Biogeosciences Discuss., 12, C4234–C4237, 2015
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Biogeosciences Discussions
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Interactive comment on "Differences between coastal and open ocean distributions of N_2O in the oxygen minimum zone off Peru" *by* A. Kock et al.

Anonymous Referee #2

Received and published: 12 August 2015

General Comments: The article Differences between coastal and open ocean distributions of N2O in the oxygen minimum zone off Peru by Kock et al. presents an analysis of a large and important data set. The data spans a significant area and period of time in an important region for N2O and nitrogen cycling. Shelf regions are known to be important sources of N2O to the atmosphere and active regions of N2O cycling, and they are poorly represented in global datasets both spatially and temporally, so this work represents an important contribution in that respect. The authors provide some insightful interpretations of the large and complex data set based on a well-researched literature review and thoughtful analysis.

I feel that overall, the structural organization of ideas in the introduction and methods are clear, but the results and discussion are not as clear or well organized, and this

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makes it somewhat difficult to follow. This is likely due to the complexity of the data and the many approaches used during interpretation. However, some of the sentences refer to multiple ideas, and could probably be broken up into two or more sentences. Furthermore, some of the ideas put forward in the results/discussion section were not presented in the introduction, and I felt that some of the material in the discussion would be better situated in the introduction (e.g. evidence for increased n2o production following reventilation, and the link between sulfur cycling and n2o cycling). This would make it less surprising to see some of the ideas presented in the results/discussion section. I would like to see the article reorganized and clarified, with added emphasis and interpretation of the main findings.

Specific Comments: One of the most striking ideas is that the data suggest N2O cycling may be coupled to sulfur cycling, though I am surprised that this is not included in the abstract, and would like to see some figures that specifically show the relationship between O2, H2S and N2O. Is it surprising that N2O reduction only takes place in the presence of H2S, given the tendency of conventional O2 measurement techniques to overestimate O2 at low concentrations?

Another important finding is the inadequacy of a linear N2O/AOU ratio to describe N2O distributions in shelf waters, and this too could be further emphasized. I was under the impression that the N2O/AOU relationship is highly variable, even in the open ocean, and that it is therefore not advisable to estimate N2O concentrations from AOU alone. Nevertheless, the authors' conclusion that the N2O/AOU relationship is even less reliable in shelf regions underscores this, and I feel this should be emphasized in the abstract and body of the article.

The authors highlighted the relatively high variability over the shelf, and suggested that advection of N2O from other locations, localized upwelling or re-ventilation may be responsible for some of the deviations from the expected N2O/AOU relationship, but did not include sedimentary processes, riverine/estuarine inputs, or topography as possible explanations. Could these also play a role?

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Page 10173 Lines 1-5: I am curious about a few details of the analysis, some only for my own interest, but perhaps others should be included. What volume of gas was injected? Was there a second syringe to accept overflow? What was the final pressure in the vials? Were the vials weighed to confirm the volume of liquid in the vial during equilibration?

Page 10173 lines 10-15 : Can you list the concentrations of the gas mixtures, and how many were calibrated against the NOAA cylinders?

Page 10173 Line 22-23: Were any measurements excluded from the analysis due to large difference between replicates? If so, what threshold was used to determine this?

Page 10174 Lines 2-4: How did you determine that the maximum overestimate would be 17%? What are your assumptions (e.g. minimum initial concentration during the year when you expect the water last contacted the surface) Can you list the range of errors that this 17% overestimate could produce for your samples? Or give a general idea of how small the effect is?

Page 10175 line 2-3: Is the 'pump-CTD' water collected from the ships flow-through system?

Page 10176 Line 15 – is it true that N2O depletion coincided with high nitrate to phosphate ratios? I would have thought N2O depletion would coincide with low N:P ratios, consistent with N loss during denitrification?

Technical Corrections: I find some of the text in some figures to be too small to read (e.g. Figures 2, 3 and 4)

Page 10176 line 2 – consider replacing 'low N' and nitrate', with 'more negative N' and low nitrate'

Page 10178 line 1-2, consider replacing 'when oxygen reached suboxic', to 'when waters reached suboxic'

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Page 10178 Line 14: do you mean that N2O accumulation took place during and following the ventilation of water? Please clarify.

Page 10179 line 14: you say ‘at relatively low concentrations’, do you mean low oxygen concentrations or N2O concentrations? Please clarify.

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