

## ***Interactive comment on “A system in balance? – Implications of deep vertical mixing for the nitrogen budget in the northern Red Sea, including the Gulf of Aqaba (Eilat)” by C. Häse et al.***

**S. Naqvi (Referee)**

naqvi@nio.org

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### General (Major) Comments

This is an interesting paper that shows significant difference between N-P relationships in the northern Red Sea and the Gulf of Aqaba, which is attributed to the difference between ventilation of the two regions i.e. while winter convection keeps the entire water column well-oxygenated in the Gulf of Aqaba, stratification in the Red Sea allows the development of a pronounced oxygen minimum. Consequently, waters in the northern Red Sea are characterized by lower N:P (11.3+/-0.4) and higher nitrate deficit (2.2+/-2.4 microM) than those in the Gulf of Aqaba (13.1+/-0.3 and 0.3+/-0.6 microM,

respectively). A more vigorous sedimentary denitrification in the former region is invoked to explain the lower than Redfieldian N:P value in the northern Red Sea with a corresponding deficiency in nitrate.

My biggest concern about this paper is that the data being reported here are not consistent with previous measurements in the region. For example, observations at the GEOSECS Station 405, located very close to the Meteor Station X in this study, give the following relationship:  $\text{NO}_3 = 18.47 \text{ PO}_4 - 0.88$  ( $r^2 = 0.9988$ ). Nitrate deficits at this station, computed with the same equation as in this study, are  $-(0.63 \pm 0.68)$   $\mu\text{M}$  and  $-(0.91 \pm 0.32)$   $\mu\text{M}$  for the entire water column and for depths exceeding 100 m, respectively. More intensive surveys in the Red Sea yielded even higher N:P ratios - 20 by Grasshoff (1969) and 21 by Naqvi et al. (1986). These high ratios were ascribed by Naqvi et al. to nitrogen fixation in the Red Sea. I wonder if the low N:P and high nitrate deficits for the northern Red Sea being reported here are the artifacts of a systematic error in the Meteor data (see lines 8-15 on p. 389)? If this is the case, then the difference between the Gulf of Aqaba and the northern Red Sea might still be valid; otherwise these data are not of much utility.

On the other hand, if the data are accurate, then a stoichiometric shift has probably occurred in the Red Sea region in recent years. This, of course, assumes that the ratios do not vary seasonally (note that while the GEOSECS observations were made in December, before the onset of convective mixing regimes in the adjacent gulfs, the period of the Meteor expedition corresponded to peak convection). Moreover, any seasonal dilution of the Red Sea Water with the Gulf of Aqaba Water would produce a mixture with a N:P value intermediate of the two end members. The lower N:P value in the Red Sea than in the Gulf of Aqaba indicate that this was not the case. Taking the data at the face value, a plausible explanation of anomalous observations is as follows.

As stated above, the Red Sea has been believed to be a region of net  $\text{N}_2$ -fixation. This is due to the fact that the inflowing surface water from the Gulf of Aden is nitrate depleted but contains substantial amounts of phosphate as a consequence of denitri-

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fication in the Arabian Sea. In the absence of any significant nutrient inputs through runoff from land and deposition from the atmosphere, this excess phosphate can only be utilized by organisms capable of N<sub>2</sub>-fixation. Now if the nitrogen loading in the region is increased without a corresponding increase in phosphate inputs (as one would expect from the ongoing increase in atmospheric nitrogen deposition - Galloway et al., 2004), then the extra nitrogen could be utilized by non- N<sub>2</sub>-fixing autotrophs to produce organic matter having a lower N:P ratio. It is possible that the competition between N<sub>2</sub>-fixers and other phytoplankton for the “excess” phosphate could eventually lower the N:P ratio not only in the Red Sea, but in other parts of the oceans as well. Alternatively, assuming that the lower N:P observed by the authors is caused by sedimentary denitrification in the Red Sea, it would imply that this process has intensified in recent years. This could have happened in response to an increase in carbon export. However, a comparison of the oxygen data does not support a decrease in mid-depth oxygen concentrations; in fact, if anything, the minimum oxygen concentration recorded at Meteor Station X (~80 microM) is substantially higher than the corresponding value (66 microM) at GEOSECS Station 405. Thus, the question is: Has the postulated shift in N:P become already perceptible in the Red Sea? I think this is one important issue that this paper should address. If there has indeed been a stoichiometric shift in the Red Sea, the system is certainly not in balance, exactly opposite to what is stated in the title!

The discussion on what limits primary production/nitrogen fixation (proximate versus ultimate limiting nutrient and iron deposition) lacks focus. I suggest the authors concentrate primarily on differences between the Red Sea and Gulf of Aqaba and the possible stoichiometric shift in the region. In both of these land-locked bodies, which are located in one of the most arid parts of the globe, iron cannot be expected to limit primary production or N<sub>2</sub>-fixation. It is the excess phosphate in nitrate-depleted surface waters that controls N<sub>2</sub>-fixation. This phosphate-excess makes nitrogen the limiting nutrient and stimulates the growth of diazotrophs. However, since N<sub>2</sub>-fixation itself will be terminated once phosphate is exhausted (as happens during the northward transport of the surface water), both nutrients limit overall biological production. The authors

should include these points in a concise discussion at one place rather than spreading it over several sections, and this cannot be the main theme of the paper.

The other major comment I have pertains to the way the data were processed. The apparent oxygen utilization (AOU) is presented as the difference between the depth-integrated saturation values and similarly integrated observed concentrations. It is not clear to me why AOU was not computed for individual sampling depths, even though I think that the integrated values will still be not too different? Similarly, the dissolved inorganic nitrogen (DIN) and phosphate were also vertically integrated to investigate the AOU-DIN and AOU phosphate relationships, and for computing the depth-integrated nitrate deficits. Scatter plots using pairs of data from individual depths can provide additional information (e.g. variability with depth).

#### Specific (Minor) Comments

The manuscript will be benefited by a careful copy-editing. Some specific comments/suggested changes are as follows:

Page 385, line 8: Replace “losses in N or P” by “Losses of N or P”.

Page 385, line 9: Delete “either”.

Page 385, line 13: Replace “This we attributed” by “This is attributed”.

Page 385, line 15: Replace “Redfield” by “the classical Redfield value of 16”.

Page 385, lines 17-19: I do not see the link between nitrate deficit and iron deposition, nor would I endorse a generalized conclusion of the Red Sea data strongly supporting the concept of nitrogen as the proximate and phosphorus as the ultimate limiting nutrient in the ocean as a whole.

Page 385, line 23: Replace “nutrients” by “two macro-nutrients”.

Page 386, lines 16-18: How is the limitation of productivity by nitrogen or phosphorus connected to the occurrence of nitrate deficits?

Page 387, lines 20-24: Change “Water renewal times ...(cf. Hulings, 1989).” to ‘The upper 200 m of the Red Sea water column has been estimated to be renewed once every 6 years, whereas estimates for the renewal time of the entire water body range from 30-45 years (Plähn et al., 2002) to 200 years (UNEP, 1997). Water renewal in the Gulf of Aqaba is much more rapid (1-2 years - Hulings, 1989).’

Page 390, lines 9-10: Change the sentence “ ... about 200-300 m with a slight peak in the middle stations (II-IV), and lower values at both ends of the gulf ..” to “ ... about 200-300 m, slightly deeper at the middle stations (II-IV) than at both ends of the gulf ..”

Page 392, section 4.4: Is the discussion on preformed nutrients necessary?

Page 392, lines 22-25: On the issue of the nitrate deficit occurring in the water column by processes other than denitrification (including within micro-anaerobic sites), please refer to Li et al. (2006) who report appreciable deficits in relatively oxygenated waters and invoke nitrification as a possible mechanism.

Page 395, lines 19-21: Not clear.

Fig. 3, caption: Delete “Horizontal”.

### Conclusion

I am not in a position to pass judgment on the manuscript as I am not sure about the data quality and also because, as I have argued above, a recent stoichiometric shift caused by enhanced N deposition from the atmosphere cannot be ruled out. In the latter event, this data set will make an important contribution to the literature. The authors have to convince themselves and the editor that the data being reported are accurate and revise the manuscript making appropriate caveats for the reader to make his/her own judgment and to stimulate further research on the topic.

### References

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