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Interactive comment on “Nitrous oxide dynamics in low oxygen regions of the Pacific: insights from the MEMENTO database” by L. M. Zamora et al.

Anonymous Referee #1

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GENERAL COMMENTS

Zamora et al. present analysis using the MEMENTO oceanic database to characterize marine N₂O dynamics with a focus on the low oxygen zones of the Eastern Tropical Pacific (ETP). The study includes estimation of net N₂O production and consumption rates in the ETP based on the MEMENTO data, and accompanying sensitivity analyses from an ocean model (UVic-ESCM). The study presents some useful analysis of net production and consumption rates within the ETP, however, the primary aims, layout, and description of the analysis are sometimes not very clear. Below I include some general comments on these issues.

1) AIMS : Is the main aim (a) to quantify net N₂O production/consumption rates in the ETP based on the oceanic data ? or (b) to provide a set of guidelines for ocean

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N₂O models in the low oxygen zones of the ETP ? If the aim is (b), then there should be significantly more focus, and detail presented on the *individual* processes that contribute to N₂O formation and destruction in low oxygen regions. The present focus on 'net' production and consumption does not adequately account for the range of possible formation pathways with significantly different N₂O yields that operate in low oxygen zones (e.g., nitrification, nitrifier-denitrification, heterotrophic denitrification, see e.g., Frame and Casciotti 2010, Westley et al. 2006, Lipschultz et al. 1990, for specific pathways). A successful predictive ocean model simulation should depend on how the yields from these individual pathways may change as the extent of oceanic low oxygen zones change. I suggest the authors focus on one or other ((a) or (b)) of these aims for this study, as the present focus is unclear.

2) LAYOUT AND USE OF MODEL ANALYSES : The current layout of the manuscript, and in particular, the role of the model simulations, is not always clear. E.g., the methods section (section 2) begins with the ocean model description, however the rationale for some of the parameterizations of N₂O production and consumption does not appear till much later in section 3. It may be clearer to present the data analysis early on (before the model discussion), so that the reasons for the specific model simulations are clearer. There is also insufficient detail on how the model was used (see, e.g., Comment 4 in Specific Comments), and on the validity of using a coarse-resolution ocean model to characterize nitrogen cycling processes and oxygen distributions in the ETP that display significant heterogeneity on scales of ~1-100 km. There is some discussion on pg. 10023 (lines 6-10) of using a higher resolution MOM4 model for 'improved accuracy of current velocities', however, there is no discussion on whether the model's nutrient, oxygen and N₂O fields were also treated at high-resolution, or of the possibilities of inconsistency if using different models for velocity fields vs. for the N₂O and O₂ fields.

Overall, the manuscript could be improved by some restructuring and refocusing on a single major aim, and by additional detail on the model and on how the model analyses

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were used.

SPECIFIC COMMENTS

1) Abstract : Lines beginning “In light of the expected deoxygenation,... it is possible that N₂O concentrations may decrease rather than increase as oxygen minimum zones expand”. As it is currently worded, this conclusion seems speculative and is not substantiated by any specific finding in this study. The main conclusions that can be drawn are that a significant degree of uncertainty surrounds N₂O production and consumption in the low oxygen zones, if these oxygen minimum zones expand.

2) Pg 10021 (lines 15-18) : It is not clear why different O₂ thresholds should not apply in different oceanic environments; i.e., the threshold may depend on which mechanisms for N₂O production or consumption dominate for that given environment, and this would be reflected in the range of threshold levels reported in the literature.

3) Pg 10022 lines 23-25 and pg 10024 (lines 1-4) : As discussed above in the General Comments, it would be clearer if the rationale for the selection of these parameterizations for N₂O production and consumption rates were discussed early on, preferably before the model discussion.

4) Pg 10024 : Lines 6-11 : Please provide a more detailed description of how exactly the MOM4 model was used in these analyses. Were only the velocity fields from MOM4 used ? Were they averaged to the larger grid size of the UVic-ESCM ? How were the model's nutrient, N₂O and oxygen fields treated ? Would there be a problem of inconsistency between the velocity values (from MOM4) and biogeochemical distributions from the UVic-ESCM ?

5) Pg 10024 (lines 15-16) Discussion of the TTD method Please clarify how the ‘modeled N₂O_{eq}’ value is obtained.

6) Pg 10026 (lines 27+) to Pg10027 (first para) : Filtering of data to exclude samples with O₂ < 10 micromol in calculating N₂O production. Will this procedure also exclude

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9, C4381–C4384, 2012

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regions of potential N₂O production though mechanisms of nitrifier-denitrification or heterotrophic denitrification ?

7) Pg 10029 (lines 25+) to pg 10030 (lines 1-3): Discussion of N₂O production from pathways other than bacterial nitrification : It should be noted that the yields of N₂O from these other pathways (which operate primarily at low O₂ levels) can be significantly larger than from bacterial nitrification, therefore the impact on overall N₂O yield could be a non-linear increase as O₂ levels decline.

8) Pg 10032-10033 Section 3.3 "Switch between net N₂O production and consumption." The rationale for and conclusions of this section need to be laid out more clearly. Currently it is not clear whether it is focused on identifying oxygen thresholds for application in ocean models, or identifying nitrite distribution characteristics that mark the onset of denitrification in the ETP. Since the authors also note the reporting of lower oxygen thresholds for nitrite accumulation from other studies that may reflect differences between oceanic environments (lines 25+ on pg 10033), they should be cautious about recommending a globally applicable higher O₂ threshold for use in ocean models.

9) Pg 10035 Lines 16-18 : Sentence beginning "If there is an increase in the volume..." As also noted for the Abstract, this conclusion seems too speculative; the main point should be that there is significant uncertainty on how N₂O production and consumption will change if the extent of the ocean's low oxygen zones change.

10) Table 1 : It would be useful if more details on the calculations and different model versions used in Table 1 were presented somewhere in the manuscript; e.g., in an Appendix.

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