Biogeosciences

Discussions

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Interactive comment on "Constraints on global oceanic emissions of N$_2$O from observations and models" *by* Erik T. Buitenhuis et al.

Anonymous Referee #2

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I strongly support the goal of this paper, to better constrain the oceanic N2O flux using optimization techniques based on a compilation of datasets of N2O and related N cycle variables, combined with process based models. However, the methodology is difficult to follow and it is hard to believe that all 4 data-based approaches converge to basically the same answer and have the same relatively narrow range of uncertainty, which is governed primarily by uncertainty in piston velocity. There is also no overall sense of what sets this paper apart from earlier efforts, since it seems to be based heavily on what is largely the same delta pN2O data set used before. While I support publication in principle, I think there are many details that should be clarified and explored before this paper is ready for publication.

Specific comments L24 Typo: It (is) also currently estimated as the dominant contribu-

tor . . .

L32 It's worth mentioning (up front) that this wide range is governed in large part by the possibility of very large coastal and estuarine fluxes. Later on line 87 we learn that the dataset resolution used here is 1x1 degree or 1.1 x 2 degree (plankTOM10.2, line 164), i.e., probably not good enough to resolve these coastal areas.

L43 although there are additional pathways, such as . . . (please give brief list).

L72 probably should mention up front that the deltaN2O/AOU data are based on ME-MENTO. Otherwise, it's a bit confusing to know the basis of this calculation.

L76 NPP is. . .estimated at 58 +/- 7 PgC/yr. . . based on what? An ocean model? Satel-lite data? Even at the lower end of 51 PgC/yr, this is on the high side of satellite-based estimates.

L83, please list relevant references rather than just saying "(see Introduction)".

Line 92-93. Since pN2O is close to equilibrium in much of the ocean, it seems im-portant to consider the quality of these pN2O measurements. For example, surface measurements made with underway systems tend to have much higher precision than analyses based on bottle collection. Was the uncertainty comparable across the ME-MENTO database and if not, were the differences in data quality accounted for in the subsequent calculations?

L125 Ocean models often do a poor job of reproducing observed O2. Suntharalingam 2012 used WOA O2 rather than model O2 for that reason. Presumably, the sensitivity to light, temperature and O2 described here is based on values from plankTOM10.2 (if not, please clarify). How well does plankTOM10.2 reproduce O2 relative to obser-vations? (Note: I saw later that my question was addressed in the Results on lines 184-188. That material belongs up front in the methods description.)

L132 paragraph starting here. This paragraph could be written more clearly, especially the sentence spanning L137-138. What is a variable N quota? Is the model using

Michaelis Menten kinetics? On line 144, a low cost function of 3.3 is better than the cost functions of >4 described for the previous model, correct? Yet, the sentence beginning on L142 with "However" suggests a large uncertainty.

L155-156 What is meant by "The slopes of the three processes" ?

Section 2.2-2.8. General question. Do the 4 databases described in section 2.2 correspond to the specific sections 2.4-2.7? If so, where does section 2.8 fit in? Is Equation 3 an alternative cost function to Equation 2 described in Section 2.3?' The apparent switch from Equation 2 to Equation 3 as the optimization technique is confusing and unclear.

Line 190 and Figure 6. The model substantially underestimates N2O in the most important hotspots of production. Doesn't this mean it will tend to underestimate global N2O production? It seems like this concern is dismissed somewhat casually with hand-waving arguments, e.g., the text starting on line 289.

Line 201-202 Please clarify how these results link together. Line 201 says that both hypoxic production AND CONSUMPTION were optimized. The subsequent results mention GROSS production of 0.33 TgN/yr, then optimized N2O production of 0.12 or 0.16 TgN/yr. Are the latter results net production? Can we infer that about 0.17-0.21TgN/yr is consumed in suboxic zones?

Line 202-203 Total production of 0.12-0.33 TgN y-1 in low O2 is only about 10% on average of total production. This is much lower than the 33% suggested by Suntharalingam et al. 2012. Does this mean that the authors are concluding that the OMZs are only responsible for a small fraction of global N2O production? Please expand on this point and call it out more explicitly in the Discussion.

Line 204 perhaps add a clause clarifying that the .0017 molN2O/molO2 slope is about 20 times the mean deltaN2O/AOU ratio of 8.15e-5 from line 82.

Line 205 production for the prognostic model is given in units of umol N2O (mol NO3)-

1. Does this represent NO3- consumed by denitrification, or produced by nitrification? Can you provide an estimate of how this relates to the previous units of mol N2O/mol O2?

Line 216 Please use consistent N2O (mol O2)-1 slope units. Here the units are umol/mol. On line 78 they were nmol/mol. On line 204, they were mol/mol.

Line 217 How does this nitrification slope in units of umolN2O/molNH4+ relate to the "N2O production slope" on line 206 in units of mol N2O/mol NO3-?

Line 219-220 Are all measurements really of deltapN2O, or are most of pN2O in the surface ocean? In the latter case, what is the uncertainty in pN2Oatm, e.g., from Eq 1?

Line 220 On what basis was this 1978 estimate made? Is there updated information that could be used?

Line 233 Typo or confusing sentence.

Line 248-250. It would be good to provide references to support these claims.

Line 282 – paragraph starting here. This exercise, combined with large data gaps in Figure 9a, including in both the ETSP and ETNP, suggest to me that the authors are overstating the degree of certainty in their confidence interval. There are large areas of the ocean with no data, including in the most important hotspots for N2O production.

Line 303-304. This is the first mention of the fact that the model produces low fluxes from the Southern Ocean. Can you cite the relevant figure here and call attention to this point earlier in the Results section? (Figure 9b,c,d all seem to indicate a substantial flux from the Southern Ocean.)

Line 308-310. The neglect of estuaries is indeed a key uncertainty, which needs to be mentioned much earlier, i.e., in the Introduction. It is also debatable whether coastal areas are adequately represented in the models presented here, which 2x1 or 1x1

resolution.

L487 Figure 2. This figure suggests very high f-ratios, e.g., of 0.8-0.9 in the northern subtropical gyres, that are a little hard to believe. The global mean looks to be on the order of 0.4! Are these generally accepted values or are they biased by measurements in highly productive coastal waters?

L505, "Model results are for the same months and longitudes as the observations." What about latitudes?

L527 This plot is dominated by the error bars and somewhat obscures the focus on the mean value, which arguably is the more important quantity. The current study makes a much more detailed effort to quantify uncertainty than most of the previous studies (some of which make no effort at all). Could a separate panel with a narrower Y-axis range be plotted to better compare the mean value of the fluxes? And can you please provide some discussion of the main factors contributing to the differences in mean value?

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