

***Interactive comment on* “Nitrogen cycling in shallow low oxygen coastal waters off Peru from nitrite and nitrate nitrogen and oxygen isotopes” by H. Hu et al.**

H. Hu et al.

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General comments: Hu et al. examine the N- and O isotopic signatures of nitrite, nitrate, and biogenic N₂ to assess the importance of various N-cycling processes in the Peruvian coastal OMZ. They provide a fairly comprehensive set of isotopic data, including measurements of ¹⁵N on N₂, a relatively novel approach in deriving water column N-loss estimates. And, while I would like to see those data published, in its current state the manuscript does not clearly convey the main goals, outcomes, and implications of the study. Particularly the introduction and the discussion section are

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not very concise and lack structure that guides the (non-expert) reader through the manuscript. Various parts of the introduction do not connect very well and contain unnecessary details that distract from the broader framework, in which the study should be viewed. In the discussion part, the authors' conclusions are not well integrated into the presentation of prior work. What I am particularly missing is a more thorough discussion of the here presented results in light of recent, extensive rate measurements of N-cycling processes in the Peruvian OMZ. Also, a number of statements/conclusions are not backed up by references or are highly speculative and not supported by the data shown.

Our response: We thank reviewer #2 for his/her helpful comments. We improved the introduction and discussion sections, making these more concise and structured. We also added a comparison with rate measurements of N-cycling processes in the Peru OMZ, from Lam et al. (2009) and Kalvelage et al. (2013). We generally addressed all other concerns below.

Specific comments:

Abstract

The abstract is missing any statement on the (novel) implications of your study.

Our response: We stress that we observed for the first time, a positive linear relationship between NO_2^- $\delta^{15}\text{N}$ and $\delta^{18}\text{O}$ at our coastal stations. This is a novel finding, implying fast NO_2^- cycling in coastal waters compared to offshore.

Page 7258 – line 4f. All oceanic OMZs subject to water column N-loss are coupled to regions of high productivity. Please rephrase.

Our response: We rephrased for: “OMZs are generally coupled to regions of high productivity leading to high rates of N-loss as found in the coastal upwelling region off Peru”

Page 7258 – line 13f. Do you mean nitrate assimilation (uptake)? Assimilatory re-

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duction would refer to the cell internal reduction for N-incorporation into biomolecules following nitrate uptake.

Our response: We changed assimilatory reduction for NO₃⁻ uptake

Introduction

Page 7259 – line 2 Please change “Chemically combined nitrogen (N, e.g. NO₃⁻)” to: “Chemically combined nitrogen (N), e.g. NO₃⁻,”

Our response: Done.

Page 7259 – line 10ff. Please specify below which oxygen level nitrate respiration can be expected and provide some reference(s).

Our response: We changed the sentence for: “N-loss typically occurs under nearly anoxic conditions where the first step, dissimilatory NO₃⁻ reduction to NO₂⁻, active at O₂ concentrations less than ~25 μM (Kalvelage et al., 2011), is used by heterotrophic microbes in lieu of oxygen (O₂) for respiration.”

Page 7259 – line 12ff. It currently reads: “successive reduction of NO₃⁻, ... and finally N₂”. Please rephrase.

Our response: We changed for: “successive reduction of NO₃⁻, NO₂⁻, nitric oxide (NO), and nitrous oxide (N₂O) to N₂”

Page 7259 – line 14ff. Maybe you could find a more suitable term for “considerable evidence”, which suggests that anammox still awaits final proof to actually occur in the oceans. Same sentence: Which are those “other pathways for N-loss” besides anammox and denitrification? Further, Lam et al. (2009) is not an appropriate reference here. The N-loss rates presented in the former study were originally published in Hamersley et al. (2007).

Our response: We changed the sentence for: “However, in the early 2000s, anaerobic ammonium oxidation (anammox: NO₂⁻ + NH₄⁺ → N₂) was discovered to be found to

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be widespread in the ocean (Kuypers et al. 2003; 2005; Hamersley et al., 2007).”

Page 7259 – line 16ff. You should probably also refer to recent studies by Dalsgaard et al. (2012) and Kalvelage et al. (2013) examining the large-scale distribution of N-loss processes in the ETSP OMZ.

Our response: We added these references.

Page 7259 – line 18ff. These two sentences remain fairly vague. Please rephrase and provide references.

Our response: We rephrased for: “While it is still a matter of debate whether denitrification or anamox is the dominant pathways for N-loss in Oxygen Minimum Zones (OMZ’s) (e.g., Lam et al., 2009; Ward et al., 2009), both N-loss processes have been shown to strongly vary spatially and temporally and are linked to organic matter export and composition (Kalvelage et al., 2013; Babbín et al., 2014).”

Page 7259 – line 23-26 Again, there are no references provided here.

Our response: We added the following references: “Marine N-loss to N₂ occurs predominately in reducing sediments and the O₂ deficient water columns of OMZ’s as found in the Arabian Sea and Eastern Tropical North and South Pacific (Ulloa et al., 2012; Lam et al., 2011 and references therein). NO₂⁻ is an important intermediate during N-loss and generally accumulates at concentrations up to ~10 μM in these regions (Codispoti et al., 1986; Casciotti et al., 2013).”

Page 7260 – line 4f. This is not well phrased. N- and O-isotopes are not useful because of their reaction rate but because they can provide information on the time-integrated activity of N-cycling processes.

Our response: We accordingly changed the text for: “NO₃⁻ and NO₂⁻ N and O isotopes represent a useful tool to study N cycle transformations as they respond to in situ processes and integrate over their characteristic time and space scales.”

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Page 7260 – line 17-28 This section needs some clarification. Particularly for the non-specialist reader, the underlying cause for the difference in $18\epsilon:15\epsilon$ between nitrate consumption and nitrification is not well explained (e.g. there is no mentioning of N-isotope fractionation during nitrification).

Our response: We added this sentence at the end of the paragraph (and modified the next paragraph accordingly): “NO₂- oxidation is associated with an inverse N isotope effect (Casciotti, 2009), atypical of biogeochemical reactions, and can cause both lower and higher ratios for $18\epsilon:15\epsilon$ in pure NO₃- assimilation or denitrification, depending on the initial isotopic compositions of the NO₂- and NO₃- and the ¹⁸O added back (Casciotti et al., 2013).”

Page 7261 – line 4 Please add “during denitrification” after “NO₂- reduction”, as there is also NO₂- reduction to NH₄⁺.”

Our response: Done.

Page 7261 – line 6 “NO₂- O” looks odd. Maybe change to: “O-isotope exchange of NO₂- with water”

Our response: Done.

Page 7261 – line 10ff. These numbers don't seem quite up to date. E.g. compare with Bianchi et al. (2009), who list a number of (more balanced) oceanic N-budget estimates.

Our response: This was also a concern raised by reviewer #1. We accordingly changed the text for: “Current estimates from direct observations and models for N₂ fixation, considered the primary marine N source, range from 110-330 Tg N yr⁻¹ (Brandes and Devol, 2002; Gruber, 2004; Deutsch et al., 2007; Eugster and Gruber, 2012; Großkopf et al., 2012). Estimates for major marine N-sinks, i.e., denitrification and anammox in the water-column of oxygen deficient zones and sediments account for 145-450 Tg N yr⁻¹ (Gruber, 2004; Codispoti, 2007; DeVries et al., 2012; Eugster and Gruber, 2012).”

Page 7261 – line 16 Maybe change “N-loss” to “denitrification”, which back then was considered the sole N-loss process.

Our response: Done

Page 7261 – line 17 Please provide some numbers for the accepted range of ε during denitrification.

Our response: We now provide a range and changed the text for: “Liu (1979) was first to suggest a lower ε for denitrification in the Peru OMZ as compared to the subsequently accepted canonical range for NO₃- reduction of 20‰ to 30‰ (Brandes et al., 1998; Voss et al., 2001; Granger et al., 2008).”

Page 7261 – line 24ff. Results and conclusions don’t belong here. Please remove.

Our response: We removed the results and conclusions from this section: “To investigate further, we present here N and O isotope data for NO₂- and NO₃- from shallow coastal waters near Callao, off the coast of Peru. These waters are highly productive as a consequence of active upwelling that is also responsible for shoaling of the oxycline. We determine the relationship between NO₂- $\delta^{15}\text{N}$ and $\delta^{18}\text{O}$ and its implication for NO₂- cycling in these shallow waters as compared to offshore stations. We finally derive isotope effects for N-loss and infer the likely influence of sedimentary N-loss, which incurs a highly suppressed isotope effect, at our relatively shallow sites.

Material and Methods

Page 7262 – line 10ff. Please delete unnecessary information that are anyways found in the acknowledgements.

Our response: Done. The sentence now reads: “The R/V Meteor 91 research cruise (M91) to the eastern tropical South Pacific Ocean off Peru in December 2012 was part of the SOPRAN program and the German SFB 754 project.”

Page 7262 – line 16ff. Please move to results section.

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Our response: We moved this sentence to the results section 3.1: “During the study period, there was active coastal upwelling as seen by relatively low satellite sea surface temperature and higher chlorophyll α concentrations along the shore (Fig. 1).”

Page 7263 – line 11f. Please delete “resulting in a final concentration...”. That is unnecessary information.

Our response: Done.

Page 7263 – line 12f. & line 20 Please provide $\delta^{15}\text{N}/\delta^{18}\text{O}$ values for nitrite and nitrate isotope standards.

Our response: We changed the text accordingly. Line 12: “In-house (i.e., MAA1, $\delta^{15}\text{N} = -60.6\text{‰}$. MAA2, $\delta^{15}\text{N} = 3.9\text{‰}$. Zh1, $\delta^{15}\text{N} = -16.4\text{‰}$) and other laboratory calibration standards (N23, $\delta^{15}\text{N} = 3.7\text{‰}$ and $\delta^{18}\text{O} = 11.4\text{‰}$. N7373, $\delta^{15}\text{N} = -79.6\text{‰}$ and $\delta^{18}\text{O} = 4.5\text{‰}$ and N10219; $\delta^{15}\text{N} = 2.8\text{‰}$ and $\delta^{18}\text{O} = 88.5\text{‰}$. see Casciotti and McIlvin, 2007) were used for NO_2 - $\delta^{15}\text{N}$ and $\delta^{18}\text{O}$ analysis.” Line 20: “Standards for NO_3 - isotope analysis were N3 ($\delta^{15}\text{N} = 4.7\text{‰}$ and $\delta^{18}\text{O} = 25.6\text{‰}$, USGS34 ($\delta^{15}\text{N} = -1.8\text{‰}$ and $\delta^{18}\text{O} = -27.9\text{‰}$ and USGS35 ($\delta^{15}\text{N} = 2.7\text{‰}$ and $\delta^{18}\text{O} = 57.5\text{‰}$ (Casciotti et al., 2007).”

Page 7263 – line 25ff. I don’t think those details on the purge and trap system, e.g. sample run time, are necessary. Please shorten.

Our response: We shortened to: “ N_2O produced by the azide reaction was purged with He from the septum sealed 20 ml vials and trapped, cryofocused and purified prior to transfer to the IRMS.”

Page 7264 – line 11 Please add “samples” after “warming”.

Our response: Done.

Page 7264 – line 23f. Some words seem to be missing here. Please check.

Our response: We changed the sentence for: “The following equations are used for a

closed system (Mariotti et al., 1981):”

Page 7265 – line 4f. There is a large number of abbreviations, symbols, and formulas, which, particularly for the non-expert reader, is challenging enough to remember. Unnecessary abbreviations, such as “[NO₃-]” instead of “NO₃- concentrations”, should thus be avoided. It also assists readability. Please check throughout the manuscript.

Our response: We significantly reduced the number of abbreviations in the text, especially the uses of brackets for concentrations, as suggested. When we use abbreviations, we made sure that they were defined at first use in the abstract and main text.

Page 7266 – line 5f. Please move the sentence to the results/discussion section.

Our response: We moved this sentence to the results section (section 3.4).

Page 7266 – line 10 Please define “N_{pdef}/expected”.

Our response: We defined N_{pexpected} after equation (7): “where N_{pexpected} is the concentration expected based on Redfield stoichiometry.” We also defined N_{def} before equation 8: “This has also been a traditional approach to quantify N-loss in OMZ’s (N deficit, N_{pdef}) by comparing observed DIN concentrations (N_{observed}) to N_{pexpected}:”

Page 7266 – line 22 Noffke et al. (2012), who quantified benthic iron and phosphate fluxes along the Peruvian margin, would be a more suitable reference here than the study by Reed et al. in the Baltic Sea.

Our response: We added Noffke et al. (2012) as a reference and removed Reed et al. (2011).

Results

Page 7276 – line 9ff. From your T/S plots it is not evident that the sampled waters originate from further offshore (there are no offshore data for comparison). Further,

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you suggest that originally all waters are oxygen deplete and only become oxygenated once they reach the surface and there is exchange with the atmosphere. That contradicts your statement that surface currents dominate the shallow coastal waters (any references?). These waters are originally oxic and become oxygen deplete below the euphotic zone (which near the coast can be very shallow due to high phytoplankton densities) as a result of oxic microbial respiration of organic matter. Also, I don't quite agree with your interpretation of the observed north-south temperature increase. Is it not more likely, that sea surface temperatures increase towards st. 67 because of the indentation of the Peruvian coastline at 14°S combined with the sheltering effect of the Paracas National Park peninsula near Pisco, impeding the advection of cold surface waters from the south? In my understanding, upwelling (vertical water mass transport) is very slow compared to lateral advection of (surface) water masses.

Our response: Upwelling along the Peru Coastal Current, with a typical depth of origin of ~200 m is well documented (Penven et al., 2005). Accordingly, if we compare with data for offshore ODZ waters from Bourbonnais et al. (2015), we observe similar T/S signatures between 100 and 200 m depths for these waters, suggesting that they are most likely the source waters. We also believe that the explanation of more intense upwelling at our more northern station is consistent with all our relevant observations.

Accordingly, we added a reference (Penven et al, 2005) and changed the text to: “During the study period, there was active coastal upwelling especially at station 63 as seen by relatively low satellite sea surface temperatures, higher chlorophyll α concentrations, and a shallow oxycline (Fig. 1). A common relationship and narrow range for T and S were found, comparable to T/S signatures for offshore ODZ waters between ~100 and 200 m depths (Bourbonnais et al. (2015), indicating a common source of water upwelling at these inner shelf stations (Fig. 2). This is expected as in these coastal, shallow waters, upwelling of the Peru Coastal Current, with low O₂ and high nutrients play a dominant role (Penven et al., 2005).”

Page 7267 – line 19f. See previous comment

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Our response: See our response above, Page 7276 – line 9.

Page 7267 – line 21ff. You also examine possible effects of nitrite oxidation, an aerobic process (at least an alternative electron acceptor has not been identified, yet, in OMZs), on the isotopic composition of nitrite and nitrate.

Our response: We added the following sentence: “In contrast, NO₂- oxidation, an aerobic process, was shown to occur even at low to non-detectable O₂ (Füßel et al., 2012).”

Page 7268 – line 3 Please specify “CTD deployed O₂ sensors”. A STOX sensor, which can resolve nanomolar changes in oxygen concentration, can be mounted to a CTD rosette system, too.

Our response: We specified that we used a Seabird sensor. We also added information about the O₂ sensor, calibration and detection limit in the material and methods (section 2.1): “O₂ concentrations were determined using a Seabird sensor, calibrated using the Winkler method (precision of 0.45 μmol L⁻¹) with a lower detection limit of 2 μmol L⁻¹.”

Page 7268 – line 8f. I do not think “intense local upwelling” is a likely reason for elevated phosphate and/or silicate levels in the shelf bottom waters. The very high concentrations of ammonium, which typically does not accumulate at such high levels in the OMZ, clearly indicate benthic release.

Our response: We changed the sentence for: “Station 63 had the highest near-bottom concentrations, a likely result of release from the sediments, which is further supported by high near-bottom NH₄⁺ concentrations (up to 4 μM) as compared to the other stations (Fig. 3, B, C, and D).”

Page 7268 – line 13ff. I agree that nitrate depletion indicates N-loss, but you did not measure actual N-removal. Please rephrase accordingly.

Our response: We changed the sentence for: “Across most of our stations, NO₃-

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concentration was $22 \mu\text{M}$ at 20 to 40 m depth but decreased to near zero deeper within the O_2 -depleted zone due to microbially mediated NO_3^- reduction (Fig. 4.A)."

Page 7268 – line 24 According to Fig. 4c, $\delta^{15}\text{N-NO}_3^-$ was $\hat{\text{A}}\hat{\text{L}}\hat{\text{i}}\hat{\text{j}}40 \text{‰}$ at stations 65 + 67. Please check those numbers.

Our response: According to our data, we observed a maximum $\delta^{15}\text{N-NO}_3^-$ of 50‰ at 99 m depth at station 64 and 47‰ at 37 m depth at station 67. In Fig. 4c, the maximum $\delta^{15}\text{N-NO}_3^-$ is clearly higher than 40‰.

Page 7268 – line 25f. Maybe move this to methods section (e.g., "samples down to $x\text{x} \mu\text{M}$ N were analyzed for their isotopic composition").

Our response: We moved this sentence to the method section (section 2.1): "The lowest concentration of NO_2^- or NO_3^- analyzed for isotopic composition was $0.5 \mu\text{M}$, thus $\delta^{15}\text{N-NO}_3^-$ and $\delta^{15}\text{N-NO}_2^-$ could not be measured below 37 m at station 63."

Page 7269 – line 1 Maybe substitute "N-loss" with " NO_3^- reduction", to be more specific.

Our response: Done.

Page 7270 – line 4f. Please provide reference.

Our response: We added a reference here (Bourbonnais et al., 2015).

Page 7270 – line 18f. How does this observation align with actual rate measurements of nitrite oxidation in the Peruvian coastal OMZ?

Our response: We now discuss this better in the discussion (section 4.1). See your comment below, page 7275, line 11.

Page 7271 – line 1ff. In my understanding, N deficit and excess P express exactly the same and are mutually interchangeable. Hence, does "excess PO_4^{3-} " refer to benthic P release here? That is only obvious at station 63, but not at the remaining sampling

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sites. Please clarify.

Our response: We changed the sentence for: “However, the slope of 0.45 for the linear relationship shows biogenic N in N₂ to be only half that expected from N_{pdef}, as a possible consequence of benthic PO₄₃- release.”

Discussion Page 7271 to 7273 – line 19 This is largely a summary of previous studies on the effects of microbial activity on DIN isotopic compositions and is not well tied in with the discussion of the here presented data.

Our response: We think it is essential to first summarize the background information on what affect the isotopic composition of NO₂- in this section. We nonetheless re-organized and condensed the text to have a better flow of ideas, and eliminated unnecessary information and repetitions (see five first paragraphs, section 4.1). See also comments below.

Page 7271 – line 21f. I disagree that micromolar levels of nitrite as found in OMZs are “low concentrations”. Also, later in the same paragraph you write: “Accordingly, relatively high [NO₂-] was observed ...”.

Our response: We changed the sentence for: “can accumulate at relatively high concentrations through the ocean.”

Page 7272 – line 3 There are more fitting references here, e.g. Lipschultz et al. (1990), Lam et al. (2009), and Kalvelage et al. (2013), all of which provide actual rate measurements of nitrate reduction in the Peruvian OMZ.

Our response: We removed Codispoti et al. (1986) and added the suggested references.

Page 7272 – line 3ff. Are you suggesting that the observed nitrite is actually not formed in the shallow shelf waters but originates in deeper OMZ waters and is simply advected (upwelled)? Previous studies have shown that nitrite is produced at those shallow, oxygen depleted depths (see also above).

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Our response: We removed this sentence.

Page 7272 – line 22ff. This needs better explanation and should be discussed in the context of the preceding sections. By describing observations made in previous studies and your own ones in separate sections, the non-expert reader has a hard time to follow your line of argumentation here.

Our response: We accordingly reorganized the text in this section, see five first paragraphs, section 4.1.

Page 7273 – line 25ff. Although high rates of N-loss have been measured on the Peruvian shelf that alone does not explain rapid nitrite turnover. The nitrite pool in these waters is affected by aerobic ammonium and nitrite oxidation, nitrate reduction to nitrite, as well as further reduction to either ammonium or N₂ (see Lam et al. (2009) and Kalvelage et al. (2013)).

Our response: We changed this sentence for: “Higher rates for aerobic NH₄⁺ and NO₂⁻ oxidation, as well as anaerobic NO₃⁻ reduction to NO₂⁻, and further reduction to NH₄⁺ (DNRA) or N₂, have been reported in shallow waters off Peru presumably due to increased coastal primary production and organic matter supply to the in-shore OMZ (e.g. Codispoti et al., 1986; Lam et al., 2011; Kalvelage et al., 2013). However as our observations are restricted to anoxic waters, only high rates of N-loss could explain this more rapid NO₂⁻ turnover.”

Page 7274 – line 14ff. Are these rates calculated based on your own data or do you refer to previously results?

Our response: We refer to the maximum [NO₂⁻] observed in our study (i.e., our own data). We clarified this in the text.

Page 7275 – line 11ff. I would like to see a more thorough discussion of how the estimates of nitrite oxidation vs. nitrate reduction compare to previous rate measurements of these processes. Anammox bacteria (in culture) only oxidize a minor fraction

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of nitrite to nitrate. At the same time, rates of nitrite oxidation mostly exceed those of N-loss via anammox several fold on the Peruvian shelf (Kalvelage et al. (2013)), clearly indicating non-anammox related nitrite oxidation.

Our response: We agree and now added this discussion following line 19 (Page 7275): “Furthermore, anammox bacteria only oxidize a minor fraction of NO_2^- to NO_3^- in culture. At the same time, estimates of NO_2^- oxidation (8.48 to 928 nmol N l⁻¹ d⁻¹) are significantly higher than N-loss rates by anammox (2.84 to 227 nmol N l⁻¹ d⁻¹) on the Peruvian shelf (Kalvelage et al., 2013), clearly indicating non-anammox related nitrite oxidation.”

Page 7275 – line 21 see comment Page 7258 – line 13f.

Our response: We changed for “ NO_3^- uptake”.

Page 7276 – line 2ff. This sentence is not very clear and needs some rewording.

Our response: We changed the sentence for: “Our observed deviation toward slopes < 1 can instead be explained by the nitrified NO_3^- with a lower $\delta^{18}\text{O}\text{-NO}_3^-$, mostly derived from water (Andersson and Hooper, 1983), relative to the high ambient $\delta^{18}\text{O}\text{-NO}_3^-$ values.”

Page 7276 – line 6f. Is this your own observation or an observation made in the cited study?

Our response: This is an observation made in the cited study. We clarified this, changing the sentence for: “. . . as observed in Casciotti et al. (2013) and Bourbonnais et al. (2015).”

Page 7276 – line 16ff. This paragraph is again very hard to digest for any non-expert reader. Maybe you could expand here a bit on the different approaches used to calculate ϵ .

Our response: We changed the first sentence of this paragraph for: “Linear regression

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coefficients for ε calculated using the different approaches presented in section 2.4 are listed in Tables 1 and 2.” The manuscript is already long and we think it is preferable to refer the reader to section 2.4, where the different approaches used to calculate ε are well explained, rather than repeating this information again here.

Page 7277 – line 4ff. Please specify those “mass balance considerations”.

Our response: We changed the sentence to: “The latter two, using DIN or biogenic N₂ as the basis to calculate ε , are more representative of N-loss.”

Page 7277 – line 25ff. As mentioned before, your T/S data merely indicate a relatively homogeneous water mass over the shelf, but that does not necessarily exclude any mixing with waters further offshore. Could you provide data from neighboring, offshore stations that show a significantly different T/S signature for those waters?

Our response: Again, if we compare with data for offshore waters from Bourbonnais et al. (2015), we observe a similar T/S signature for the “upwelled” source waters, which make sense in this context. However, given the narrow range in T and S, further mixing between different water masses on the shelf is unlikely, favoring a closed system. We changed the sentence to clarify: “Closed system estimates of ε are likely more reliable in our setting because of low likelihood of mixing between water masses of contrasting characteristics on the shelf. Temperature and salinity in the OMZ at our stations narrowly ranged from 13.5 to 15 oC and 34.88 to 34.98 (Fig. 2), similar to T/S signatures from offshore source waters (Bourbonnais et al., 2015), and suggestive of a single water mass.”

Page 7278 – line 21f. Please specify how potential effects of contributions from organic N to N₂ formation were taken into account.

Our response: We already explain this in the above text (Page 7277, lines 6 to 10): “Calculations based on changes in $\delta^{15}\text{N-NO}_3^-$ are affected by NO_2^- accumulation and isotope effects of NO_2^- oxidation (see above). The 4‰ difference in ε calculated from

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changes in $\delta^{15}\text{N}$ of biogenic N_2 vs. $\delta^{15}\text{N}$ of DIN may arise from the contribution of NH_4^+ derived from organic matter to biogenic N_2 via the anammox process.” We think that it would be repetitive to state this again.

Page 7279- line 12ff. This is too simple a conclusion. One could also argue that N-flux measurements over a relatively short time span and at relatively few locations overestimate benthic N-loss. And, there are hot spots of water column N-loss, too.

Our response: We revised the sentence for: “However, our comparison to direct measurements of fluxes should be considered tentative as they are made at single locations over relatively short time periods are thus subject to considerable spatial and temporal heterogeneity.”

Page 7279 – line 21f. Not only N-loss processes, but, as you have demonstrated in previous sections, also nitrification and incomplete denitrification determine the isotopic compositions of nitrite and nitrite. Please revise.

Our response: We accordingly changed the sentence for: “We found that N-loss representing the net effect of partial denitrification, anammox and nitrification produced in sum large variations in isotopic composition.”

Page 7280 – line 6f. How does your estimate of nitrite turnover time compare to recently published N-fluxes and N-inventories for the Peruvian coastal OMZ by Kalvelage et al. (2013)?

Our response: We added the following discussion, Page 7275, line 14: “This estimate is higher than ratios of NO_2^- oxidation/ NO_3^- reduction of up to 54% for the Peruvian coastal ODZ derived from direct rate measurements (Lam et al., 2009; Kalvelage et al., 2013), and should thus be considered as an upper limit.”

Page 7281 – line 4f. Please include some reference for benthic N-loss on the Peruvian shelf, e.g. Bohlen et al. (2011).

Our response: We added this reference.

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Tables and Figures Table 1 and 2 For non-expert readers it may not be obvious that ε corresponds to the slope of the linear regression. Hence, instead of listing ε and the error of the slope of the linear regression separately, I suggest to delete the “Error on slope” column and include the error in the ε column (e.g. N2 biogenic = 14.27 ± 0.86).

Our response: Done.

Figure 1 Panel A is not labeled as such. Further, the inserted ODV map is very small and provides little extra information. I suggest removing it.

Our response: We labeled panel A and removed the inserted ODV map.

Figures 3, 4 and 8 A cosmetic detail: ODV allows plotting bathymetry using station bottom depths. Those grey sediment boxes look somewhat arbitrarily drawn.

Our response: We improved all ODV figures based on more accurate bathymetry using station bottom depths.

Figures 5, 6 and 9 Please increase figure size.

Our response: We will ask copyediting to increase figure size for these figures, as requested.

Figure 7 The axis label looks somewhat skewed (aspect ratio not locked during increase/decrease of plot size). Please correct.

Our response: We removed this figure, as suggested by reviewer #1.

Figure 10 The figure does not add any information, all values are given in Table 1 and 2. Please remove.

Our response: We also removed this Figure, as suggested by reviewer #1.

Technical corrections:

Page 7258 – line 11 Please add comma after “10 μM ”. Page 7259 – line 12 Please add comma after “nutrient”. Page 7259 – line 17 Please change to: “dominant pathway for

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N-loss” Page 7262 – line 17 “temperatures” instead of “temperature”. Page 7265 – line 4 “parcels” instead of “parcel”. Page 7267 – line 9 Please add comma after “found”. Page 7268 – line 20 Please add comma after “[NO₂-]”. Page 7269 – line 20 Please add comma after “N₂ Page 7269 – line 24 Please add “ones” after “canonical”. Page 7270 – line 2f This should probably read: “The Rayleigh equations’ y-intercepts, where $f = 1$ represents the initial $\delta^{15}\text{N}$ of NO₃⁻ or DIN, varied from ...” Page 7274 – line 1 Change to: “Kalvelage et al., 2013” Page 7275 – line 1 “average” instead of “avering” Page 7277 – line 4 Please add comma after “two”.

Our response: We applied all these technical corrections.

Interactive comment on Biogeosciences Discuss., 12, 7257, 2015.

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