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Discussions

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

Anonymous Referee #2

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General comments

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 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.

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).

Specific comments

Abstract

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.

Introduction

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

Methods

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

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.

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.

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?

Indicate the software used for statistical analyses.

Results

3.1. Are the “experimental stations” different in any aspect from the other stations? Why is Z_SCM and the relation between Z_SCM and Z_nitracline 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.

In P 6454, L. 6-9 NO2 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.

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).

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.

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.

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

In fig. 6, grey and white circles are hard to distinguish.

P. 6458, L. 7. What is the “station-specific” decrease?

Discussion

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:

- 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?

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

- Was species composition during the different years/seasons/areas analysed? Are the phytoplankton assemblages comparable?

- Why was photoinhibition observed at the SCM but not in surface?

4.1 This section is not clear to me. L. 13-14: Are you saying that only 39 (or 45)% of DB of NO3 (orNH4) is taken by autotrophs (i.e., that 61% (or 55%) is taken up by heterotrophs)? And this algae uptake is for both assimilation and non-constitutive uptake? (what would non-constitutive uptake be, anyway? Luxury uptake?)

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 NO3 in the dark? How can light limitation for phytoplankton production in the SCM be assessed through this (P. 6460, L., 15) anyway?

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

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

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.

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.

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