

Interactive comment on “Excess nitrogen as a marker of intense dinitrogen fixation in the Western Tropical South Pacific Ocean: impact on the thermocline waters of the South Pacific” by Alain Fuménia et al.

Anonymous Referee #1

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Fuménia and coauthors have presented biogeochemical data from the OUTPACE expedition that took place in the western tropical South Pacific Ocean. Presented data are important but it lacks a robust analysis. There are too many speculative statements to list them all. I try to list some of main points below:

Authors have tried to interpret their biogeochemical data (such as N^*) based on community structure (such as lines 385–388 on page 10, “In the SW, the N^* variations.”). But the authors present no record of biology data, so all such statements (including the ones are PON:POP stoichiometry) remain speculative!

The manuscript seems like more or less a data reporting manuscript with no robust statistical analysis. All the figures and tables list some data with repetition but no rigorous analysis. For example, Figures 10, 11 and 12 are very similar to each other and it is not clear what the authors are trying to convey. What is the main hypothesis that the authors are trying to address?

Line 21 suggests positive N^* while line 24 suggests N^* to be zero. I presume these must be from different regions. Please specify.

Line 53: But nothing can be better than in situ measurements better! Criticism of in situ measurements should be with caution and proper justification.

Line 63: Let the equation come first before it is to be referred.

Line 83: area should be replaced with areal

Line 114: Addition of $HgCl_2$ might degrade Organic matter and one my overestimate nutrients. Convention is just to freeze the water for nutrient analysis. Why was $HgCl_2$ added despite that samples were frozen?

Line 140: Provide lot no for the $^{15}N_2$ gas as these have been subject to contamination recently.

Why 2.90 was not added in equation 2 but in 3. Is PON_{excess} zero without 2.90. Reasons should be provided why is it necessary to make it zero. I believe the equation was originally given by (Gruber and Sarmiento, 1997; Michaels et al., 1996) and not by Deutsch et al (2001) as authors seem to suggest (lines 150 and 191)

If inorganic and particulate values of N and P were considered for “excess” analysis here (equations 2 and 3) then why not dissolved organic values (DON and DOP) as well? That is considered to be an equally significant pool of N and P?

Provide the reproducibility (precision) and detection limit of all the measured parameters in the method section. Can PON and POP be measured in three decimal digits of

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mico-mol/L, which is almost like nmol/L in 3 decimal digits?

Line 383: there is a typo in “surface”

Lines 424-425: sentence “thezooplankton” is not clear

Line 428-431: “ On an annual.N* signal”. See (Mills and Arrigo, 2010; Singh et al., 2017) for an alternative hypothesis.

Lines 433-436: “Nevertheless.atmospheric nitrogen input”. See (Jickells et al., 2017; Singh et al., 2013) for more details.

Line 521: how can “0” rates be measured.

Line 555: “deficit of NO₃ and PO₄”. Mathematically (from the N* concept used here), both nutrients cannot be deficit.

Line 572: “occurred” should be replaced by “been undertaken”

Line 680: Letscher is misspelled (Letscher et al., 2013)

References:

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Letscher, R.T., Hansell, D.A., Carlson, C.A., Lumpkin, R., Knapp, A.N., 2013. Dissolved organic nitrogen in the global surface ocean: Distribution and fate. *Glob. Biogeochem. Cycles* 27, 141–153.

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Mills, M.M., Arrigo, K.R., 2010. Magnitude of oceanic nitrogen fixation influenced by the nutrient uptake ratio of phytoplankton. *Nat. Geosci.* 3, 412–416.

Singh, A., Bach, L.T., Fischer, T., Hauss, H., Kiko, R., Paul, A.J., Stange, P., Vandromme, P., Riebesell, U., 2017. Niche construction by non- N_2 diazotrophs for N_2 fixers in the eastern tropical North Atlantic Ocean. *Geophys. Res. Lett.*

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