

## ***Interactive comment on “Biogenic nitrogen gas production at the oxic-anoxic interface in the Cariaco Basin, Venezuela” by E. Montes et al.***

**Anonymous Referee #2**

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General comments:

Montes et al. used measurements of excess nitrogen gas above atmospheric equilibrium to investigate N loss in the Cariaco Basin. They concluded that the excess N<sub>2</sub> varied seasonally in response to changes in POC/PON export. The authors also found the excess N<sub>2</sub> was comparable to the DIN deficit, which substantiated the hypothesis that the N<sub>2</sub> excess is mainly of biological origin. Overall this manuscript is well written and suitable for publication after revisions.

Specific comments:

Section 3.2: In the introduction the authors state that waters were sulfidic below ~250m which is also where most of the N<sub>2</sub>/Ar measurements were taken. They do not spec-

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ify in the method whether sulfide was removed, corrected for, or did not affect N<sub>2</sub>/Ar. Granted, one would not expect good agreement between the excess N<sub>2</sub> and DIN deficit if this were a major problem, however interfering gases are a well-known problem experienced by other researchers using IRMS (e.g. Emerson et al. 1999). For the information of other researchers please specify.

pp. 10559, line 13; pp. 10560, lines 3, 17. “Negative anomalies in N<sub>2</sub>...”, “. . .undersaturated in dissolved N<sub>2</sub>”. It is misleading to state that N<sub>2</sub> is undersaturated due to heating. The author’s sufficiently explain how, due to the differing solubility curves of N<sub>2</sub> and Ar, heating coupled to incomplete equilibration can give rise to a normalized N<sub>2</sub>/Ar < 1, and thus a negative excess N<sub>2</sub>. However, heating sans equilibration would lead to a supersaturation of the absolute concentration of N<sub>2</sub> gas. For instance, pp. 10559, line 13 should more accurately state “Negative anomalies in excess N<sub>2</sub>...”, or pp. 10560, line 16-17 “. . .in situ heating is the most likely cause for the negative values of excess N<sub>2</sub>...”

pp. 10562, lines 13 – 17. “We did not observe a difference...higher in March 2009 and September 2008”. It hard to reconcile the two statements that 1) there was no difference between March 2009 and September 2008 in the average nutrient concentrations of the stations sampled for excess N<sub>2</sub>, and 2) DIN was significantly higher between the two seasons at the CARIACO Time-Series station, especially given that the Time-Series station IS one of the stations at which excess N<sub>2</sub> was determined. This apparent incongruity seems to arise from the fact that the authors averaged all the nutrient data from all 6 of the stations sampled for N<sub>2</sub>/Ar. It would help if the authors showed the Time-Series nutrient data that was used to calculate the DIN deficit on Figures 4 and 9 (include data with O<sub>2</sub> < 65  $\mu\text{mol/kg}$ ) given that subsequent analyses and conclusions rely on this data from the Time-Series.

pp. 10564, section 5.3. I am surprised that the excess N<sub>2</sub> matched the DIN deficit because: 1) The authors did not subtract the preformed normalized N<sub>2</sub>/Ar from the measured value (Devol et al., 2006; Chang et al. 2010). Normalized N<sub>2</sub>/Ar > 1 due to

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physical processes have been observed at BATS, which this study is using as its source waters. This requires more discussion given previous work using normalized N<sub>2</sub>/Ar. 2) DIN deficits have been difficult to determine in anoxic waters due PO<sub>4</sub><sup>3-</sup> reacting with reduced metals, which must also happen in the Cariaco Basin. How did the authors determine this did not occur? As I already commented above, it would be remarkable for the DIN deficit to match the excess N<sub>2</sub> if the above issues were significant, however I do believe they warrant consideration.

pp. 10566, lines 8 – 10. “. . .where 0.86 is a factor that removed the effect of SRP production from organic matter. . .” The factor of 0.86 was used in the past when it was unknown if organic N remineralized during denitrification was converted to N<sub>2</sub> and thus the loss of fixed N was simply a NO<sub>3</sub><sup>-</sup> deficit. However, the authors are assuming the anammox process occurs and so the factor of 0.86 is unnecessary.

Figure 5. In many of the other figures the authors present average values for each season (nutrients, N<sub>2</sub> excess, DIN deficit). They should be consistent and present the average for each season in this figure.

Figure 7. What is the variability of the POC/PON flux? Please add error bars to this plot or indicate range of values.

Figure 8. This schematic is not useful and should either be removed or significantly altered. First, there are undefined abbreviations (Ez, SUW). Secondly, other than changing the width of the arrows to represent increased fluxes, the authors make no attempt to illustrate the other differences they describe in the text between “upwelling” and “relaxation”. In the text they hypothesize that an intrusion of water from the Caribbean Sea could lead to a deepening of the oxic-anoxic interface and higher O<sub>2</sub> in the upper water column, neither of which are represented in the schematic. In fact, contrary to the text, they show NO<sub>3</sub><sup>-</sup> rich (and O<sub>2</sub> laden) water intruding directly into the zone of N-loss, which, if this were actually occurring, would DECREASE anaerobic N loss due to the introduction of O<sub>2</sub>.

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