**Biogeosciences
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# *Interactive comment on* "Diatoms and their influence on the biologically mediated uptake of atmospheric $CO_2$ in the Arabian Sea upwelling system" *by* T. Rixen et al.

**Anonymous Referee #2**

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Diatoms and their influence on the biologically mediated uptake of atmospheric CO2 in the Arabian Sea upwelling system, by T.Rixen et al.

This paper addresses an interesting topic in a complex and dynamic ocean system. A quantification of the uptake of CO2 mediated by diatoms is a relevant question and would be suitable for Biogeosciences Discussions.

However, the complexity of the Indian Ocean and Arabian Sea hydrography directly illustrates to the weak points of the approach the authors used in this manuscript. A large number of assumptions is needed for all calculations carried out. Both the calculations of the proportion of oligotrophic waters for each station, and the calculations of the consumption of C, N, P and Si on which all conclusion are based assume that

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only 2 water masses play a role in the study area and that these can thus be used as end members. All properties of the intermediate points are calculated as resulting from mixing between these end memebers. Given the highly complex and variabel current system of the area during the SW monsoon, with filaments of upwelling water originating from the Somali coast and driven eastwards by the Findlater Jet (illustrated nicely in Schiebel et al; 2004, fig. 10), this seems an unjustified simplification, because the Findlater Jet crosses the transect sampled in this study. Vertical mixing may further complicate matters. This directly implies that conclusions based on such calculations may be a sum of errors resulting from these assumptions.

Several conclusions are listed as resulting from the calculations carried out in this paper, but they are either obvious or unsustained. By definition, diatoms will enhance rain ratios, so there is no need to draw this conclusion. The same goes for the statement that diatom blooms are terminated by the lack of silicon in the euphotic zone, because this is not surprising and has been known for decades. In the data produced I don't see any evidence for changing Redfield ratios. Finally, since the Si/N uptake ratios result from two calculated parameters they may have a large error and the differences shown may be partly or fully attributed to errors in calculation. Considering the uncertainties, such a difference would hardly be significant. The iron concentration in the surface waters hardly varies, so the conclusion that Si:N ratios decrease when iron increases does not appear justified.

A better evaluation of all errors involved is needed, both the errors associated with the analytical procedures and those associated with the many assumptions in the caclulations, to asses the accuracy of the calculated data and thereby the reliability of the conclusions resulting from these calculations. Without such an assessment, it is impossible to base conclusions on small differences as are observed in the dataset presented, because they may be entirely dus to a succession of errors.

Specific comments.

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Abstract: Conclusion about silica limitation is not new (a.o. Officer and Ryther, 1980), should be removed. I can't find any real evidence that an enhanced abundance of diatoms elevates carbon to nutrient uptake. The discrepancy between the calculated C:N ratios and the C:N ratios measured in the sediment traps, that give similar values along the transect and thus with decreasing importance of diatoms to the assemblage, gives some indication that the calculated C:N ratios may be incorrect.

In iron enrichment experiments, addition of iron was shown to enhance diatom blooms, not hinder development. (a.o. diTullio et al, 1993)

Page 105-106 According to Archer (2000) a doubling of the silicic acid inventory of the oceans is needed to increase the rain rate suficiently to lower atmospheric CO2 by 70ppm. Archer et al however cannot rationalize such a doubling. It is a very unlikely scenario that the oceans would simply shift to diatom production, because although diatoms would take over in the earlier stages of production, they would lose their advantage over other organisms when silicic acid gets depleted. Since organic carbon, being recycled by bacteria and other organisms, is recycled at much shallower depth in the water column than the silica frustules, which are only degraded by chemical dissolution, this would result in replenishing of surface waters with C, N and P but not Si, thereby shifting production to carbonate producing organisms. Only additional silicic acid could cause a permanent shift to diatom production. The external input to the marine silica budget is minor (5%; Treguer et al, 1995) compared to internal recycling, so it seems impossible to be able to supply the ocean with enough silicic acid to cause a major disturbance.

The POC/PIC ratio determined in a sediment trap at 3000m will shift from the ratio in the surface waters because of the high reactivity of POC compared to PIC. How valid are these extrapolations to the surface layer?

Results section As mentioned before, a large number of assumptions and calculations were applied before any results were presented. To be able to calculate any nutrient

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consumption, nutrient ratios, consumption ratios at all, the Arabian Sea system has to be highly simplified, by assuming two end-member water masses only. Looking at fig. 3, I have a problem connecting 3a and 3b. How can the high phosphate concentrations (1.2) at 500km offshore end up below the mixing line in fig 3b? I would assume that calculations on very low concentrations such as phosphate have a larger error than those carried out on high concentration such a silicic acid.

A similar figure comparable to figure 12, but then for Si:N ratios comparing water column calculations with sediment trap data would be helpful to get some idea about the reliability of the Si:N calculations from the water column.

Discussion

To calculate Si/N uptake ratios from calculated nutrient consumption values, it is assumed that N-consumption by diatoms is proportionate to their contribution to the phytoplankton assemblage. This would imply that all species, diatoms and carbonate species, have the same N uptake. Is this correct? When I calculate the ratio with estimated data from fig 9 a and b (Si cons. 5; N consumption 7 and % diatoms 15, this would give a Si/N ratio of 4.7 instead of 6 at the 600km offshore point. A second complication is that N, in contrast to Si, it recycled largely in the upper layer of the water column. This will compromise calculations. Is it possible to explain why the Si/N ratio would be high compared to the iron limited Eq. Pac. A higher Si/N would be expected with iron limitation. Why would the high ratio found after the transition to the oligotrophic region conflict with silicon limitation? The calculated Si:N ratio is determined largely by the contribution of diatoms to the assemblage, and since this is only 15%, meaning that only a small portion of the N consumption is attributed to diatoms. The diatom species assemblage is likely to change towards the end of a bloom period, and the Si:N uptake could change as well. Based only on 1 datapoint, calculated with a large possible error, it seems premature to conclude doubt the concept of silicon limitation.

What significance does the total Si:N uptake have? I don't think fig. 10c adds anything

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significant to the paper. The remark that the high iron concentration in the region from 50-600km offshore could account for the relatively low silicon export during cruise ttn49 is not supported by the data. Only at one station, 93 km offshore, a significantly higher iron concentration was observed.

Rain ratios The enhanced silicon uptake in the upwelling region hardly seems significant. It is to be expected that diatoms make up a larger part of the community in the upwelling area compared to the oligotrophic region (also shown by Haake et al., 1993). Lower rain ratios do not nescessarily imply an increased amount of diatoms. A changing POC flux, PIC flux, diatom flux or a combination of these three could be responsible.

Figure 10c does not show rain ratios derived from sediment trap data. These are shown in 11b. For comparison, it would be more interesting to show the sediment trap and surface data-derived rain rates in the same figure, as was done for C:N ratios in figure 12. This would show at first glance that the sediment trap-derived rain rates are more or less constant compared to those derived from surface data. This is shown nicely for the C:N ratios that appear to show a distinct trend in figure 11b, but show in fact similar values for all sediment traps.

Both sediment trap rain rates and C:N ratios were recalculated from a depth of >3000m to the 100m level. It seems impossible to account for all reminiralization and dissolution processes and come up with a reliable value C:N and rain rate history for when these particles were at 100m. More so because sediment trap data are collected over a much longer time interval and are therfore subject to changes in surface hydrographic properties. The enormous discrepancy observed between these sediment trap values and those calculated from the surface data hints that there are severe problems that compromize such an exercise. Considering all processes involved, it would be surprising if sediment trap and surface results would give comparable results.

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