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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Silicate:nitrate ratios of upwelled waters control the phytoplankton community sustained by mesoscale eddies in sub-tropical North Atlantic and Pacific" *by* T. S. Bibby and C. M. Moore

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Dear All,

I find this manuscript has a number of issues that need to be addressed:

1) Regarding the physical oceanography, Figure 4 and the text appears to suggest that a thermostad of SAMW reaches the North Atlantic. Estimates of watermass age based on ^{3}He however indicate that the water between 100-250 m near Bermuda is only a few years old (e.g. fig. 9 in Jenkins and Goldman, 1985, J. Mar. Res. 43:465-491). It

might be considered to contain a minor fraction of SAMW, but it is not SAMW.

2) It is also suggested that Sargasso sea cyclones and mode-water eddies are "different water masses" (p 7514, line 24-25). Yet no T-S plots are presented to support this idea over that they are essentially different isopycnal displacements of the same watermass. For instance, if the values in Figure 3 and 2c were plotted against potential density rather than depth, would they be statistically different?

3) I noticed that the Si* values for Atlantic cyclones in Fig. 2b are in disagreement with Fig. 3a, which shows Si* in cyclones from 80 m to 140 m not only to be positive, but higher in cyclones than in MWE.

Part of this discrepancy might be due to the fact that Fig. 3 is BATS data and Fig. 2 EDDIES data. Yet Table 2 in Li and Hansell (2008, Deep-Sea Res. II 55:1291-1299), which is also EDDIES data, gives Si* values at cyclone centers of 0.36, 0.41, 1.16 and -0.06, i.e. 0.33 $\pm$ 0.22 standard error, significantly higher than here.

One possibility is that a mathematical error has been made. The other possibility is that you are using significantly different profiles than Li and Hansell (2008). However the difference between your results and theirs suggests the uncertainty is larger than suggested in Fig. 2b, and should at least extend to positive values. This uncertainty may be related to the fact that the DCM sits at the nitracline, where Si* transitions sharply from positive to negative (Fig. 3a). Thus there is an additional source of uncertainty, due to the fact that bottles spaced 10 m apart can only resolve the DCM to within $\pm$ 5 m, which in Fig. 3a can mean a Si* difference of 1, and positive versus negative values. That is, apparently you site the DCM deeper in cyclones relative to the nitracline than do Li and Hansell (2008)?

4) In any case, as phytoplankton modify nutrient concentrations at the DCM, it is unclear whether Si* there is a cause of the phytoplankton species composition (as you suggest), or an effect.

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5) Fig. 3a appears to foil the thesis of the paper. The 80-140 m bottles show higher Si* in cyclones than MWE, and the 160-300 m bottles show no statistical difference in Si* between cyclones and MWE.

6) Anticyclones are not shown. During EDDIES, one CTD/bottle cast was made at the center of an anticyclone, namely station 2 of R/V Oceanus cruise 404-1. The 300 m bottle had DIN of 1.5 and SiO3 of 0.7, for a Si* of 0.8. This is similar to the MWE value in Fig. 2c. Yet anticyclones generally do not contain diatoms (p 7514 lines 13-15). If anticyclones were plotted in Fig. 3, I expect they would have higher Si* than cyclones at a given depth (similar to MWE), because Si* increases with depth and anticyclones are a downward displacement of isopycnals. Thus I suspect anticyclones are a counter-arguement to the thesis of the paper, and I would like to see them plotted in Figs. 2 and 3.

7) You propose a qualitative relationship between high Si* and diatoms, without considering a mechanism of how this would actually work. Our understanding is that the diatom specific growth rate is related to nutrient concentrations typically through something like

diatom growth rate = min($[NO_3]/(k_N + [NO_3]])$, $[SiO_3]]/(k_{Si} + [SiO_3]])$) * etc.

Let us say k_{Si} = 1 mmol/m^3, and k_N = 1 mmol/m^3. Then we can contruct a table of growth rates as a function of NO_3 and SiO_3, using typical Sargasso values (Table 1). From the table, one can see (a) little relationship between Si* and diatom growth rate, though if anything it is higher for lower Si* (contrary to the manuscript), and (b) equal values of Si* do not have equal growth rates, as it matters whether a Si* value is due to excess silicate or a nitrate deficit.

An alternative mechanism is one of "logistical supply"; namely that diatoms will take up all the SiO_3 in upwelled water in excess of the background residual of 1.0 mmol/m^3, and NO_3 in a 1:1 ratio to SiO_3, and that other phytoplankton species will take up the remaining NO_3. Yet this mechanism does not show a relationship between high diatom

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Table 1. diatom specific growth rate as a function of Si*

SiO_3	NO_3	Si*	diatom specific growth rate
1	0	1	0
1	1	0	0.5
1	2	-1	0.5
1	3	-2	0.5
1.5	0.5	1	0.33
1.5	1.5	0	0.6
1.5	2.5	-1	0.6
1.5	3.5	-2	0.6
2	1	1	0.5
2	2	0	0.67
2	3	-1	0.67
2	4	-2	0.67

biomass and high Si* either (Table 2).

The problem is that MWE have higher Si* than Sargasso cyclones at 400 m not because of higher SiO_3 (which is barely above threshold), but because of lower NO_3 (Fig. 3a-c). The Sargasso cyclones have both higher SiO_3 and NO_3 at a given depth. The authors need to provide a mathematical model of how these nutrient concentrations could possibly cause enhanced diatoms in MWE but not cyclones, as the two mechanisms in the tables suggest the opposite.

Note a passive tracer release during the EDDIES field experiment found both persistent upwelling and enhanced vertical mixing in a MWE, suggesting the cause of diatoms in MWE is not related to greater nutrient concentrations, but greater nutrient flux (Ledwell et al., 2008, Deep-Sea Res. II, 55:1139-1160).

8) Thus I am not convinced that Si* explains the diatoms observed in Sargasso Sea mode-water eddies but not Sargasso Sea cyclones. However I think the manuscript is

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Table 2. diatom and picoplankton excess biomass (mmol N/m^3) as a function of Si*

SiO_3	NO_3	Si*	diatom N	picoplankton N
1	0	1	0	0
1	1	0	0	1
1	2	-1	0	2
1	3	-2	0	3
1.5	0.5	1	0.5	0
1.5	1.5	0	0.5	1
1.5	2.5	-1	0.5	2
1.5	3.5	-2	0.5	3
2	1	1	1	0
2	2	0	1	1
2	3	-1	1	2
2	4	-2	1	3

on track with regard to North Atlantic cyclones versus North Pacific cyclones. Fig. 1g,h show both to have about 5 mmol/m^3 NO_3 at 200 m; Fig. 1j,k consequently suggests that the North Pacific cyclone has 8 mmol/m^3 SiO_3 at 200 m, while the North Atlantic cyclone has 2 mmol/m^3 SiO_3. It is this difference in silicate concentration (not Si*, per point (7) above), that may allow diatoms to be more prevalent in North Pacific cyclones. This would be a new explanation, and I suggest the authors re-focus their manuscript around that.

Interactive comment on Biogeosciences Discuss., 7, 7505, 2010.

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