Biogeosciences Discuss., 6, C2155–C2161, 2009
www.biogeosciences-discuss.net/6/C2155/2009/

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.

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Received and published: 23 September 2009

Please find below our response to reviewer 2 and attached revised paper as supplement file "bg-2009-138-supplement.pdf".

Anonymous Referee #2

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.

We agree that there is no easy way of determining size-classes from pigments alone, and as this was pointed by all three reviewers as a potential weakness, we decided to withdraw Figures 8 and 9 computing size class fractions. These were much less discussed in the Discussion section than the actual FUCO and HEX distributions, hence their withdrawal should not have consequences for our conclusions. We kept the Tchla, FUCO, HEX and FUCO:HEX ratios data but combined them in one figure (Fig 8) and renumbered all the following figures consequently.

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.

We added a sentence at the end of the modified paragraph on this aspect p5812 l7:

"It is also known that detached coccoliths can accumulate in the surface layer and that these particles have a very high reflective index, which may bias satellite estimations. We emphasize that comparing satellite images to in situ data is not trivial and that monthly composites cannot be expected to represent local sites sampled during the cruise. Our point is to show that despite potential large meso-scale features, the general trends of surface Chla and calcite measured during the cruise in terms of range of concentrations and main features could be reflected by composite satellite images. Furthermore, we show in the following section that in situ PIC and HEX data were poorly correlated, which emphasizes that satellite calcite data cannot be directly converted to coccolithophore abundance."

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.

The situation described in Smith et al 1991 was located further north in the Fram Strait in the Greenland Sea (where coccolithophores are not observed) and was set in polar/ice edge waters and is more comparable to what has been observed in the Ross Sea with an intense Phaeocystis development. In the Smith paper, surface waters were enriched with NO3 and SiOH4 with similar concentrations as bottom waters and thus do not provide the same initial settings for the bloom development as in the NEA. Temperature might also be a controlling factor in this region as well. Even though we do not provide radically new results about the bloom development in the NEA, we believe that our data set, by its broad spatial coverage (compared to a lot of studies in the NEA which spanned much more limited areas), and its combination of numerous relevant measured and discussed parameters deserves more than a data report. There has also been a gap between the JGOFS program and more recent studies in the NEA, so our study provides a synthetic and recent view of the bloom development which will also be useful for future comparisons should climate change strongly displace or modify phytoplankton bloom sequence in this region.

Finally, the authors try to answer the question 'What causes recurrent silicic acid deple-

tion in the NEA and what are the potential consequences for phytoplankton 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.

Silicate depletion probably prevents diatoms from using up nitrate and phosphate in the NEA, leaving an opening for other groups such as prymnesiophytes to develop. However this has not been much documented since NABE, when Loechte et al 1993 suggested that Si limitation could occur, but were still unsure whether this was a recurrent feature or an exceptional year. Except for the POMME study (Leblanc et al., 2005) we don't believe other studies have since documented the widespread Si deficiency of the NEA compared to nitrate, nor emphasized the role of silicate limitation for the diatom bloom development in this region. This was shown during the POMME program between 40-45°, but the NASB data confirms this limitation and covers a much larger latitudinal span from 40 to 66°N. We think this is an important result that is still poorly referred to in the NEA, and that this section is worth maintaining in the manuscript, even though we do not have pre-bloom values for that year. Some reference to earlier work has been added page 5821 l22 :

"Since then, several other programs such as BIOTRANS, BOFS, PRIME and POMME conducted in the NEA during the productive season have reported Si depletion prior to N exhaustion later in the season, as well as consistently low Si:N ratios in the surface layer (Lochte et al., 1993; Sieracki et al., 1993; Passow and Peinert, 1993; Taylor et al., 1993; Savidge et al., 1995; Bury et al., 2001) that are well below the usual 1:1 requirement for diatoms (Brzezinski, 1985). From earlier work during the POMME program, it was shown that winter surface silicic acid availability between 40 and 45°N was already 2–3 μM lower than nitrate and that this deficit increased with depth, with a 5–7 μM difference between DSi and DIN concentrations at 1000 m (Leblanc et al., 2005)."

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).

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.

These corrections were all incorporated in the text : P5793 line3 "NABE (1989-1990)" P5793 line25 "NABE 1989-1990" P5812 line3 "The Biotrans site study"

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

This was indeed a mistake and the ref has been switched.

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

This was added line 22.

P5793, 6-8: give references please

Sentence was modified as follows: "Thus, the stoichiometry of initially available nutrients following winter deep mixing likely plays a crucial role in the structural development of the spring bloom, which feeds back on the availability of nutrients in the mixed layer (Moutin and Raimbault, 2002)."

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

HEPA filters, by definition, remove at least 99.97% of airborne particles 0.3 micrometers (μm) in diameter. This is a standard type of filters used in trace metal clean work or cultures. The sentence was modified as follows : "After flushing the tubing well, a 50 L polyethylene carboy was filled in a clean van and used for subsampling under HEPA-filtered air (removing particles above 0.3 μm diameter)"

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

The label is depth and has been added.

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?

This is a question mark for us as well. We can't explain such high salinities at the near surface level and did not want to mention a meddy as they usually flow deeper (yes about 1000 m).

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

The downloaded images do not have latitude scales originally. The latitude scales on these images are unfortunately very difficult to place, as this projection is orthographic and latitudes are differently spaced going poleward. We tried to indicate the approximate cruise track with visual help from coastlines, but lat-long would be very difficult to place correctly.

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 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.

All figures were redrawn to increase font size, and we will emphasize to the editors that some of the figures need a full portrait page to be readable.

Please also note the Supplement to this comment.

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