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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

Anonymous Referee #1

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General comments Despite the fact that the ocean is a major source of atmospheric N2O, surprisingly little is known about the oceanic production and consumption pathways of N2O. Therefore, simulations of the present oceanic N2O distribution as well as prediction of future changes of the oceanic pathways of N2O are very uncertain. Most published model efforts are based on the simple fact the N2O production is tightly linked to dissolved O2: A variety of empirical N2O/O2 or deltaN2O/AOU relationships have been established to simulate (moderately successful) N2O water column distribution. However, simulation of N2O consumption in extremely depleted O2 minimum zones is still a challenge.

Based on a compilation of previously published data sets of N2O, O2 and nutrients from the eastern South Pacific (ESP), Cornejo and Farias derive two new parameterization

of N2O in the ESP (incl. N2O consumption in OMZ). Although the results presented are of interest for model development, I have some severe concerns about the basic data treatment which seems to be too superficial.

Therefore, I recommend publication of the ms only after major revisions.

Specific comments A) The authors have compiled an impressive amount of almost 900 N2O measurements and other measurements from 10 cruises. However, important information to judge the quality of the data is missing: - What is the analytical error and accuracy of the N2O measurements? Obviously the measurements are not calibrated against usually applied international N2O standard scales. - What is the measurement error for the frozen nutrient samples? It is well known that measurements from frozen samples have a high degree of uncertainty. - Errors of O2 measurements by Winkler and STOX are not given. On page 2698 I find the statement " . . .and taking into consideration the possible biases in O2 measurements, (e.g., detection limit of the Winkler method; CTD response; contamination during the sample collection, etc.) ..." When there are significant biases between the different O2 measurements by Winkler and STOX, then it needs to be discussed. Or in other words: any determination of O2 threshold is meaningless unless the O2 are not on the same scale. - I did not find any comments about potential bias in the data set caused by seasonal and interannual variabilities. How comparable are the data at all? - In order to calculate deltaN2O one need to know the atmospheric N2O dry mole fraction at the time when the sampled water mass had its last contact with the overlying atmosphere. This is also important for calculation of mixed layer deltaN2O because the measurements cover a period from 2000 to 2010 with the consequence that the atm. N2O dry mole fraction has increased significantly during that period.

B) In general the ms suffers from not being up-to-date with the literature (see also my comment D), some important references are missing: p. 2692 , l.16: There is an ongoing debate about the N2O production during nitrification and the resulting O2 dependency. This has been ignored completely, see e.g.: Santoro et al., Science, 2011; Frame and Casciotti, BG, 2010. p. 2692, l. 20: Bange, 2006 should be replaced with IPCC 2007 and the number given in the IPCC report should be cited. p. 2692, l. 25: The O2 sensitiveness of the denitrification was also shown by Naqvi et al., Nature, 2000. p. 2693, l. 9: add/discuss Naqvi et al., BG, 2010. p.2693, l. 11: Seitzinger and Kroeze 1998 is not the appropriate citation in this context. Please cite/discuss Nevison et al., 2003 and Naqvi et al., BG, 2010. p. 2693/2694, discussion of the ESP nitrogen cycle and N2O: Ryabenko et al., BG, 2012 is missing. p. 2694, l.8: the most recent approach to model N2O water column distribution is given in Freing et al., GBC, 2009. p. 2697, section 2.2: discuss Freing et al., 2009, as well.

C) Another major concern about the presented results is a more fundamental one: The two empirical relationships presented (equations 1 and 2) are based only on data by Cornejo and Farias and, of course, they fit to their data very well (this is not surprising). It would have made much more sense to test the new relationships against other data sets from the same region or from another OMZ region as well. As it stands now, equations 1 and 2 are therefore only of limited applicability. In the conclusion section it should be clearly pointed out, therefore, that the results are only valid for the ESP and no general conclusions for other OMZ or even global modelling effords can be drawn.

D) Secondary NO2- max. (SNM): I am missing a critical discussion about the SNM. For many years, the SNM has been used as an indicator for denitrification in the OMZ. However, in a recent article Lam et al. (BG, 2011) showed that the SNM is a poor indicator for denitrification: “Altogether, our data do not support the long-held view that NO2− accumulation is a direct activity indicator of N-loss in the Arabian Sea or other OMZs.” So I am wondering whether the SNM is indeed the ‘best indicator for very low O2 levels’ (see e.g. statement in the abstract). Thus, I am wondering whether the presented correlation of NO2- and N2O is just by chance and does not reflect any cause-and-effect relationship. The ms would have benefited from a more detailed and critical discussion about the SNM.

Minor comments - Throughout the ms: it must read Nevison instead of Nevinson - Fig.

2: Labeling on x-axis: in-situ PN2O (nM)? When PN2O is meant, then it should be given in natm or nbar; but I guess the correct labeling should be in-situ deltaN2O (nM). - Fig 3: again, in-situ PN2O does not seem to be the right labeling on the y-axis. - Fig3c: Figure legend is obviously erroneous: NO2- is depicted in both green and black points?

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