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Biogeosciences Discussions

Interactive comment on "An eddy-stimulated hotspot for fixed nitrogen-loss from the Peru oxygen minimum zone" *by* M. A. Altabet et al.

Anonymous Referee #2

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In general, this is a well-written paper describing an interesting phenomenon. However, from the data provided, it's not clear how quantitatively important this phenomenon is for fixed N loss in oxygen minimum zones. Extension to other OMZs is purely speculative. I had a few comments and requests for clarification.

p. 8015, line 29- 8015, line 2: "isotopically enriched" and "isotopically depleted" should be revised to "enriched in the heavy isotopes" or "depleted in the heavy isotopes". Also "isotopically light" should be rephrased.

p. 8017: Please indicate how isotopic analyses were normalized to international reference scales, i.e., what standards were used. Also they should further describe or cite a reference that documents the effectiveness of NO2- removal by sulfanilic acid.

p. 8021: It seems surprising that the N2 excess would be twice what is expected from DIN measurements! The authors should further explore all avenues of uncertainty in their measurements and calculations and offer more of an explanation for the apparent imbalance. Is this non-Redfield organic matter being remineralized? Does this represent communication with sediments? Data from nearby stations should offer a distinction between these alternative possibilities. I'd like to see the authors explore the legitimacy of this surprising finding a bit more.

p. 8024, lines 19-20: I don't see why remineralization of OM from the eddy would lead to greater loss of fixed N if the eddy is transported offshore, relative to its retention onshore, as the suboxic source water is presumably carried offshore with the eddy.

In general, station 7 does not seem special in terms of the chlorophyll or circulation pattern (Figure 5)âĂŤwhy would this area apparently be so special in terms of N dynamics? Shouldn't other stations sampled during this cruise (Figure 1) provide a view as to the past or future state of the eddy, or similar features? Is the large peak in excess N2 found in any of the nearby stations? If this phenomenon was very important, you would expect to see large excesses of N2 at other stations after the eddies have dispersed, given that there is no sink for the excess N2 produced.

This excess N2 distribution looks strikingly like the record from Devol et al (2006) in the Arabian Sea, and unlike those found by Chang et al (2010) in the ETSP. What explanation can be offered for the N2 excess not being associated with the maxima in δ15N-NO3- and NO3- deficit? Does it make sense that it would occur in waters that are more oxygenated compared to surrounding waters? I think that the authors should include O2 concentration profiles in Figure 2, and offer some explanation for the apparent discrepancy of extreme N loss at relatively high oxygen concentrations.

p. 8027, lines 12-13: I think this overestimation of NO3- removal by N' needs clarification. As discussed by Devol et al (2006), the issue may be more clearly understood as the fate of NH4+ produced from organic matter breakdown. Either way, the way I

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understand it, N' should be an underestimate of N removal, not an overestimate. I think what they are doing here is correct, but their explanation should be revised/clarified.

p. 8028, lines 6-7: I think they must be referring to Figure 4B here, not 3B.

p. 8028, equation A5: the [N2atm] term should be multiplied by an isotopic value (δ15Natm, or the air/sea equilibrated value) for the units to make sense.

Interactive comment on Biogeosciences Discuss., 9, 8013, 2012.