

Interactive comment on “Constraints on global oceanic emissions of N₂O 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-

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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 MEMENTO. 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? Satellite 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 important 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 MEMENTO 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 observations? (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

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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)-

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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 delta pN₂O, or are most of pN₂O in the surface ocean? In the latter case, what is the uncertainty in pN₂O_{atm}, 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

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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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