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 #1

Received and published: 4 September 2009

1. Does the paper address relevant scientific questions within the scope of BG? Yes

2. Does the paper present novel concepts, ideas, tools, or data? Yes

3. Are substantial conclusions reached? Yes

4. Are the scientific methods and assumptions valid and clearly outlined? Yes

5. Are the results sufficient to support the interpretations and conclusions? Yes

6. Is the description of experiments and calculations sufficiently complete and precise

to allow their reproduction by fellow scientists (traceability of results)? Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? Yes

9. Does the abstract provide a concise and complete summary? Yes

10. Is the overall presentation well structured and clear? Yes

11. Is the language fluent and precise? Yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes but see comments below.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes - see comments below.

14. Are the number and quality of references appropriate? Yes

15. Is the amount and quality of supplementary material appropriate? Yes.

Additional Comments.

This is a well written, interesting paper that is a pleasure to read, and it should be published. 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.

Page 5795, Section 2.2.1, lst 3 lines in the paragraph: It is not clear how Z_m 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 Z_n was determined from the difference between two niskin samples. What was the bottle spacing, and how was Z_n determined - the midpoint between adjacent bottles, or the depth of the deeper bottle or what? This needs clarification.

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.

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.

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 Z_m and Z_n have been calculated (continuous profile vs bottle samples). A comment here would be appropriate.

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?

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

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.

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 accu-

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mulate. Rewrite as "During growth ... shed their coccoliths. These coccoliths are too small to sink, and tend to accumulate in the surface layer."

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.

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?

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.

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.

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.

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!

5. Conclusions, page 5824, line 16. Replace If the temporal... with Although the temporal... Figures and tables.

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

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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 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.

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.