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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.

K. Leblanc et al.

karine.leblanc@univmed.fr

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Anonymous Referee #1

Please find below our response to reviewer 1 and attached revised paper as supplement file "bg-2009-138-supplement.pdf".

Additional Comments. I have a number of minor comments that the authors should consider before the paper is finally accepted. The discussion does seem overlong, and if it can be trimmed a little or tightened up it would be an advantage.

Figures 8 and 9 have been withdrawn, and TChla data is presented along with FUCO and HEX in a new Figure 8. Hence all sections describing size-class fractionation calculation from pigments and their subsequent distribution description have been removed from the manuscript, shortening it by several pages.

Page 5795, Section 2.2.1, lst 3 lines in the paragraph: It is not clear how Zm was determined - strongest gradient in density, fine, but over what depth interval, and what was the reference depth or density used? Presumably this was from the CTD data. I assume that Zn was determined from the difference between two niskin samples. What was the bottle spacing, and how was Zn determined - the midpoint between adjacent bottles, or the depth of the deeper bottle or what? This needs clarification.

Clarifications to the text were added in yellow p 5795 line 13 : "The depths of the mixed layer (Zm) and the nutricline (Zn) were determined as the depth of the strongest gradient in density and dissolved inorganic nitrogen (DIN) respectively between two measurements between the surface and 200 m. Treated CTD density data averaged every 0.5 m were used for the calculation of Zm, while nutrient data collected at 12 depths on average with Niskin bottles were used to compute Zn over the 0-200 m layer. At the highest concentration gradient identified between two Niskin measurements, Zn was determined as the depth of the upper bottle. The euphotic depth (Ze) was calculated as the 1% light level using CTD PAR data averaged every 0.5 m."

Section 2.2.4, Page 5798. Do the global weightings from Uitz et al (2006) hold for this region, especially given the two different communities encountered? Was any verification of the global weightings done? I have found that these weightings did not apply in a different part of the ocean, and probably regional and maybe seasonal weightings are really necessary. This is one of the potential major weaknesses in the paper.

Uitz et al 2006 used an extensive database of 2419 vertical profiles and 4238 surface data collected worldwide, but mainly in the Atlantic, Pacific and Mediterranean. These data and the statistical treatment were applied to obtain pigment weighting correspond-

ing to case I waters, spanning from oligotrophic to eutrophic conditions. However, 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 remove 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 removal should not bear 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 accordingly.

Section 3.1.2 Circulation, page 5800. The Azores current is not shown on Fig 1, and it would be useful to have it for non-North Atlantic readers.

The Azores Current at these longitudes flows between 30 and 35°N (Sy 1988; Paillet and Mercier 1997) and is hence not visible on this map, which starts around 37.5°N

Section 3.1.5, Mixed layer, euphotic zone and nutricline depth. Last sentence in the paragraph. This result is a little unusual, and may be related to the two different ways that the Zm and Zn have been calculated (continuous profile vs bottle samples). A comment here would be appropriate.

We agree that Zn calculations are less precise than Zm data, as one comes from bottle gradient calculations and one from CTD data averaged every 0.5m. Bottle sampling was often done every 5 m in the 0-30 m layer and every 10 to 20 m below 30 m. Hence some discrepancies between the actual in situ Zn and calculated Zn can occur. The error is potentially between 5 and 20 m, and is thus covering the average difference between Zm and Zn which is $\pm$14 m. We therefore agree that no conclusions may be drawn on the significance of the differences between Zm and Zn in our study. The text has been modified as follows p.5802, l.14:

"There were furthermore no significant correlations between Zm and Zn over the study area. While Zn depths were calculated from bottle data, spaced every 5 to 20m, Zm and Ze were calculated from CTD data averaged every 0.5 m. Hence, no significant

correlations can be calculated between Zm and Zn."

Section 3.3.2 Size-fractionated Chl-a distribution. See comments in Section 2.2.4 above. How good are the global weightings in this region?

See our response to this above.

Page 5806, same section. Line 23: "but increased significantly: was this actually statistically tested?

No, this was not meant to be a statistical statement, the word "significantly" has been removed.

Section 4.1 Bloom development – General Features. Page 5812. These two paragraphs are a bit weak. Given the comment on p. 5792 quoting Robinson et al of a area riddled by mesoscale and eddy activity, there should be no expectation that monthly composites would reflect point sampling on a ship. We have had trouble reconciling 2 day old images with what we see from ship instruments. A comment suggesting weekly composites are about as long a time span for comparisons with shipboard data would be useful. Some understanding that ocean colour satellites can only "see" chlorophyll in one optical depth (1/extinction coefficient) is a better reason for decoupling column and near-surface chl estimates.

We agree with this argument and have attempted to explain this in a more comprehensive way at page 5811 line 16-17, with this added sentence :

"Indeed, satellites only peer through the near surface to a depth equivalent to 1/extinction coefficient."

And at page 5812, line 6 with this added paragraph : "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. However, weekly images were too obscured by cloud cover to be useful. 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 suggest that satellite calcite data cannot be directly converted to coccolithophore abundance."

Section 4.2 Community Structure: Page 5814, lines 26-27. The meaning here is a little unclear: I'm not sure if the coccoliths accumulate or the coccolithophorids accumulate. Rewrite as "During growth : shed their coccoliths. These coccoliths are too small to sink, and tend to accumulate in the surface layer."

This is now corrected.

Section 4.2 Community Structure page 5815, lines 1-3. Is it worth saying something about the increased reflectance from detached coccoliths that may be giving the very high PIC estimates seen on satellite images? The presence of huge numbers of highly reflective particles can dramatically increase the reflectance signal, and may explain the differences.

We agree, but in this paragraph we no longer discuss differences with satellite images but discrepancies between measured PIC and HEX from water samples. We feel that adding a sentence about the high reflectance of coccolith would make this paragraph unclear. Rather we added this argument to section 4.1 dealing with satellite/in situ data comparison on p 5812 line 3.

"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."

Section 4.2 Community Structure page 5815, lines 14-15. Is there any evidence from the pigments to support the senescence argument from chlorophyllide or phaeopigments?

Indeed, phaeophyllides were much higher in the IB and IS. This sentence was added p 5818 line 19: "This is further confirmed by phaeophyllides concentrations (data not

shown), which were much higher in the IB and IS regions than in the PAP and RHP regions."

Section 4.2 Community Structure page 5815, lines 22-25. The meaning of this sentence is unclear. Rewrite, dividing it into two or more to clarify.

This sentence was rewritten as follows :

"Phaeocystis spp. is also known to produce FUCO (Schoemann et al., 2005) and could explain differences between BSi and FUCO comparisons. However, in our study, the presence of FUCO was always matched by the presence of BSi, and we often observed the presence of BSi without FUCO. Hence, it is likely that in our study FUCO was mostly indicative of diatoms."

Section 4.2 Community Structure page 5817, lines 3-4. In Table 2, in the IS region, HEX and Dsi shows a Rs of -0.417, 47 df, P =0.003. P is substantially less than the P<0.01 given in the table heading. Please correct statement.

This statement is now corrected as follows and the section "except over the IS" has been deleted.

Section 4.3, Phytoplankton control factors, lines 14-16. I agree with the thrust of the argument, but is there evidence for a shallower winter mixed layer over the PAP compared to further north? And is the store of nutrients in deeper water available to be mixed in during winter higher in northern waters compared to the PAP? A couple of sentences would strengthen this argument.

We address this issue in the following sentences, added on p.5818 line 16 "For the year 2005, the MLD was much deeper south of Iceland than over the central part of the NEA corresponding to the PAP until April (Mercator data available at http://bulletin.mercator-ocean.fr/html/welcome_fr.jsp). From May to July, this trend was much less obvious. Hence the latitudinal trend of the MLD during winter and early spring, but also the highest store of nutrients towards the North (Sarmiento and Grüber, 2006) may also

reflect the South-North increase in nutrient stocks in the stratified surface layer during the productive season."

Section 4.3, page 5819, lines 27-28. How was this depletion measured? Was it against winter values, and if so what were these so the magnitude of the drawdowns can be seen? Support assertions with numbers!

This sentence was misleading as, only DIN was lower in surface waters in the IS than in other regions. This was corrected as follows :

"Finally the IS was characterized by the highest biomass accumulation of the transect, which was reflected by an increased surface consumption of nutrients, particularly in DIN which showed the lowest concentrations encountered during the cruise (<1μM)."

5. Conclusions, page 5824, line 16. Replace If the temporal: : : with Although the temporal: :

This was corrected.

Figures and tables. Figure 1a: what is the FC, and where is the Azores current?

We forgot the label for the Faroe Current, which is now added in the Figure Legend. As noted above, the Azores current is not visible at this latitude.

Most of the problems concerning the figures originate from the fact that we submitted figures in portrait format, whereas BGD has adopted a landscape format. This has caused our Figures to be much smaller than intended, which makes some of the text and transect data very difficult to read. All figures were redrawn to increase font size and transect details, but we emphasize that in the final BG version the figures will be in portrait and should be much larger than the ones edited for BGD in landscape. We will ask the editor to keep one full page for the figures containing 3 or 4 transects.

Figure 1b and 1c: the legend numbers on the two graphs are too small to be easily legible in a printed copy. Figure 4: the legend numbers on the figure are too small for

paper publication. Figure 6: the legend text and numbers are about at the limit of size for successful paper reproduction. Figure 7: the legend numbers on the figure are too small for paper publication. Figure 8: the legend numbers on the figure are too small for paper publication. And, why are TChla concentrations in Fig 8a given in ng l-1, while ug l-1 is used for the size-classes in Figs 8 B-D. Figure 8 now presents TChla with HEX and FUCO which are all in ng L-1. Figure 9: the legend numbers on the figure are too small for paper publication. Figure 10: the legend numbers on the figure are too small for paper publication. Figure 11: the legend numbers on the figure are too small for paper publication. Figure 14: the legend numbers on the chlorophyll scales are not readable. Latitude bars on the images would help with the interpretation of the figure.

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.

Table 2: this is a horrible table, and I found it almost unreadable: numbers are much too small. The authors should consider omitting the non-significant results, or highlighting the significant results. There may be an argument for keeping correlations that nearly meet the fairly tough significant test (P<0.01) adopted in the table header. But, this table needs rethinking about what should be presented, and how it should be presented.

This table was redone, omitting P and df values, which makes the table easier to read. The correlations for the size-fractions are also removed. The range of dfs was indicated for each region in the Table legend. Insignificant rs (P>0.01) were omitted and the cells coloured in grey, significant results coloured in yellow. We believe this table is now much easier to read and still contains the needed information for the discussion.

Please also note the Supplement to this comment.

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