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Biogeosciences Discussions

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

Anonymous Referee #1

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In general this is a well written paper, however, the Author sets up a number of straw men and proceeds to knock them down, when in fact a careful consideration of the literature requires no such effort. I would suggest a reconsideration of the Author's model parameters and subsequent results is in order.

Straw man # 1: Codispoti's budgets require a budget out of balance. This is incorrect. What his papers imply is that we have underestimated the amount of N2 fixation taking place. He does not imply that the fixed N budget has been out of balance for the Holocene, and in fact there are a number of papers that make good arguments based on CO2 balances that the oceanic nutrient budget has been balanced over the last 10,000 years or so. This does not preclude shorter term imbalances, and certainly this is a valid subject that is well tackled in the manuscript. But the need to somehow

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balance the fixed N budget by adjusting the present loss numbers downward is simply not justified. It is also worth noting that Codispoti clearly states that several of the budget terms have been altered by human activities.

Straw man # 2 Brandes and Devol assume full fractionation is effected during water column denitrification. If one reads said paper B and D use both a Rayleigh model and an advection-diffusion model, both of which fully account for the increases in 15N values of nitrate within suboxic zones. The values they report for fractionation in suboxic waters of the Arabian Sea and ETNP are therefore already adjusted for this phenomena. What this manuscript does is engage in a type of “double counting” whereby the reduction of effective fractionation factors during water column denitrification is magnified in an unjustified manner. One cannot apply both a Rayleigh model (which assumes a closed system) AND an advection-diffusion model (which is, by definition, partially open) to the same water column! The effect of setting the effective fractionation factor back to 20 per mil may be to increase the signature of changes in water column denitrification in the Author’s models.

The point about the isotopic influence of ammonium released in the suboxic region may be valid, but only if said ammonium is oxidized to nitrate (a rather suspect occurrence within a suboxic zone!) If instead the ammonium released by organic matter decomposition is converted to N2 via Anammox then no influence will be exerted on the marine nitrate pool. This is an important and often misunderstood point. All paleo-proxies for 15N are measuring the local composition of nitrate 15N, not total 15N of losses! The Codispoti et al paper (2001), where a value of water column “denitrification” is given as 150 Tg N per year, correctly notes that only about half of this figure is attributable to canonical denitrification using nitrate. It is this value, and this value alone, that affects the isotopic composition of nitrate in the oceanic pool, unless one wants to make assumptions about ammonium oxidation as noted above. Note that this value of 75 Tg N per year for canonical denitrification has remained roughly the same for the past 2 decades.

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It is worth observing that, while the isotopic composition of marine fixation is invariant (at least, so far as we know) the isotopic composition of loss terms is highly dependant on the isotopic composition of average marine nitrate. The end members, therefore, of oceanic nitrate composition are around 0-2 per mil, for no water column denitrification (Given that there is some small sedimentary denitrification fractionation) and +20 per mil, for only water column denitrification. Of course, this latter end member is a fiction as no natural analogue can be imagined for this. Thus, the marine nitrate response is "compressed" in one direction but not the other.

This having been said, there is one suboxic region where Deutsch's et al, 2004 point about the effective fractionation factor being lower at high denitrification levels is certainly true, and that is the ETSP. This is a region where sulfide production has been seen in the water column. So one can certainly make the assumption that this portion of the suboxic ocean will have a smaller denitrification fractionation factor than the other regions. But please also note that such fractionation factor will only have been larger during the Glacial periods, where denitrification intensity was less in all the three suboxic regions. The Author well knows this!

Another point, that needs to be made, is that there is a growing consensus that there exists enhanced N2 fixation on the edges of suboxic regions. This will have the effect of masking or diluting the local and regional effects of suboxic water column denitrification.

Straw man # 3 Water column and sedimentary denitrification sinks have long been thought to be "equal" (page 1128, line 15). Where would we be if we always made the assumption that older studies were infallible? Frankly the idea that the two sinks are equal was disproven around the time of Devol's 1990 paper showing the effect of coupled nitrification-denitrification, which doubled sedimentary denitrification fluxes over those measured by nitrate fluxes alone. And if that was not the only nail in the coffin, the discovery of Anammox and related processes certainly was another. I know of no-one in the scientific community who honestly would say the two sinks are equal

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now. So there is no need to perform mathematical gymnastics to try and get the two values to be equal. Let the “chips fall where they may”.

Note on

1130, line 9 “produce” instead of “produces”

1136 line 14. Change to “must be attenuated through negative feedbacksĚ”

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