

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

**S. Walter et al.**

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Walter & colleagues present a large and impressive new dataset of dissolved nitrous oxide along three E-W transects of the North Atlantic, a region where measurements were previously sparse. The data are described in terms of regional variability and related to water masses and dissolved oxygen. The paper is largely descriptive in nature and I feel it would benefit from more focus and emphasis on the factors determining sub-surface isopycnal variation in  $\delta\text{eIN}_2\text{O}$ , with removal of the text on surface  $\text{N}_2\text{O}$ , to strengthen the interpretation and conclusions.

In our opinion the rather short discussion of the  $\text{N}_2\text{O}$  surface layer distribution completes the overall description and should not be removed.

Changes in presentation and description of the data would also improve the paper. Section 3 - Re the calculation of  $\delta\text{eIN}_2\text{O}$ , I applaud Walter & colleagues for addressing the issue that deeper waters did not originally equilibrate with  $\text{N}_2\text{O}$  at current at-

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mospheric concentrations when last ventilated. The standard approach of estimating  $\delta\text{elN}_2\text{O}$  using the current atmospheric  $\text{N}_2\text{O}$  will lead to underestimates of  $\text{N}_2\text{O}$  production since ventilation; however it should be borne in mind that if  $\delta\text{elN}_2\text{O}$  is used as an indicator of air-sea exchange upon future ventilation of the water then the final  $\delta\text{elN}_2\text{O}$  will be lower than predicted by both approaches. I would have liked to see more discussion here, with consideration of a more robust approach - for example, using water mass age (available for some of the stations in the literature) to estimate the initial  $\text{N}_2\text{O}$  at ventilation, and comparison of the resulting  $\delta\text{elN}_2\text{O}$  with both the standard approach and their approach using an average  $\text{N}_2\text{O}$  between the thermocline and 2000m. Although not the primary aim of this manuscript, they should provide more information on the sensitivity of  $\delta\text{elN}_2\text{O}$  to the different approaches. Improvements in presentation and description of regional trends would benefit the paper.

In order to improve and clarify this point, we added more information and two additional Figures to chapter 3.

Fig 3. is not easy to interpret (or read), with contours that are directed by extrapolation across large gaps in the data, producing features that don't necessarily reflect local hydrography (is the absence of the N. Atlantic Gyre real or an artefact of the extrapolation?). Furthermore, Fig 3b), c) and f) have little data on some transects but still indicate major features; for example, the N-S boundary in  $\delta\text{elN}_2\text{O}$  associated with the Mid-Atlantic Ridge running from 0-50°N is based upon data at 10°N only.

Unfortunately up to now the data set is not large enough to avoid those gaps. Nevertheless, in our opinion the chosen isopycnal levels were the best compromise to show the distribution of  $\text{N}_2\text{O}$  in the North Atlantic.

It would be more appropriate and informative to present the data as  $\text{N}_2\text{O}$  contour plots against longitude for each of the three E-W transects, with depth or density on the y-axis. If presented this way, with the corresponding density contour and/or dissolved oxygen plot, it would improve the interpretation by emphasizing the high and

low  $\delta\text{elN}_2\text{O}$  water masses. This would aid the discussion and provide insight into the role of mixing and dilution via comparison of the individual water mass  $\delta\text{elN}_2\text{O}$  signals on the three transects.

We do not see the need for a different presentation. Both presentations have weak points.

Section. 4.1.1. Para 1. I found the contribution on the surface saturation rather weak, as no information is provided on air-sea fluxes and surface  $\delta\text{elN}_2\text{O}$  gradients, and there is little interpretation other than “surface waters were slightly supersaturated so they are an atmospheric source”. Walter & colleagues have considered N. Atlantic  $\text{N}_2\text{O}$  emission in a recent paper (at least for the tropical transect), so this section should either be expanded to include flux estimates for the sub-tropical and subpolar regions or dropped. As the major thrust is the isopycnal  $\delta\text{elN}_2\text{O}$  signal and its variation with latitude/longitude I recommend removal of Section 4.1.1 and expansion of the section on isopycnal  $\delta\text{elN}_2\text{O}$  variation.

The interpretation of the North Atlantic as a source of  $\text{N}_2\text{O}$  is not found in chapter 4.1.1 but in chapter 5.1. Indeed, the primary aim of this manuscript is the description of the distribution of  $\text{N}_2\text{O}$  in the water column, not of its emissions, thus we only included a reference to the mentioned paper. In our opinion the rather short discussion of the  $\text{N}_2\text{O}$  surface layer distribution completes the overall description and should not be removed.

Section. 4.1.1. Para 2 onwards. This section would be succinct with more clarity if the concentration data were separated out into a Table, for example, with the three E-W transects as rows, east and west as columns, and the average & max concentrations for  $\text{N}_2\text{O}$  and  $\delta\text{elN}_2\text{O}$  presented for each region (against isopycnal range where appropriate). This would allow the text to focus upon the major features and key differences, which is currently diluted by the inclusion of concentration ranges. More rigorous analysis and comparison with BLAST II data and other data (Oudot et al, 2002) would be useful, particularly as the BLAST II stations would be less impacted by inter-annual

variability in upwelling, dust and riverine input. Why only compare with the BLAST II data from below 1500m - how did it compare at shallower depths where the N<sub>2</sub>O max is found?

We agree with the referee that a comparison with BLAST II data only from below 1500m might be irritating. Thus we compared our data to those BLAST II data with correspond best in position and depths. For this we used the stations 1, 2, 4, 6, 7 and 8 of the BLAST II cruise, station 12 of the Gauss cruise, stations 190, 195, 196, and 197 of the Meteor 60-5 cruise and stations 20, 22, 24, 25, 26 and 27 of the Meteor 55 cruise. The mean N<sub>2</sub>O concentration difference is  $2.05 \pm 1.78$  nmol L<sup>-1</sup> (n=92). We changed the text of the ms accordingly.

Section 5.1. The comments above on 4.1.1 also apply to 5.1 which contains a few generalised points relating surface N<sub>2</sub>O to solubility. The most important sentence in 5.1 is derived from the published Walters et al (2004) paper, so they should remove this section and instead develop the discussion of the factors responsible for sub-surface isopycnal variation in  $\delta\text{IN}_2\text{O}$  in Section 5.2.

We don't agree with the referee. The chapter 5.1 completes the discussion about N<sub>2</sub>O in the water column, which also includes the surface layer. We also think that the factors influencing the N<sub>2</sub>O distribution were discussed in detail.

Abstract and Section 5.2. Although phrased as a "suggestion" in the abstract, Walter & colleagues neither show new evidence of nitrification, or extend and develop the current dogma that the  $\delta\text{IN}_2\text{O}$ -AOU relationship results from nitrification.

Of course the N<sub>2</sub>O data set presented in the ms gives no final evidence for nitrification. However, we found several strong hints for nitrification which are discussed in section 5. The abstract was modified according to the reviewer's suggestion.

As the analysis and discussion of nitrification as a source of N<sub>2</sub>O is brief this should not feature in the abstract.

We don't agree with the referee. Although nitrification was suggested as the probable production pathway of N<sub>2</sub>O in the North Atlantic by other authors before, this is an important result of our measurements and should be mentioned, too.

The discussion on temperature effects on nitrification is unclear - Walter & colleagues make the point that low temperatures may reduce bacterial activity (and so N<sub>2</sub>O production), but end by relating to bacterial abundance rather than activity. I strongly recommend that Walter & colleagues consider the role of pressure and mixing/dilution as these will be important factors determining the δ<sup>15</sup>N<sub>2</sub>O-AOU relationship (see Nevison et al, 2003).

In this part we discuss the uniform distribution of N<sub>2</sub>O in the water column of the cold-temperate North Atlantic. It is known that bacterial growth and production rates depend on temperature; however, the influence of temperature on nitrification is discussed controversially. Thus, it is possible that bacteria living in this region show comparable nitrification rates to those bacteria in warmer regions, but show lower growth rates and lower abundances due to the lower temperatures. Therefore, in our opinion, it is worse to mention this possibility to explain the uniform distribution in this region. And this uniform distribution over the complete water column is the reason why we think that pressure is negligible.

As a minor comment, in Section 2.2 Walter & colleagues note that the northerly transect was on the boundary of the subpolar and subtropical gyre, so this transect is clearly not typical subpolar water. I'd question the use of the term "sub-polar", which usually refers to the latitude band north of 50°N. As none of the stations are north of 50°N a more appropriate description is perhaps "temperate"?

We agree with the referee and changed the denomination "subpolar" to "cold-temperate" throughout the text.

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