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

Anonymous Referee #1

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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 delN2O, with removal of the text on surface N2O, to strengthen the interpretation and conclusions. Changes in presentation and description of the data would also improve the paper.

Section 3 - Re the calculation of delN2O, I applaud Walter & colleagues for addressing the issue that deeper waters did not originally equilibrate with N2O at current atmospheric concentrations when last ventilated. The standard approach of estimating delN2O using the current atmospheric N2O will lead to underestimates of N2O production since ventilation; however it should be borne in mind that if delN2O is used as an

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indicator of air-sea exchange upon future ventilation of the water then the final delN2O 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 N2O at ventilation, and comparison of the resulting delN2O with both the standard approach and their approach using an average N2O between the thermocline and 2000m. Although not the primary aim of this manuscript, they should provide more information on the sensitivity of delN2O to the different approaches.

Improvements in presentation and description of regional trends would benefit the paper. 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 delN2O associated with the Mid-Atlantic Ridge running from 0-50oN is based upon data at 10oN only. It would be more appropriate and informative to present the data as N2O 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 emphasising the high and low delN2O water masses. This would aid the discussion and provide insight into the role of mixing and dilution via comparison of the individual water mass delN2O signals on the three transects.

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 delN2O 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 N2O 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

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or dropped. As the major thrust is the isopycnal delN2O signal and its variation with latitude/longitude I recommend removal of Section 4.1.1 and expansion of the section on isopycnal delN2O variation.

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 N2O and delN2O 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, patricularly 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 N2O max is found?

Section 5.1. The comments above on 4.1.1 also apply to 5.1 which contains a few generalised points relating surface N2O 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 delN2O in Section 5.2.

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 delN2O-AOU relationship results from nitrification. As the analysis and discussion of nitrification as a source of N2O is brief this should not feature in the abstract. The discussion on temperature effects on nitrification is unclear - Walter & colleagues make the point that low temperatures may reduce bacterial activity (and so N2O production), but end by relating to bacterial abundance rather than activity. I strongly recommend that Walter & colleagues consider the role of pressure and mix-

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ing/dilution as these will be important factors determining the delN2O-AOU relationship (see Nevison et al, 2003).

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 50o. As none of the stations are north of 50oN a more appropriate description is perhaps "temperate"?

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

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