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Biogeosciences Discussions

Interactive comment on "Following the N_2O consumption at the Oxygen Minimum Zone in the eastern South Pacific" *by* M. Cornejo and L. Farías

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Reply Reviewer # 1 We thank the referee for his/her comments which helped to improve our work. We totally agree about the necessity to refer to the quality of the data, especially given the nature of this ms. This was initially omitted because of the ms. was written in the form of a letter. However we can expand and include these issues in methodology section. We have separated the comments from the reviewer and below we responded to each one of the reviewer. Regarding each point 1. Quality of the data a. What is the analytical error and accuracy of the N2O measurements? We are calculated the analytical error of the N2O measurements is least than 3% and the accuracy of the N2O measurements are given in the plots. These information are now included in the text. Moreover, during the period in which data have been collected, two

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inter-calibration exercises have been conducted with colleagues from Japan (Yoshida lab; Figure 1, here) and Germany (Bange lab). These exercises gave optimal results and we are confident in the quality of the data of N2O.

b. What is the measurement error for the frozen nutrient samples? Regarding this point, it is important to note that the analysis of nitrite and phosphate in two of most extensive cruises (Knorr and Galathea) were analyzed on board so that we and other colleagues could make decisions about the depth of experiments. But, we also took samples for further analysis in laboratory. These collected samples were then analyzed in Denmark (Bo Thamdrup) and USA (Jim Moffat) and the results did not give differences higher than 2% if we compared to those samples analyzed on board. Anyway, part of nutrient data has already published in different paper: Farias L, Castro-González M, Cornejo M, Faúndez J, Boontanon N, Yoshida N. Denitrification and nitrous oxide cycling within the upper oxycline of the oxygenminimum zone off the eastern tropical South Pacific. Limnol Oceanogr 2009; 54 (1):132-44. Fernández C, Farías L, Ulloa O. Nitrogen fixation in denitrified marine waters. Plos ONE 2011; 6(6): e20539. Ward BB, Tuit CB, Jayakumar A, Rich JJ, Moffett J, Wajih S, Naqvi A. Organic carbon, and not copper, controls denitrification in oxygen minimum zonez of the ocean. Deep-Sea Res I 2008; 55:1672-83. Bo Thamdrup a, TageDalsgaard b, Niels Peter Revsbech.2012 Widespread functional anoxia in the oxygen minimum zone of the Eastern South Pacific. Deep-Sea Research I

c. Errors of O2 measurement by Winkler and STOX are not given. Analytical error of O2 measurements by Winkler was 1.26%. Our laboratory have good expertise on dissolved oxygen measurements; we had built an automatic system for measurement of oxygen, called AULOX, that avoids many analytical errors of titration and had been monitoring the transmission of the errors. Also, to avoid the interference of nitrite in the titration, we added azide to the reagent 2 as was early suggested by Broenkow and Cline (1969) (Limnology and Oceanography, 14, 450-454). Undoubtedly, part of the error in the oxygen measurement is associated with sampling and contamination with

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atmospheric oxygen, in spite of all the taken precautions. Regarding the Stox measurements, these measurements have been performed only after the Galathea cruise and MOOMZ. This sensor with nanomolar sensitivity is giving to us the real concentration of O2 is the eastern South Pacific OMZ and probably others. Reference relating to the analytical error can be found in Revsbech. N. P., L. H. Larsen, J. Gundersen, T. Dalsgaard, O. Ulloa& B. Thamdrup. Determination of ultra-low oxygen concentrations in oxygen minimum zones by the STOX sensor.Limnology and Oceanography: Methods, 7, 371-381, 2009. Bo Thamdrup, TageDalsgaard, Niels Peter Revsbech. 2012. Widespread functional anoxia in the oxygen minimum zone of the Eastern South Pacific. Deep-Sea Research I, doi:10.1016/j.dsr.2012.03.001

d. Seasonal and interannual variability, How comparable are the data at all? First of all, we only consider seasonal data most of them came from austral spring summer time. Data got in winter time did not include in the data set. We don't consider the interannual variability in N2O, because one of the largest expected interannual changes would be associated with ENSO cycle with a warm phase El Niño, but it did not register any event with a biogeochemical impact into the region nor an oxygenation event in the Peruvian (Dimitri Gutierrez, con. Pers.) and Chilean coast during the study period.

e. Atmospheric N2O dry Although "contemporary" method to calculate equilibrium N2O produce a deviation up to 15% (Nevison et al, 2003), we have re-calculated the N2O equilibrium concentration considering the age of the water mass. As the cruises considered in this study don't have CFC concentrations, we took reference values of CFC-11 and CFC-12 from P19 (0° - 13°S) and P21E (14° - 16° S) of the WOCE transects. Then we calculated the age of the each isopycnal from the subsurface equatorial water. We assumed that the age of the ESSW's isopycnals in this location is the same during our cruises. For stations southward from these locations we assume that the ESSW moved toward south at 10 cm/s to obtain the age (Pizarro, O., G. Shaffer, B. Dewitte, and M. Ramos (2002), Dynamics of seasonal and interannual variability of the Peru-Chile Undercurrent, Geophys. Res. Lett., 29(12), 1581, doi:10.1029/2002GL014790).

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With these ages we obtained the atmospheric N2O concentration (in ppb) from the data of Holland et al (2005) at the time of the water mass formation. The equilibrium N2O concentrations were obtained using these values. This information is now included in the manuscript. It is important to mention that the new ïĄĎN2O concentrations were as much as 0.5 nM higher than the previously calculated. However, for low ïĄĎN2O these differences may be important. We have re-evaluated our equations with these new data. 2. Up-to-date literatura We are updated the literature as was suggested by the reviewer as follows: a) P.2692, I.16. Archaeal nitrification has been mentioned in the text as a source of the marine N2O concentrations (Santoro et al, 2011). The recent findings of oxygen dependency found by Frame and Casiotti (2010) has been cited. b) P.2692, I.20. The estimated oceanic contribution to atmospheric N2O was changed and the correspondent references were included. c) P.2692, I.25. The Naqvi et al (2010) work was included. d) P.2693, I.9. We included some discussion from Naqvi et al. (2010) e) P.2693, I.11. We changed the Seitzinger and Kroeze cite f) P2693, 2694. We included results from Ryabenko et al (2012) g) P2694, I.8. Cite included 3. Fit of the equations. The reviewer is correct to saying that the equation 2 is made from our own data, we carefully correct the conclusions about to obtain new relationships in other OMZs. About the equation 1, it was made was made with data from cultures of Bonin et al (1987) and then applied to our data. As for nitrite, is also true that for the core of the OMZ of the ESP and with our data, the equation works, and it must be tested in other OMZs. 4. SNM discussion. Discussion of SNM is given in the text. We add some new information in there and we think now that is more documented. Also, it have been well parameterized in our study area by Farias et al., 2007, Thamdrup et al (2012) and new insight about the origin could be found in Lam et al 2011 (Origin and fate of the secondary nitrite maximum in the Arabian Sea, Biogeosciences, 8, 1565–1577). Minor Comments. 1. We changed all the Nevison cites and reference 2. PN2O has changed by ïĄĎN2O 3. Figure 3: PN2O has changed by ïĄĎN2O. 4. Figure 3c: The profile shows in green the NO2- concentration and in black theïĄĎN2O modeled as a function of NO2-.

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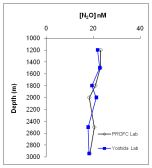

Figure 1. Profile of N2O analysed by two laboratories.

Fig. 1.

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