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Biogeosciences Discussions

Interactive comment on "Nitrous oxide in the North Atlantic Ocean" *by* S. Walter et al.

Anonymous Referee #2

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This manuscript presents measurements of dissolved N2O across transects in the tropical, subtropical and subpolar North Atlantic ocean. The Atlantic is relatively undersampled in N2O compared to, e.g., the Pacific, thus the current manuscript contributes significantly to the pool of knowledge on oceanic N2O. The main conclusions summarized in the Abstract seem straightforward and accurate. I have no major criticisms, but will offer a number of suggestions to help improve the paper.

Suggested Revisions:

1) The calculation of deltaN2O using the modern atmospheric pN2O in the mixed layer, a preindustrial pN2O of 270 ppb below 2000m and an average of the two (294 ppb) between the upper thermocline and 2000m is an interesting departure from past studies, which have simply used modern day pN2O at all depths to calculate deltaN2O, perhaps underestimating deltaN2O in deep water in the process. It would be useful to

show N2O_sat as estimated by this new approach as a thin solid line in Figure 4 and/or 5. Are there any discontinuities at 2000m and at the upper thermocline boundary?

2) The discussion of the difference between subtropical N2O in the eastern and western basins, which are divided by the Midatlantic Ridge, is geared toward suggesting that advection of Labrador Sea Water dilutes N2O in the eastern basin (p. 1001, 1005, Fig 5b). This point could probably be developed and supported better. First, the east-west distinction is not very evident in Figure 3. Second, Figure 5b compares a western station that is 10 degrees further south than the eastern station. Given the strong north-south gradients observed in this study, can the difference in latitude be ruled out as a contributor to the larger maximum in the western Atlantic?

3) The transitional sentences on p. 1003, lines 2-4 are weak and could use revision.

4) The last paragraph on p. 1003 compares the regression slopes of deltaN2O vs. AOU for deep ($>$ 500m) and shallow ($<$500m) data. Lines 25-26 state that the steeper slope for the deep data implies a higher yield of N2O at depth. This seems unlikely given our current understanding of N2O production in the ocean, i.e., that the highest rates of production occur just below the euphotic zone and that the N2O yield is very low at depths below 2000m (Bange et al., 1999). I would guess that the difference in slopes is more a reflection of mixing effects. An end-member mixing model might help filter out these mixing influences.

5) Plotting N2O on a T-S diagram in Figure 8 is a nice idea, but the color bar scale should be reduced to better resolve the data. Most points simply appear as green in the current graph. Also, I'm not sure that the 2 panels (a) N2O and b) deltaN2O) really add new information. Showing just deltaN2O probably would be sufficient. On that same note, the discussion on p. 1001-1002 of differences in absolute dissolved surface N2O between the subpolar, subtropical and tropical Atlantic is not very meaningful unless presented in the context of the respective surface temperatures of the different regions. DeltaN2O is the more interesting quantity.

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6) The use of “until now” on p. 995 line 5 implies that the exact pathway is resolved in the current study. I suggest deleting it.

7) The legends and axes labels in many of the figures are illegible, at least in my downloaded version, which detracts from their ability to convey information. Figure 3 and 7 are particularly bad. Please increase the font size.

8) The “whereas” on p. 1005, line 9 should be replaced with “where.”

9) A nice paper overall and an important new dataset.

Interactive comment on Biogeosciences Discuss., 3, 993, 2006.