

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

Anonymous Referee #1

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Review of “Diatoms and the influence on the biologically mediated uptake of atmospheric CO₂ in the Arabian Sea upwelling system”, by T Rixen, C Goyet and V Ittekkot.

General:

The Arabian Sea upwelling system is a dynamic and interesting system to study for insights into whole ocean biogeochemical functioning, as attempted in this paper. The topic of this paper is very appropriate for the journal. This paper covers similar ground to a recent paper on which Rixen is also an author (“Distribution of diatoms, coccolithophores and planktic foraminifers along a trophic gradient during SW monsoon in the Arabian Sea”, R Schiebel et al. 2004. Mar. Micropal. 51: 345-371), but Rixen et al. additionally look at implications for CO₂ dynamics. The CO₂ calculations in this

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MS are, however, rather indirect and involve many assumptions, and the conclusions therefore have a high degree of uncertainty. Because the novel conclusions of the paper are only weakly supported by the data, I do not recommend it to be accepted in its current form.

The conclusions section lists three main results. Conclusion #1a is unsurprising (diatoms enhancing rain ratios) and conclusion #1b is unsustainable and, I think, improbable (diatoms elevating Redfield ratios). Conclusion #2 is unsurprising. Conclusion #3 is based on incorrect calculations and is likely to be erroneous.

Specific:

1. Schiebel et al. (2004) must be cited and results of the two papers compared.
2. Abstract: “The new results imply that a deficiency of silicon (Si) in the euphotic zone terminates diatom blooms.” - this is already well known from Norwegian mesocosms (Egge & Aksnes 1992, already cited elsewhere in the MS), from study of North Atlantic seasonal succession at the NABE/BIOTRANS site (e.g. Lochte et al 1993, DSR-II, 40:91-), and elsewhere.
3. Abstract: “Ěbut indicate also that enhanced iron concentrations hinder the development of diatom blooms.” - because the conclusion about Fe effects on Si/N uptake ratios is incorrect, this implication does not follow. It would be in direct contradiction to all eight of the iron enrichment experiments carried out so far in HNLC regions, all of which stimulated diatom blooms.
4. pg 105, para 1: it would be preferable to include a more recent reference than Redfield 1963 for calcification effects on atm CO₂. “Geider” not “Greider” (also in reference list).
5. pg 105, para 2: because of the rapid decay of the POC flux with depth (and less rapid decay of the PIC flux) it is meaningless to quote rain ratios without reference to depth. The actual rain ratio at 100m will be very different (5-fold?) from that at 500m.

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6. pg 105, para 3: Heinze et al 2004 (GRL, 31: doi:10.1029/2004GL020613) and Chuck et al 2005 (Tellus B, 57:70-) should also be cited for CO₂ impacts of reduced calcification. The impact of altering the rain ratio may depend on whether it is changes in the POC flux or in the PIC flux (or both) that alters the ratio. When quoting any impact on atmospheric CO₂ it is also important to give an indication of the time interval. The effect will be much larger by year 2500 than by year 2100. It is unlikely that the glacial-interglacial CO₂ difference is driven solely by variation in the diatom-coccolithophores balance because this entails changes in the lysocline depth which are not seen in the sedimentary record (see Archer et al 2000, cited in the MS). Harrison (2000) and Conley (2002) consider the effects of variations in the inputs of silicon, not iron.

7. These calculations assume that there are only two sources of water: either the upwelling zone (characterised by water at station S1) or the open ocean (for which station S13 is assumed to be representative). But the circulation is more complicated, with the SW monsoon leading to currents such as the Findlater Jet (referred to in the paper) traversing the middle of the transect in a NE direction. Some part of the water in the middle of the transect has therefore presumably come from the SW, and may be different in composition (e.g. different nutrient levels and salinity) than the water to the E near Station S13. The analysis is compromised in this case. The assumption that station S13 is representative of oligotrophic source waters supplied to the transect needs to be justified. The calculation of errors using a random-number generator is incomplete because only errors associated with the measurements are considered, errors associated with approximations/assumptions in the calculation procedure are ignored. The latter source of errors should be acknowledged, and quantified if possible.

8. pg 111, near bottom: “The resulting mean CO₂ emission was subtracted from the amount of DIC held in the mixed layer”: surely they should be added?

9. pg 113, Si/N uptake ratios: both uptake rates are by calculation rather than from direct measurements, and the ratio of the two is doubly uncertain. It would have been interesting to see a comparison to in-situ measurements of P_{Si}/P_{ON}. Do such mea-

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surements exist? How do they compare? Si/N uptake would be expected to be lower in the Arabian Sea (iron replete) than in the iron-limited HNLC areas of the Eq Pac and S Ocean. Instead it is found to be similar or higher. The discrepancy should be pointed out. These discrepancies are presumably due to an additional problem: what is calculated is not actually the uptake ratio, it is the export ratio. Because most organic matter (perhaps 90%) that is formed is subsequently remineralised back into the same surface layer, uptake obviously does not equal export. The ratio of uptake:export is probably higher for PON than for P*Si* (less surface layer dissolution of Si than remineralisation of PON). This would explain the discrepancy above, because Si/N of export will be large compared to Si/N of uptake. Throughout the paper “Si/N uptake ratios” needs to be changed to “Si/N export ratios”. Conclusion #3 is not supported and should be removed (also from abstract).

10. pg 114, para 1: there are only 4 points for Si/N uptake (fig 9c) and the highest is not, as stated, 900km offshore. There is no obvious correlation between the two (figs 9c vs 9d): the highest (~600km offshore) and lowest (~300km offshore) Si/N uptake ratios are at similar iron concentrations.

11. page 114, para 3: The calculation of POC C-uptake is also affected by uncertainties in calculating the amount of outgassing that has taken place.

12. page 115, para 2: the calculation of C:N ratios is likewise compromised by the difference between export and uptake and it is interesting that the trend seen in the calculations is not seen at all in sediment trap data (compare two curves in Fig 12). In contrast to the result here, Quigg et al (2003, Nature, 425:291-) compared C:N, N:P and C:P ratios in different functional groups and found diatoms to have either medium or low, but not high, stoichiometric ratios compared to other functional groups. The part of conclusion #1 which relates to Redfield ratios should be removed because it is not substantiated, and also from the abstract.

13. The calculations all assume no vertical exchange. This should be justified in the

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text, otherwise the import of DIN-depleted water from the oxygen minimum zone further compromises the calculations.

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