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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.

Anonymous Referee #2

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Review of: Recent advances in the biogeochemistry of nitrogen in the ocean

By: S.W.A. Naqvi, M. Voss and J.P. Montoya

General Comments:

This paper is well-written and does a superior job of summarizing the discussions at the 2005 SPOT-ON (Significant Processes, Observations and Transformations in Oceanic Nitrogen) meeting, the papers submitted to the BGD volume arising from this meeting, and several related papers. 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

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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 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.

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.

Specific Comments:

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.

Page 1127, lines 3-6: OMZ means oxygen minimum zone in general, but it seems like

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

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

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".

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Summary Comments: This paper requires only minor revision, and the authors deserve our thanks for taking leadership roles in the SPOT-ON conference and in production of the associated BGD volume.

Respectfully Submitted: Louis A. Codispoti

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