General comments

The manuscript by Cornejo and Farias deals with a specific aspect of an important problem in ocean biogeochemistry - the cycling of nitrogen in the ocean - by focusing on the removal of nitrous oxide (N_2O) by denitrification. N_2O is an important greenhouse gas, and about a quarter to one-third of its natural sources are found in the ocean. While the oceanic production seems to be relatively well constrained to about 6 Tg/year (e.g. Nevison et al., 2003), the oceanic sink – denitrification within oxygen minimum zones (OMZ) – appears much more uncertain. In fact, to my knowledge, no simple model of N_2O consumption by denitrification, similar to Nevison et al. (2003)'s N_2O production model, or its analogues, exists. However, due to the small volumes of denitrifying regions in the open ocean, it is not clear how important the denitrification sink could be for the N_2O budget. A simple scaling argument (see note at the end) suggests about one order of magnitude less removal than production; nonetheless it would be very useful to put a firm constraint on this figure.

A second point of interest of the manuscript is that since denitrification is the only known sink of N_2O in the ocean, any evidence of N_2O removal can be interpreted as strongly suggestive of ongoing denitrification. This is an important issue, as there seem to be disagreement regarding the dominance of denitrification or anammox in the Eastern Tropical South Pacific (ETSP).

The major contribution of Cornejo and Farias' manuscript is providing and analyzing a relatively large dataset of N_2O measurements (~900 data points) from a major open ocean OMZs. In conjunction with measurements of other properties that are relevant to the N-cycle (in particular NO_3^-, NO_2^- and O_2), this dataset has the potential to allow large-scale syntheses of N_2O cycling in low-oxygen regions, similar Nevison et al. (2003)'s model of N_2O production. This would be particularly useful to constrain the oceanic N_2O budget, improve existing models of N_2O cycling in the ocean, and explore the consequences of projected ocean deoxygenation on the N_2O balance.

Unfortunately, I have some major concerns about the manuscript that prevent me from supporting publication of it as is.

Specific comments (major points)

(1) The manuscript falls short on the expectations outlined at the end of the abstract (l. 11-14) to "quantify the ratio of N_2O production/consumption that is being cycling in O_2 deficient water". The usefulness of the analysis in Cornejo and Farias' manuscript is somewhat limited from a modeling and N-cycle budgeting point of view. In order to have a closure of the N_2O budget in the ocean, large scale N_2O production/consumption *rates* need to be quantified, for example by relating them to measured oceanographic properties (e.g. O_2 concentration, as in Nevison et al., 2003). However the Authors provide parameterizations for N_2O concentrations only.

The Authors show correlations between measured N_2O excess (ΔN_2O) and O_2 deficit (-ΔO2, or AOU) as well as NO_2^-. It is unclear how these correlations can be translated into rates of N_2O consumption within the denitrifying domain of the OMZs. The problem is similar to the determination of N_2O production rates by Nevison et al. (2003)'s. By combining laboratory results (Goreau et al., 1980) with large-scale observations of ΔN_2O, Nevison et al. (2003) were able to propose an equation for instantaneous N_2O production by nitrification that proved very useful for N_2O models and budgets.

The subtlety in Nevison et al (2003)'s analysis, which seems to be missing from Cornejo and Farias', is that one cannot directly use the correlations between measured ΔN_2O and O_2 (or NO_2^-) to infer the instantaneous rates of NO_2 production or consumption. This is due to the fact that ΔN_2O represents the integral result of N_2O sources and sinks along the pathways of circulation of water parcels, and is therefore also strongly affected by mixing processes between water masses with different $O_2/NO_2^-/N_2O$ gradients. Nevison et al. (2003) were able to use ΔN_2O and $-\Delta O_2$ data in a water-mass path integral sense, to constrain an empirical model (based on laboratory experiments) of N_2O production due to nitrification. It is unclear from the manuscript how a similar and potentially very useful analysis can be done to estimate N_2O removal rates from in situ O_2 and NO_2^-.

In fact, the empirical relationships proposed by Cornejo and Farias (equations 1 and 2), although able to reproduce ΔN_2O in the low-O_2 regions reasonably well, cannot be interpreted in a straightforward way to obtain the N_2O sink. Cornejo and Farias' analysis appear rather descriptive, and is not directly translated in a quantitative model of the oceanic N_2O cycle (as implied in the abstract, as well as in the conclusions, p. 2700, lines 14-16).

(2) I am not sure that the analysis of Cornejo and Farias brings new insights on the cycling of N_2O in the OMZ. For example, the relevance of denitrification as a removal pathway for N_2O in the ETSP was already apparent from the results of a previously co-authored paper (Farias et al., 2007).

The analysis of global data by Nevison et al. (2003) failed to show substantial correlations between ΔN_2O and N* (after correction for mixing). I wonder if the much larger dataset described in Cornejo and Farias's manuscript can be used to show the opposite – that is, a robust biogeochemical signature of denitrification in the ΔN_2O distribution (and distinguish between production and consumption regimes).

It is certainly true that Cornejo and Farias' analysis shows the inadequacy of current parameterizations of N_2O cycling, for example Nevison et al. (2003)'s model, in capturing the N_2O distribution within the OMZ cores. This is a major point to be gathered from Figure 2.b. However, the comparison is somewhat unfair, and the result to be expected, since Nevison et al. (2003) parameterization excludes by construction the denitrification sinks of N_2O. Additionally, the issue of water mass mixing is not adequately discussed in relation to equations 1 and 2 and figure 3a-b, and the Authors should be more specific in excluding any mixing origin for the correlations that they observe.

Since the thresholds that control anaerobic processes in the OMZs are so poorly constrained, I found interesting that Cornejo and Farias updated the O_2 limit under which N_2O reduction dominates from ~4 mmol/m3 to ~8 mmol/m3. This is potentially important as it expands the denitrifying domain approximately two-folds. I have no particular reason to prefer a threshold value to another, and Cornejo and Farias' limits seem supported by a large dataset. Nonetheless, it is not clear whereas the threshold of 8 mmol/m3 was determined from the in situ data alone (and how exactly it was determined), or if it was chosen based on Bonin et al. (1989) laboratory experiments. Similarly, a discussion on how the NO_2^- threshold of 0.75 mmol/m3 was determined should be included too.

Minor points

p. 2697, lines 19-20. It would be useful to specify the equation and parameters from Nevison et al. (2003) that were used to generate Figures 2.b-d.

p. 2699, line 2. The relationship reported does not give the rate of N_2O consumption, but an estimate of the integrated N_2O standing stock. It is not clear how to translate this into a rate. See the previous discussion for point (**1**).

p. 2699, line 16. Equation 2 is not an exponential function. Perhaps he Authors intended reciprocal function (power-law with exponent -1).

p. 2699, discussion of equations 1 and 2, as well as Figure 3.a-b. I am not sure that equation 2 represents a substantial improvement over equation 1. Is the predicted ΔN_2O substantially better? The usefulness of equation 2 seems somewhat limited as O_2 is routinely measured and overall larger O2 datasets exist compared to NO_2^- (e.g. World Ocean Atlas, GLODAP). However it is true that some concerns exist regarding the reliability of O_2 measurements at low O_2.

p. 2700, first paragraph, and Figure 3.c. It is not clear why the authors chose to compare the updated fit for ΔN_2O (equations 2) to the ΔN_2O profile in Figure 3.c alone (it has only 4 points!), instead of comparing it to the full set of in situ ΔN_2O data, as done in figure 2.b-d. This would be straightforward and should provide a better way to evaluate the new fit of equation 2.

Figure 2. Please clarify what the dashed lines in panels b-d represent. Are they the result of a linear fit to the data? Maybe statistics of these fits can be added to the panels (R^2, number of points).

Technical points and typos

The manuscript is concise and generally well written. However, a number of long sentences would benefit from being rephrased or broken down into shorter ones to improve readability. A number of typos should be corrected in the final version.

p. 2692, line 12. “cycled” instead of “cycling”?

p. 2692, line 13. Remove “of N_2O” (sentence unclear as is).

p. 2693, lines 13-18. This paragraph is unclear and hard to read, and would benefit from rephrasing.

p. 2694, lines 1-5. Also this paragraph is somewhat unclear and hard to read, and would benefit from rephrasing. Also, use “scenarios” instead of “sceneries”, and “makes” instead of “make”.

p. 2694, line 9. Remove the comma after “both”.

P 2696, line 23. “lower than previously reported” instead of “lower than the previous reported”.

p. 2699, line 11. “therefore” instead of “because”?

p. 2699, line 17. “with” instead of “where”?

p. 2699. Equation 2 should be edited/clarified. $[NO_2^-]$ should be used instead of [NO]. Also, I am not sure whereas the error ±8 refers to 39.145 (in which case the three decimal figures should be dropped), or if its the coefficient for $[NO_2^-]$. Also, I wonder why an error is not associated to the constant coefficient (-9.2744) as well.

p. 2700, line 1. “for the vertical” instead of “forvertical”.

p. 2700, line 5. “depicted” instead of “despicted”.

p. 2700, line 1. “in relation to” instead of “within”.

p. 2700, line 18. What do the author mean with "metal availability measurements". I find it odd that the first and only reference to metals is in the last part of the conclusion. Maybe this reference can be clarified?

p. 2700, line 22. "simulating" instead of "stimulate".

Figures 1.b-e and 2.a. I feel that better contouring technique would substantially improve the figures' clarity. Also, contour lines and labels could be added to Figure 2.a.

Figures 2 and 3. Please change "PN_2O" to "ΔN_2O" in the labels.

Figure 3. Please change the "," to "." In the x labeling of panel 3.b (e.g. "0.0" instead of "0,0" etc.)

Pages 2694 and 2697. Please change in all the references "Nevinson" to "Nevison".

Scaling of net N_2O removal by denitrification. Assuming a volume for OMZs (O2<8.0 umol) of ~5*10^15 m3, and a net removal rate of ~5 nmol/l/year (e.g. Yamagishi et al., 2007) the global net removal of N_2O by denitrification in the OMZs results on the order of less than ~1 TgN/year, or about one order of magnitude less than the N_2O net source (~6 TgN/year).