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Interactive comment on “Following the N₂O 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

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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,

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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 efforts 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.

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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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