

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

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First we thank the anonymous referee for his/her thoughtful comments.

Regarding his/her first comment that “it’s not clear how quantitatively important this phenomenon is for fixed N loss in oxygen minimum zones”, we grant that this requires a much more extensive study of OMZ eddies but our observation of an eddy N-loss hotspot now provides the motivation for such a study. In the interim, we believe that our discussion of eddy statistics in the Peru and other OMZ’s makes clear the likely generality of the phenomenon.

Below sections of the referee’s comments are followed by our response.

Referee#2: p. 8015, line 29- 8015, line 2: “isotopically enriched” and “isotopically

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depleted” should be revised to “enriched in the heavy isotopes” or “depleted in the heavy isotopes”. Also “isotopically light” should be rephrased.

Authors: We believe that our usage is acceptable but can change to “ ^{15}N enriched”, etc.

Referee#2: 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 NO_2 - removal by sulfanilic acid.

Authors: We will add these methodological details to the revised text. We did use NO_3 - isotopic reference materials USGS 34, USGS 35 and IAEA N3.

Referee#2: p. 8021: It seems surprising that the N_2 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.

Authors: As noted in the text, this is only observed above the OMZ. Within the OMZ the agreement between observed biogenic N_2 and that expected from the NO_3 - deficit and Richards stoichiometry is remarkably good as seen in Figure 4. We will add text comparing measurement uncertainty and the observed signal. We did discuss reasons for the deviations above the OMZ but will expand for clarity. From our other related work, we believe such deviations are only observed in proximity to the shelf

Referee#2: 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

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eddy.

Authors: The high chlorophyll eddy streamers originating on the shelf are in limited in depth to the shelf depth. Once offshore, high OM fluxes can influence the deep OMZ that the eddy streamer has flowed over.

Referee#2: In general, station 7 does not seem special in terms of the chlorophyll or circulation pattern (Figure 5) why 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?

Authors: We are not clear what the referee means here since we show the distinct subsurface biogeochemistry of Station 7 relative to nearby stations and its proximity to the eddy filament. In addition to the isopycnal maps in Fig 1 and the sections in Fig. 3, we can add profiles from a nearby station (Station 9) to Fig. 2 (see accompanying figure) to further highlight the contrast. We also note that local high chlorophyll is not necessarily expected locally at Station 7 since high OM flux would be the result of a collapsed phytoplankton bloom, though we can expand further. We also point out that the study was not designed to study individual eddies but was an unexpected discovery.

Referee#2: 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.

Authors: Again, the study was not designed to study eddies so the station map is not suited to observe the generation and evolution of N-loss hotspots. The lower NO₃-deficit and biogenic N₂ at nearby stations could very well be the result of eddy dispersion.

Referee#2: 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.

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What explanation can be offered for the N₂ excess not being associated with the maxima in $\delta^{15}\text{N-NO}_3^-$ and NO_3^- 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.

Authors: Also not clear what the Referee's point here is. Except for the points above the OMZ, the data do look like Chang's et al. with respect to stoichiometry. The points above the OMZ have been interpreted in the text and above as originating from shelf process. We can add an O₂ profile to Fig. 2 to make this clearer.

Referee#2: p. 8027, lines 12-13: I think this overestimation of NO_3^- removal by N' needs clarification. As discussed by Devol et al (2006), the issue may be more clearly understood as the fate of NH_4^+ produced from organic matter breakdown. Either way, the way I 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.

Authors: We will revise and clarify.

Referee#2: p. 8028, lines 6-7: I think they must be referring to Figure 4B here, not 3B. p. 8028, equation A5: the $[\text{N}_2\text{atm}]$ term should be multiplied by an isotopic value ($\delta^{15}\text{N}_{\text{atm}}$, or the air/sea equilibrated value) for the units to make sense.

Authors: Thank you for catching these errors. For eq. A5, it is a missing isotope value after the first 'N₂ atm'.

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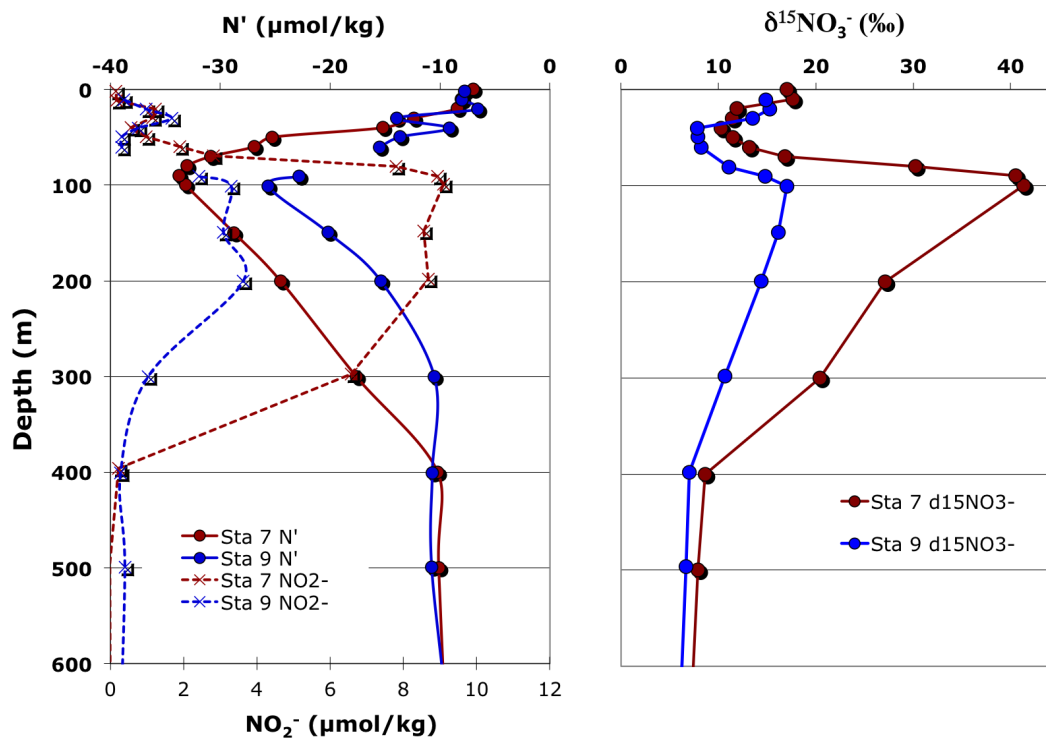


Fig. 1. Comparison of property profiles between the N-loss hotspot at Station 7 and nearby shoreward Station 9

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