Biogeosciences Discuss., 5, S845–S850, 2008
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Biogeosciences Discussions

Interactive comment on "Recent advances in the biogeochemistry of nitrogen in the ocean" *by* S. W. A. Naqvi et al.

S. W. A. Naqvi et al.

Received and published: 15 June 2008

Authors' Response

We thank Dr. Lou Codispoti and the other referee for their very positive and helpful comments. We have revised the manuscript accepting all of their suggestions as described below.

Response to Review # 1

.... there are some minor points that I would like the authors to consider before publication.

Page 3: Thamdrup/Dalsgaard and co-workers 2002/2003/2004 were probably the first to detect anammox activity in sediments. They also reported a relation between the organic content and the relative importance of anammox an denitrification in sedi-

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ments,which I think should be taken into this section.

Our response: We have included two additional references (Dalsgaard and Thamdrup, 2002; Thamdrup and Dalsgaard, 2002) that provided the first reports of anammox in marine sediments. The text has been suitably modified also including the observation by these authors concerning denitrification being more important in coasral, organic-rich sediments.

Page 4: On the discussion about the giant sulphur bacteria there are two references (Fossing et al and Schulz et al) which are claiming that these bacteria are denitrifying. Otte et al (AEM 1999) and more recently Preissler et al (ISME 2007) demonstrate that these bacteria are reducing the stored nitrate to ammonium (DNRA) and find little or no evidence for denitrification. Ammonium is not lost to the atmosphere which makes a significant different for the role of these bacteria in the marine nitrogen cycle, consequently the authors should take this into account in this section.

Our response: The two references (Otte et al., 1999; Preisler et al., 2007) have been added and the text suitably modified.

Page12: The section on oxygen is problematic and as far as I can see there is no reference or data supporting the 1 micromolar oxygen concentration suggested as a limit for denitrification. Although some work exist on the effect and oxygen-tolerance of denitrifies has been done on sediments (i.e Bonin and Raymond Hydrobiologica 1990), no experimental data has been published on this for denitrification in water samples (to the best of my knowledge). Packard et al (DSR 1983) reported 9 micromolar as the limit for marine denirtifying bacteria which is very similar as recently reported oxygen tolerance of anammox activity (Jensen et al L&O 2007).

Low concentrations of oxygen in oxygen minimum zones is a difficult subject as conventional methods do not work for concentrations below 1-2 micromolar. Nevertheless, 1-2 micromolar oxygen might be very important for the N-cyling (reported N-Loss rates are all in nM/d) and aerobic ammonium oxidation rates has been shown oxygen defi-

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cient waters (Ward et al DSR 1989, Lipschultz et al DSR1990 Lam et al PNAS 2007). Because of the importance of this subject I would suggest the authors to extend the discussion on this subject. This statement cannot be left as it is.

Our response: The 1 micromolar oxygen threshold for denitrification in the oceanic OMZs was proposed by Richards (1971), but it is an obscure publication that most people are not aware of (it has now been cited). While it is true that there exist reports in the literature on the occurrence of denitrification (at least nitrate reduction) in well-oxygenated waters (up to 150 micromolar), this obviously does not happen in the ocean. To our knowledge the secondary nitrite maximum, taken as an indicator of denitrification, has always been found to be confined to sub-micromolar oxygen concentrations whenever oxygen was carefully measured (following the colorimetric method of Broenkow and Cline (L&O, 1969) and, more recently, by automated titrations). One remarkable example of how rigid this threshold is in the open-ocean OMZs comes from the the Bay of Bengal where the minimum oxygen concentration is 2-3 micromolar. Absence of vigorous denitrification in this region is reflected by the absence of a secondary nitrite maximum and much higher (x2) nitrate concentration within the core of the OMZ (200-300 m) as compared to the central Arabian Sea.

However, the referee has raised an important issue and following his advice we have discussed it in some detail also including several new references to support our view.

Response to Review # 2

.. My only overall quibble is that I think that the introductory paragraphs should include some comments on how our knowledge of the oceanic source term for nitrous oxide have also been "revolutionized"; in recent decades. Early thinking on nitrous oxide fluxes from the ocean to the atmosphere concentrated on mid-ocean data where fluxes from the ocean to the atmosphere are modest, but more recent work has shown that there are "hotspots" for nitrous oxide production in the ocean that necessitate upwards revision of the oceanic source term. More importantly, they suggest a nitrous oxide

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system that may be highly sensitive to anthropogenic impingement. Naqvi is a major contributor to this new knowledge, and perhaps he was too modest to include this topic. I personally think that it is vital to better understand whether unprecedented forcing(s) introduced by civilization will lead to a situation in which nitrous oxide may play a greater role in future climate change than might be inferred from ice-core data. Therefore, I think that the introductory paragraphs of this paper should pay some attention to our changing understanding of the oceanic nitrous oxide regime.

Our response: We thank Dr. Codispoti for his very generous comments, and accepting his suggestion, we added text in the introductory paragraphs briefly highlighting the role of coastal zones in marine nitrous oxide cycling and how it may be impacted by human activities.

Because of the elapsed time between submission of the earliest papers to the SPOT-ON volume and this paper that summarizes all of the submissions, I am a little confused by the years assigned to some of the references. This paper has a 2008, date, many of the SPOT-ON papers have a 2007 data, and I think that there are a couple with a 2006 date. Because of BGD's methods for handling papers that include heavy reliance on electronic publication, I am not sure if these dates should be harmonized or left as they are. Anyhow, someone might want to double check to make sure that the year assigned to each reference is correct. I like the BGD system, but I don't yet fully understand how all of the pieces fit together.

Our response: We agree. Unfortunately, the publication schedule of the special issue was long. But this is how the journal operates, which in a way is a good thing, because a publication is not held up just because others are not ready. Some of the authors took longer than others in delivering their manuscripts. This "Summary" was prepared after all manuscripts had been accepted; it spilled into 2008 largely because we were under the wrong impression that it did not have to be peer reviewed.

Specific Comments:

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Bottom of page 1121 and top of page 1122: It is curious to me that papers that suggest a role for dinitrogen production in suboxic water columns by conventional denitrifiers are not mentioned. The work of Jayakumar, Ward and colleagues on Nir comes to mind,and, if memory serves, there was a talk at the SPOT-ON meetings that suggested dinitrogen production in Chilean waters by both anammox and conventional denitrifiers.

Our response: The work of Jayakumar and coworkers was cited, as was that by Thamdrup and colleagues (who did observe denitrificaton off Chile, as rightly pointed out by the referee), but maybe we failed to convey the right message. We have made amends in the revised version.

Page 1127, lines 3-6: OMZ means oxygen minimum zone in general, but it seems like the authors are actually speaking about suboxic OMZs in this section. I know what they are trying to say, but I think that the wording could be a bit more precise.

Our response: Accepted; "OMZs" changed to "suboxic zones".

Line 15: What is "this issue"? Do the authors mean this issue of BGD, or the topic under discussion?

Our response: We meant the special issue of BGS. Clarified.

Page 1128, lines 1-5: I find the wording of this paragraph a little clumsy. "Some of the findings were unexpected"; what findings are being referred to? If it is only that nitrous oxide concentrations were lower than might be expected, then this phrase is redundant. Also, I am not sure that the differences can be attributed to different pathways of production. I am more inclined to think that there are differing balances between production and consumption rates. For example, one might speculate that in the Baltic and Black Seas there is more frequent contact between high nitrous oxide waters and anoxic waters and sediments where nitrous oxide is consumed. What about the E. Indian Shelf? Here, one might be dealing with exceptionally high production rates that at least temporarily overcome the close proximity of anoxic waters and sediments. I don't

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pretend to be able to solve this problem. I am merely pointing out that the invocation of different pathways might be a trifle glib at this stage of our understanding.

Our response: Accepted; text suitably modified.

Line 7: I would have chosen a slightly higher value for the threshold oxygen concentration, but this is a debatable point.

Our response: This issue has now been addressed in detail (Please see response to the other review).

Editorial Comments: Page 1122, line 24: strike "for" before "terrestrial". Page 1125, line 4: strike "," after "SPOT_ON". Line15: strike "the" before "anthropogenic". Page 1126, lines 1-2: Change "These C:N uptake.." to "The C:N ratios of these diazatrophs are higher than Redfield..". Line 5: strike "the" before "larger". Line 13: strike "other" before "unidentified". Page 1129, line 2: Add "the" before "N cycle". Line 6: Strike "the" before "diazotrophs". Line 12: Change "US coast" to "US East Coast".

Our response: All accepted. Thank you very much, Lou.

Interactive comment on Biogeosciences Discuss., 5, 1119, 2008.