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Biogeosciences Discussions

Interactive comment on "Distribution of calcifying and silicifying phytoplankton in relation to environmental and biogeochemical parameters during the late stages of the 2005 North East Atlantic Spring Bloom" *by* K. Leblanc et al.

Anonymous Referee #2

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1 General remarks:

The paper presents observations of late spring bloom conditions in the open ocean Northeast Atlantic from a transect sampled between about 45°N and 67°N in June 2005. The authors bring together a quite extensive dataset of relevance to the understanding of biogeochemical/biological processes in the surface near euphotic zone during that season, including nutrients, phytoplankton pigments, POM, PIC, opal, trace metals, etc. This paper also forms the background paper for experimental work carried

out during the cruise and published elsewhere (f.e. Feng et al., 2009, MEPS).

Based on their data and a 'non-exhaustive synthesis' of pervious studies in the region, the paper by Leblanc et al. intends to answer a set of biogeochemical key questions about this part of the NA and this particular season, respectively:

1) Are the coccolithophore blooms often indicated by the large calcite patches seen in satellite images a major component of the phytoplankton bloom in the NEA?

2) Which environmental factors can best explain the relative dominance of coccolithophores vs. diatoms in this high latitude environment?

3) What causes recurrent silicic acid depletion in the NEA and what are the potential consequences for phytoplankton composition and carbon export?

Combining pigment information which principally can be used to quantify the relative contribution of different phytoplankton groups to total phytoplankton stocks and measures of inorganic components and remains of two important groups (PIC for calcifying coccolithophores, biogenic silicate (BSi) for diatoms) is a particularly interesting aspect of this paper. This has not very often been done before and is highly welcomed. In comparing the pigment data and measurements of PIC and BSi, the authors find significant correlations between BSi and FUCO (fucoxanthin) but usually a poor correlation between PIC and HEX. The former is interpreted as reflecting a close coupling (representation) between FUCO, diatoms and BSi, both in living cells and concerning the fate of remains after cell death. The loose correlation between PIC and HEX on the other hand is discussed in relation to a number of processes like presence of (non-calcifying) Phaeocystis, potential presence of naked coccolithophores, coccolith overproduction and detachment. This is all well presented and discussed in the paper. The lack of correlation between PIC, coccolithophore counts and HEX pigments highlights that the interpretation of remote sensing PIC data with respect to coccolithophore abundance/blooms is a non straight-forward issue.

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Concerning this part of the paper I have two remarks:

a) In the M+M section 2.2.4 the authors present in detail how they compute size class contributions of total chl-a (micro-, nano-, picoplankton) from pigment data. Later the focus, however, is not on size classes (though they are presented in Fig. 8/9) but on distinct groups like diatoms and coccolithophores. Others (f.e. Barlow et al. 1993) have tried to explicitly compute f.e. the diatom fractions from combined pigment data. Perhaps such an approach might have provided better/other correlations as compared with correlations with individual pigments. Please discuss why that approach has not been chosen here.

b) In the discussion you could more explicitly elude on the consequence of the mismatch between PIC and HEX for the interpretation of remote sensing data.

Dealing with **question 2** of the paper's objectives the authors start off with what they call a 'short non-exhaustive synthesis' of spring/summer blooms in the region. In agreement with conventional understanding, their own data, and their review of published work one might summarize the sequence of phytoplankton dominance during spring/summer blooms as 'diatoms first, prymnesiophytes second'. However, there are examples of the opposite, f.e. Smith et al., (1991, Nature, 352, 514ff) observed a situation where the prymnesiophyte Phaeocystis p. dominated the spring bloom basically using up most of the nitrate while silicate values largely stayed unchanged. Forward searching from that paper one might discover more such opposing sequences of phytoplankton blooms? Perhaps providing a more exhaustive review of the literature on relevant studies from the NEA is hence needed in this paper. Otherwise one could argue that the overall findings of this work concerning question 2 are not really novel or unique and that the respective description of more a data report and not a scientific publication.

Finally, the authors try to answer the question **'What causes recurrent silicic acid depletion in the NEA and what are the potential consequences for phytoplankton**

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composition and carbon export?' In doing so, they return to the early work from the JGOFS NABE study, which perhaps for the first time for the open ocean NEA showed that diatoms though being an important component of the spring blooms obviously do not use up all the nutrients, leaving significant N and P resources for other phytoplankton groups to form a second bloom. The partitioning of wintertime accumulated N and P resources between phytoplankton groups with different export potential is clearly an important issue not only in view of potential future changes in stratification and nutrient supply. Nevertheless, I found this section of the paper somewhat weak. F.e. there is no explicit mentioning of pre-bloom nitrate and silicate conditions, though this has been studied in the past, f.e. by Glover and Brewer (1988, DSR), Koeve (2001, Mar. Chem.) and likely others as well. These studies show that pre-bloom waters are relatively poor of silicate, compared with typical N:Si ratios of diatoms (even non-iron limited ones). Moderate iron deficiency in the NEA is mentioned as contribution to Si scarcity via heavily silicified cells in the paper. Generally, as mentioned also by the authors, it's the general circulation, or more explicitly the interplay between general circulation and remineralisation depths of N (P) vs. Si which explains observed nitrate:silicate conditons. However, that is a trivial statement which is true everywhere in the ocean. The particular data presented in this paper, however, do not shed more light on this question (the specific conditions found in the NEA). I suggest to either skip this part completely from the discussion or to provide better justification why data from this study explain the observed specific conditions of the NEA.

Anyway, overall this paper is a very valuable contribution and within the scope of BG. I suggest it to be published with minor to moderate modifications (as suggested above and also in the detailed comments section below).

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2 Detailed comments

P 5792, 5: Sometimes scientific programs are not correctly referred to. F.e. NABE was conducted only in 1989. Also on page 5812 you refer to the Biotrans study (47N, 20W). This site (47N, 20W) is better to be referred to as the Biotrans SITE. This site has been a reference study site used by a number of quite different studies like the German JGOFS pelagic field program and particle flux studies, the benthic Biotrans study, parts of international NABE and others.

P5792, 16: Bidigare ref. is from Sargasso Sea and that is not NEA. Sieracki et al. 1993 is perhaps the better ref.

P5792, 22: replace 'follows' with 'frequently follows' or similar

P5793, 6-8: give references please

P5796, 11: HEPA filtered air. Lab slang. Please explain.

P5800/Fig. 1c: colour scale (depth?) is not explained. Give label for colour bar, please.

P5801, 10. What do you mean by Mediterranean outflow waters (between 150 and 200m)? The deep Med sea outflow is usually deeper (1000m), right? Do you mean a meddy?

P5818, Fig. 14. Please add lat long to Fig. 14

Figures: Numbers (isoline labels in particular) are quite small; in the printout from the 'print' version it was partly impossible to read them.

Also printing the figure from this paper did not work very well. As said 'print' version figure often where too small to be useful at all and for some the print outs had errors (in comparison to screen view). The 'screen' version of the paper partly did not printout at all but stopped with error messages. The error msgs indicated some non-standard ps/eps/pdf commands being used. I had to try several times (on different printers) and

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finally had to printout the original figures from the src directory of this manuscript. Fig. 12 never printed out in the correct way. I strongly suggest that this is being checked by BG staff before the final version goes online.

Interactive comment on Biogeosciences Discuss., 6, 5789, 2009.