Biogeosciences Discuss., 3, S226–S232, 2006
www.biogeosciences-discuss.net/3/S226/2006/

Biogeosciences Discussions

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.

Anonymous Referee #2

Received and published: 25 May 2006

Review for C. Haese et al., 2005

"A system in balance? - Implication of deep vertical mixing for the nitrogen budget in the northern Red Sea, including the Gulf of Aquaba (Eilat)"

In Biogeosciences Discussions

General comments:

The authors are presenting new data from the northern Red Sea and the Gulf opf Aquaba, focusing mainly on nitrate (and nitrite), phosphate and oxygen concentration. In the introduction the authors mention their goal to investigate the implication of deep

winter mixing on the nitrogen budget. Understanding the nitrate deficit oberved in the global ocean is a large goal within ocean biogeochemstry, and hence the work presented here is within the scope of Biogeosciences.

However, I have to admit that I found the manuscript very hard to understand. The title, abstract, and the introduction do not seem to have a clear line and a clear statement where the authors want to put their focus on. It seems, that a lot of work has been done with the presented data set, and every aspect was put into this manuscript. To me, this made the manuscript quite confusing to read. I don't want to question the general science behind the presented work, but the way it is presented. Therefore, I have structured my commments by the paragraphs as they appear in the manuscript.

Specific comments:

Abstract:

I found the paper written in a somewhat confusing way. The main aim of the paper is not clear to me. From the title I would reflect that the authors are investigating the nothern Red Sea and the Gulf of Aquaba as a whole system, but the first two sentences in the abstract reveal that they are doing a comparison. This should be reflected withing the title. The authors argue that benthic denitrification is the major difference in these two systems, leading to significantly lower nitrate deficits in the Gulf of Aquaba. The second last point in the paper mentions nitrate as the ultimate limiting nutrient. So is this the qestion that the authors want to address with their investigations? Where is the connection to deep convection and higher O2 concentration mentioned earlier and benthic denitrification? The last point about eutrophication comes somewhat out of nowhere. However, there are plenty of studies e.g. withing the Baltic Sea, that have addressed the issue how important it is to reduce phosphate to reduce eutrophication.

Minor comments: I would not put something like "Redfield" into the abstract without explanation. Although probably everybody in the field knows about the constant stoichiometric ratio presented by Redfield et al., 1963, "Redfield" is in my opinion not

BGD

3, S226–S232, 2006

proper writing.

Introduction:

From the introduction it seems, that the authors aim to investigate the nitrate deficit in the water column of the Gulf of Aquaba, by applying the 'concept of nitrate deficit', and they refer to e.g. Gruber and Sarmiento, 1997. Minor comments: The authors refer to Gruber and Sarmiento, 1997, but this paper is not listed wihtin the references.

Study Site:

All the sills and sites mentioned in this paragraph should be shown in the map, that would help a reader who is not familiar with the investigated region.

Method:

3.1:

Data that are not shown should not be mentioned. I find it confusing to talk about a three years time-series, and no data are shown from this. Instead, the work is focusing on data collected during February 1999. This one month investigation is the base of the presented research.

Why are mixing dephts mentioned ? Are they important for the calculations?

Minor comments: What kind of nutrients have been measured, that are presented? What is the accuracy/presicion? When trace metals are not shown, there is no need to list it.

3.2:

It would be good to include an equation that shows the N* concept, or 'concept of nitrate deficit', in addition to the description mentioned in the text.

Minor comments: Please define total oxidized nitrogen

Results

BGD

3, S226–S232, 2006

4.1

When there is no exchange in oxygen and nutrients between the upper mixed layer and the deep waters, i.e. the northern Red Sea is always depleted in nitrate? Is the only surface N input here due to N2 fixation?

The authors should explain why it is necessary to use nitrate+nitrite instead of nitrate only when using the N* method.

To what extend can the larger variance in the nitrate deficit in the northern Red Sea be attributed to a not representative distribution of stations in that region? The gulf seems much better covered and seems to be an almost enclosed system, seperated by a sill from the northern Red Sea. But looking at the space distribution of the stations within the northern Red Sea, there are open boundaries. Please clarify.

4.3

The authors mention a mixing depth of 200m in the Gulf of Aquaba, whereas in 4.1 they mention mixing depths up to 500m and say that the oxygen concentration stays high even "far below" the mixed layer. The oxygen profile looks like that the concentration is slowly decreasing to a depth of around 500m in the gulf (the maximum of the mixed layer depth in the north of the gulf), and then stays more or less constant with increasing depth. Can this oxygen signal be due to lateral transport of oxygen rich water from the north along that depth?

Is the oxygen minimum zone in the northern Red Sea related to the oxygen minimum zone of the Arabian Sea?

Here, the mixed layer depth is mentioned, but until here, the results have been presented for the entire (sampled) water column. Why is this important to mention the nitrate deficit within the mixed layer depth at this point?

Does the nitrate deficit really reflect the reverse oxygen concentration? In the Gulf, at depth between 200 and 400m, a negative nitrate deficit has been observed, doesn't

BGD

3, S226–S232, 2006

EGU

this mean it is positively correlated with oxygen at these depths ?

In Fig. 5 it looks like that AOU is only linear related to the nitrate deficit within the Gulf of Aquaba and for station VII in the northern Red Sea, which is located closest to the Gulf. For the other points, I cannot see a linear relationship. Please clarify.

4.4 The applied constant stoichiometric ratio for (C:)N:P:O (Redfield et al., 1963) should be mentioned once in the manuscript.

Discussion:

The discussion starts with mixed layer only for the Gulf of Aquaba, but the discussion for the northern Red Sea is for the entire water column?

Is the northern Red Sea a more "steep and narrow basin" compared to the Gulf of Aquaba? Here, this is the main argument for benthic denitrification, based on pers. communications. When this is the main aim of this paper, this should come out more precise.

5.1

The authors talk about Trichodesmium blooms and try to explain why N2 fixation rates due to Trichodesmium blooms cannot be lower in the Red Sea compared to the Gulf of Aquaba. However, to me it seems that the reasons mentioned are pretty much speculation. Since they are citing several papers that Trichodesmium blooms are well known in both areas, it would help to give some numbers about N2 fixation rates, and also some numbers about dust deposition. E.g. iron can also entering the water column through the sediments. Looking at the map, it seems that due to the shape of the Gulf, it can have a much larger lateral iron impact from the sediments compared to the wider Red Sea. So far, the arguments sound handwaving to me.

How much is known about 'subsequent remineralization' of Trichodesmium, and what exactly do the authors mean by this?

BGD

3, S226–S232, 2006

5.2

The fact mentioned here would be better represented by showing e.g a mean of all oxygen and nitrate deficit profiles, not only in one example.

Again: do the data from the the stations sampled in the northern Red Sea give a good mean estimate for the entire region?

5.3

Why this extra paragraph about iron limitation? Shouldn't this be a part of 5.1? And if not, why not? Are there any iron data that can support the authors arguments?

5.4

In 5.3, the authors mention that N2-fixation may be stimulated by "phosphate-containing dust than by levels of dissolved iron in the ambient water". In 5.4 they say, that the Gulf of Aquaba is exposed to episodic phosphate enrichment due to mining. When iron can be ruled out as a reason for different rates of N2 fixation in both regions, can it be possible that there is a difference in these rates due to these episodic phosphorus enrichment events ? Or is the same true for the northern Red Sea?

Reducing phosphate to fight eutrophication has been proposed in other regions as well (e.g. Baltic Sea). Therefore, one citation to this topic from other regions would be appropriate. However, this conclusion comes a little bit out of nowhere, and mentioning dying reefs within this manuscript might rather confuse then help the reader in this context.

Technical corrections:

Gruber & Sarmiento (1997) are mentioned in the text but not listed in the refences.

Fig 5: the "a" is not necessary

Fig 6: Stations IV and V are mentioned in the figure captions, but not shown in the

BGD

3, S226–S232, 2006

figures.

Interactive comment on Biogeosciences Discuss., 3, 383, 2006.