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Biogeosciences
Discussions

Interactive comment on "An oceanic fixed nitrogen sink exceeding 400 Tg N a^{-1} vs the concept of homeostasis in the fixed-nitrogen inventory" *by* L. A. Codispoti

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GENERAL COMMENTS

I had seen a pre-submission version of this manuscript and had passed on my comments to the author, most of which have been considered by him. As such, I have only a few minor comments to offer here.

In my opinion this manuscript is a significant improvement over Codispoti et al. (2001) that made similar arguments (there are some additional ones here) for a high rate of denitrification in today's ocean. Like the earlier paper, this manuscript is also expected to generate lively discussion (the two other reviews already provide sufficient indication of that) on whether or not the oceanic combined nitrogen budget is in a steady state. Unfortunately, this issue is not likely to be settled quickly. But I think a steady state in unlikely to be maintained on a decadal time scale. Given the sharp oxygen threshold for denitrification, minor changes in the oxygen distribution in and around the oceanic OMZs can lead to changes in water column denitrification rates on the order of 10's of Tg/y. An excellent example of this exists in the Indian Ocean. The minimum oxygen concentration in the Bay of Bengal is higher than the corresponding value in the Arabian Sea by no more than ˜2 micromole/litre; yet, due to the absence of denitrification, nitrate concentration within the core of OMZ in the Bay of Bengal is up to 2x that in the Arabian Sea. Thus, in the Bay of Bengal a decrease in the mesopelagic oxygen concentration that would be within the the precision of the Winkler procedure could potentially result in an increase in oceanic denitrification rate by an amount comparable to the rate in any one of the existing open-ocean suboxic zones. To what extent and how quickly these changes can be compensated by nitrogen fixation is not known. However, it would seem quite likely - as Codispoti points out here - that water column denitrification rates in today's ocean could already have been impacted (enhanced?) due to human activities. Should this be the case, then the isotopic composition of deep-water nitrate would not have been affected yet, and the conversion of current water column estimate to compute sedimentary denitrification using the sedimentary-to-water-column rate ratio would not be proper. Even otherwise, as has been pointed out, there exists considerable uncertainty regarding the use of this ratio because, among other things, we do not know the fractionation involved with the other processes (mainly anammox) involved in elemental nitrogen production.

As pointed out in the other two reviews Codispoti's arguments (both here as well as in the 2001 paper) for an upward revision of the water column denitrification rate are to a considerable extent based on the nitrogen/argon data of Devol et al. (in press) from the Arabian Sea. Devol (personal communication) has since observed similar discrepancies between the N2/Ar-derived "excess nitrogen" and stoichiometrically-computed nitrate deficits off Peru as well. So these measurements do call for a re-evaluation of the extent of oceanic N2 production. I also point out two other aspects of the N2/Ar data that Codispoti might like to mention. First, the maximum in excess N2 exactly coincides with the nitrite/nitrate deficit maxima and nitrate minimum, which indicates that these anomalies are generated in the water column rather than in marginal sediments. It is extremely unlikely that the maximal sedimentary signal occurs at the same depth as the depth of maximal nitrate reduction in the water column, and the same also applies to the mixing argument (i.e., mixing of waters with different temperatures causing N2 anomalies). Secondly, the N2/Ar data suggest a secondary increase in excess N2 within the lower oxycline where Li et al. (2005) found "nitrate loss due to partial nitrification". The exact mechanism of this loss is not clear, and this feature deserves to be investigated in greater detail.

In conclusion, I do agree at least qualitatively with the assertion that the rate of water column denitrification has been probably underestimated so far. I am not sure how accurately this information can be used to constrain sedimentary denitrification rate. Obviously, the uncertainties with all such estimates are considerable and some quantification of these uncertainties will be in order. Overall, I recommend the publication of the paper with minor revisions.

SPECIFIC COMMENTS

Page 1206, line 19: Add “2” before “H2O” in anammox reaction.

Page 1207, line 24: The orbital time scale also includes glacial-interglacial (delete “orbital”?).

Page 1210, line 10 (also page 1217, line 25; and page 1227, line 12): Should it be “Altabet, 2006”?

Page 1231, line 5: Delete “of”?

Page 1240, line 14: Replace “Biogeochem.” By “Biogeosci.”.

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