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Biogeosciences Discussions

Interactive comment on "Coastal versus open-ocean denitrification in the Arabian Sea" *by* S. W. A. Naqvi et al.

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We thank Hermann Bange, Maren Voss and the anonymous referee for their helpful comments. Our response to each review, point-to-point, is as follows:

1. REVIEW BY MAREN VOSS

Referee's comment: However, I had major difficulties to judge which data belong to what category. To indicate which material has been published and which not would be helpful with a clear citation. The same is true for some figures which refer to past cruises but it stays unclear whether the material was published elsewhere or not.

Our response: A large part of the data being presented here is unpublished. In the revised version, the published data have been identified in figure captions.

Referee's comment: Please show a map with all stations presented offshore and at the

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coast.

Our response: Locations of all stations are now shown in Fig. 1, Fig. 2 (b, inset) and a map that accompanies cross shelf sections (Fig. 3).

Referee's comment: Page 673 line 20. The loss of 20 micromolar NO3- within a month would demand a loss rate of app. 0.7micro-mol/L/d which is similar to 0.83micro-mol (line 24).

Our response: It lends support to our calculation.

Referee's comment: Page 674 line 10. There is a 25 years gap between the UNDP/FAO cruises and the years 97-04. Can one really talk about in trend over time in this case? I think it is more a two point observation.

Our response: While it is true that we are relying mostly on the two sets of data taken during 1971-1975 and 1997-2004, we are also referring to the data included in Naqvi et al. (2000, supplementary information). These data while not strictly showing a trend reveal a distinct change. Note that even though over two decades apart these observations were made over extended periods and should therefore not be considered as two "points".

Referee's comment: Line 19: I cannot agree to a regime shift on the Indian shelf. There may have been drastic changes, but a regime shift needs more features than suggested here.

Our response: We have replaced the term "regime shift" by "change".

Referee's comment: Page 674 line 14: It is the presence of a zero Winker oxygen, not the absence.

Our response: What we meant was that zero oxygen values are characteristic of sulphate reducing systems. The referee misunderstood the statement which has now been modified.

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Referee's comment: Figures 3 and 6 are very small and numbers difficult to read. Figure 2 shows isolines of temperature, salinity, O2.and nitrite, esp for the latter the data points may be shown.

Our response: We have modified the figures as desired by the referee.

2. ANONYMOUS REVIEW

Referee's comment: The distinction between previously published and new data is not quite clear, however, and I agree with the comment by Maren Voss concerning this issue.

Our response: A large part of the data being presented here is unpublished. In the revised version, the published data have been identified in figure captions.

Referee's comments: I also miss a brief summary of the main differences between the two regions, preferably in the form of a table.

Our response: The major differences between the coastal and open-ocean systems are summarized in the abstract, and to present them in a Table as well will be redundant.

Referee's comments: Heavy N2O, p. 675-676: The inference of large N-fractionation associated with the N2O-N2 step (p. 676 l. 1 on) is contradicted by the later statement that other processes than the (stepwise)denitrification contribute to N cycling (l. 9 on). Thus, the first part should be softened, e.g., "The higher delta15N of N2O SEEMS to imply ..."

Our response: Accepted.

Referee's comment: In parts of the paper NO3- means nitrate and in other parts, it means nitrate + nitrite. This ambiguity is confusing and becomes awkward in places such as p. 680 l.10-12. Throughout the paper NO3- should be used only for nitrate, while NO3- + NO2- should be used whenever the data pertain to the combined pool.

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Our response: We have introduced a footnote the first time the isotopic data of nitrate is mentioned, indicating that it actually includes nitrite as well. However, because it is cumbersome to use the term nitrate+nitrite repeatedly, and as is customary, we have thereafter retained the term nitrate alone. Nevertheless, we have ensured that it did not cause confusion or awkwardness as pointed out by the referee.

Referee's comments: It should be stated explicitly that the calculation of expected delta15N values for nitrate, on p. 679 l. 2 on, are based on the assumption of a rayleigh distillation (which is first introduced at the bottom of p. 680), i.e. that the waters behave as what isotope geochemists typically refer to as a closed system. In the discussion of mixing as a possible explanation of the low observed values, it should also be noted that continuous mixing ("open system behaviour"), would also result in an under-estimation of the fractionation factor.

Our response: Accepted; necessary changes have been made.

Referee's comment: Isopleths should always be accompanied with a description of the interpolation method used for their generation. The string-of-pearls-like surface oxygen curves in Fig. 2 indicate that the method used for this plot might not be optimal.

Our response: The isopleths were drawn using Surfer for Windows version 8 by Golden Software, Inc. We do not believe that the pattern observed is an artifact of inappropriate interpolation.

3. REVIEW BY HERMANN BANGE

Referee's comment: p. 666, l. 20-21, Introduction: This sentence is misleading. Nitrogen fluxes are not necessarily controlled by O2 alone (e.g. N2 fixation fluxes are controlled mainly by Fe, P,).

Our response: Accepted. The sentence has been modified.

Referee's comment: p. 669, l. 1-11: Based on results from moored sediment traps, Rixen et al. 2000 (Sedimentation in the western Arabian Sea: The role of coastal and

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open-ocean upwelling. Deep-Sea Res. II, 47: 2155-2178) were able to show that fluxes of organic material to the deep western Arabian Sea are higher than the fluxes of organic material to deep central and eastern Arabian Sea. Thus, the intermediate waters in the western Arabian Sea receive more organic material than the central and eastern Arabian Sea. Moreover, the shapes of N2O profiles from the western Arabian Sea are very similar to the profiles observed in the central and eastern Arabian Sea (Bange et al. 2001). If we assume that the N2O consumption in intermediate layers is caused by denitrification, we can conclude that there are at least two different types of denitrification pathways operating in the Arabian Sea (see e.g. Bange et al. 2001, who suggested denitrification via IO3-/I- to explain the observed N2O profiles; see also Farrenkopf et al. 1997 (Sub-surface iodide maxima: Evidence for biologically catalyzed redox cycling in Arabian Sea OMZ during the SW intermonsoon. Deep-Sea Res. II, 44(6-7): 1391-1409.). This implies that the use of the secondary nitrite maximum as the sole indicator of denitrification might be misleading and might be the reason for the 'apparent' paradox that the denitrifying zone in the Arabian Sea is not directly connected to the centers of upwelling.

Our response: We have made substantial changes in the manuscript in response to the referee's views concerning high POC fluxes in the western Arabian Sea and that some denitrification may also take place outside the secondary nitrite maximum zone including relevant references. However, the postulated production of N2O through ammonium oxidation is yet to be fully established. Moreover, we remain convinced that vigorous denitrificattion is confined to the secondary nitrite maximum zone.

Referee's comment: p.669, l. 12-15: The aerosol measurements by Siefert et al. 1999 (Chemical characterization of ambient aerosol collected during the southwest monsoon and intermonsoon seasons over the Arabian Sea: Labile-Fe(II) and other trace metals. J. Geophys.Res., 104(D3): 3511-3526.) support the view that the primary production during the SW monsoon is indeed Fe limited. Air masses during the SW monsoon are coming from the pristine southern hemisphere and do not carry much Fe to the Arabian

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Sea waters (Siefert et al. 1999).

Our response: We have included this reference.

Referee's comment: p. 673, l. 2: Please provide a reference for the given range of denitrification estimates.

Our response: We have added one more reference (Mantoura et al., 1993).

Referee's comment: p. 673, anammox vs denitrification: The presence of anammox does not necessarily means that N2 production (from 15NO3- incubation experiments) is underestimated because both processes depend on the same substrate: nitrate. During anammox NH4+ reacts with NO2- to form N2. However, NO2- is delivered by denitrifiers via reduction of NO3- to NO2- (see Kuypers et al. 2005). BUT: One might speculate that NO2- is delivered by nitrifier-denitriers under suboxic conditions. Then, indeed, we have two substrate-independend processes leading to N2: 1. classic denitrification and 2. nitrifier-denitrification coupled to anammox (for the 2. case incubation with 15NO3- would underestimate N2 formation rates).

Our response: The referee's logic is correct. However, Kuyper et al. (2005) found lesser production of 29N2 in experiments involving 15N labeled nitrate alone than in those conducted with 15N labelled nitrate and unlabelled ammonium. Nevertheless, we have modified the statement, "Thus, incubations with 15NO3 are expected to lead to an underestimation of the extent of N2 production" to "Thus, incubations with 15NO3 alone might lead to an underestimation of the extent of N2 production".

Referee's comment: p. 674, anthropogenic influences. The CATS data shown in Figure 4 are means for the period 1997-2004, which do not allow deciphering any interannual trends. Is it possible to separate the CATS data into annual data sets in order to see interannual variabilities/trends? (which might indeed be caused by anthropogenic activities)

Our response: We have observed substantial interannual (and also intra-seasonal)

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changes at the CATS site, but as yet no clear secular trend. The inter-annual changes will be published elsewhere. However, we have included additional text here to address this issue.

Referee's comment: p. 674, l. 25: Bange et al. 2001 calculated atmos. N depositions for the central and western Arabian Sea but not for the eastern (coastal) Arabian Sea. Thus the argument may not be valid.

Our response: While Bange et al.'s data were indeed for the open ocean, the deposition of N from the atmosphere is expected to be even higher over the shelf. This issue is discussed in more detail in the revision.

Referee's comment: section 5: N2O cycling. In Figure 4 an interesting novel N2O data set from CATS is shown, but is not discussed in the context of section 5. I would like to suggest to omit the N2O data in Figure 4 or to discuss the data in more detail.

Our response: N2O data at the CATS site have been discussed in greater detail in the revised version, as advised by the referee.

Referee's comment: Please provide subtitles "open ocean" and "shelf" in each section. This will help to clarify the structure of the ms.

Our response: The comment is well taken, but the suggested reorganization would interrupt the flow, and after different options we found the existing structure to be the the best.

Referee's comment: Fig. 1a. Please indicate the location of CATS station.

Our response: The CATS site is now shown in Figs. 1 and 3.

Interactive comment on Biogeosciences Discuss., 3, 665, 2006.