f-ratio) at the SCM due to its association with the nitrcline.

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**25) Comment:** 4.2. L. 23: *SCM communities located within or below the halocline. . .? The following sentence could be rewritten in less tortuous way.*

**BGD**

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**Response:** We meant “in strongly-stratified portions of the water column”. We changed this sentence.

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**26) Comment:** *P. 6464, L.25-27. As mentioned above, Arrigo et al. 2011 estimate that omission of SCM production would lead to an error of 8% which compensates somehow with overestimation of Chl-a in other areas.*

**Response:** See comment #2.

**27) Comment:** *4.4 This section contains valuable information that gets lost the way it is presented. A table indicating which relation authors suggest for each case or set of conditions would render it more useful for readers searching for parameters for modelling, for instance.*

**Response:** Added.

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Interactive comment on Biogeosciences Discuss., 9, 6445, 2012.

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