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Interactive
Comment

Interactive comment on “Constraints on oceanic N balance/imbalance from sedimentary^{15}N records” *by* M. A. Altabet

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First to respond Moritz Lehmann’s thoughtful review:

While I did intend the paper to be synthetic with respect to oceanic 15N and combined nitrogen budgeting, the paper does indeed contain substantial new results. The temporally well-resolved continental margin del15N records from sites that are neither within water column denitrification (WC) or HNLC zones have not been previously published. These data are the first to show likely stable oceanic average del15N over the late Holocene (more about this below).

Lehmann makes a good point that a lowered effective WC N isotope effect implies not only a lower sediment denitrification (SED) to WC ratio but also a lower overall estimated rate for total oceanic denitrification as compared to Codispoti et al. (2001)

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and Codispoti in this volume. This follows logically since their increased estimate for SED is dependent of the SED/WC derived from 15N budget considerations. I will amend the manuscript where appropriate in this regard.

The next comment concerns highlighting the limitations of the 15N budget approach. I believe my analysis shows both its potential as well as its limitations including sensitivity to the kinds and degree of variation in N cycle components. Fig. 2 gives a good sense of the sensitivity of oceanic del15N to the fraction of oceanic N2 fixation lost via WC at steady state. Figs. 3 to 5 specifically show the sensitivity to a variety of non-steady conditions. Granted greater certainty in the isotope effects associated with SED and WC (as well as anammox) is critical. I certainly can add to the text the desired level of confidence in these parameters.

I did not mean to state that SED and WC were likely to vary together to maintain the same ratio. The 'fractional removal' I was referring to was the fraction of nitrate in the WC zone that is removed by this process. Lehmann notes my inference that changes in WC intensity could change the degree of nitrate removal in the suboxic regions where this process takes place and thus the effective isotope effect. I agree that recognition of the likeliest scenarios await detailed numerical modeling of WC zones.

Regarding Lehmann's last comment, it seems that how one judges the usefulness of a global marine del15N record is a matter of whether one views 'the glass half full or half empty'. As noted, I have made clear the points of ambiguity but nevertheless show how such records provide important and unique constraints on our understanding of past changes that when combined with other paleo-records are likely to prove conclusive. The alternative explanation to the stable del15N of the last 3 kyr or so is that the WC/N2 fixation ratio was stable but that SED varied. This reduces considerably the degrees of freedom and the number of hypotheses to be considered further. I am not clear why Lehmann believes that identification of the global del15N signal is not achievable. A major point of the paper is laying down the roadmap for how to do so.

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Regarding anonymous Referee#1's comments:

Given the topics discussed at the SPOT-ON meeting, Lehmann's review of my paper, the discussion of Codispoti's paper in this volume, as well as the current literature, I believe the issues raised in my paper are indeed quite topical and not 'straw-men'. Also, this review contains incorrect statements regarding prior work as well as this paper.

Addressing specific points:

Codispoti in this issue continues to conclude that the present-day budget is in deficit. This is not an inference on my part. I never argued that the loss terms estimated for today by Codispoti have to be lowered, I concluded instead that for the latest Holocene the oceanic N budget had to on average be balanced. I can add to my conclusions, that the late Holocene records are not likely to be sensitive to any recent anthropogenic changes. We have observed, though, large centennial to decadal scale changes in Peru del15N over the last 3 kyr that do suggest recent variability in Peru denitrification observed by Codispoti and colleagues could be naturally forced. I am not aware that there is community consensus that stable Holocene CO2 required a stable oceanic N budget, it would have been helpful to list the papers mentioned.

The Brandes and Devol (2002) conclusion of a SED/WC of ˜3.5 (also see Sarmiento's discussion of this in his review of Codispoti, this volume) is clearly the result of using the full microbial isotope fraction effect of 25 given a del15N of -2L' for N2 fixation and average modern del15N of 5L' for oceanic N (25/[5 - -2] = 3.6). The reviewer seems to be confusing this paper with Brandes et al. (1998) which examined different approaches for calculation of the inherent fractionation factor from water column del15?O3 data. I thus have engaged in no 'double counting'..

The point made regarding an ammonium contribution to denitrification-produced N2 illustrates the confusion behind this reviewer's arguments. The critical issue for the oceanic N isotopic budget is the average del15N of N2 produced in association with water column denitrification. Yes, conversion of NH4+ to N2 as part of the overall denitrification stoichiometry does not directly affect the ?15N of remaining NO3- in the denitrification zone. That is why its significance has been hidden to us despite significant influence on the del15N of total N2 produced. As explained in the paper, the overall isotopic balance, though, does determine of average oceanic del15?O3 , which is the signal I sought after in the core data presented.

The comments regarding Codispot's flux estimates and del15?O3 are not relevant since the relative drawdown in the suboxic zone determines the local del15N signal whereas the global signal is determined by the N2 fixation/WC ratio. The next comment regarding 'compression' would have meaning if the reviewer tried to show that realistic variations in oceanic del15N were small compared to our ability to detect them. Again I refer to my sensitivity analyses mentioned above.

The point about the ETSP and sulfide is not clear. Is the inherent microbial fractionation factor, meant? If so, there is no evidence for this. I believe instead he/she refers to the influence of the degree of NO3- removal on the del15N of N2 produced which is certainly relevant for all three denitrification zones as discussed above. My only guess about what is meant by the reference to sulfide, is that its presence signifies the development of fully anoxic conditions after the exhaustion of NO3- by denitrification. But this condition does not exist in any of the WC regions where the maximum removal of NO3- is about 50%. I also dispute that there is any consensus, growing or otherwise of the global importance of N2 fixation in proximity of suboxic zones. It is certainly an important issue worthy of research but its impact of the N budget is far from established.

Finally, this reviewer appears to know my state of mind when I was writing this paper. In reality, I do not hold as dogma any particular partitioning between WC and SED. What I did attempt to do was to take a more sophisticated and honest approach in using the isotopic balance arguments on which the estimate of WC/SED of 3.5 is based. The results are not contrived and the reviewer should recognize that I have let the 'chips fall as they may'. I do not argue that the ratio has to be 1, but that it is closer to 1 than 3.5 and I expect further refinements in the future. Critique my analysis not my 'presumed'

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intention.

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Interactive comment on Biogeosciences Discuss., 3, 1121, 2006.