Biogeosciences Discuss., 12, C4867–C4870, 2015
www.biogeosciences-discuss.net/12/C4867/2015/

Biogeosciences
Discussions
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12, C4867–C4870, 2015

Interactive comment on "Influence of mesoscale eddies on the distribution of nitrous oxide in the eastern tropical South Pacific" *by* D. L. Arévalo-Martínez et al.

Anonymous Referee #2

Received and published: 31 August 2015

Only recently has mesoscale eddies been recognized as having an important role for OMZ biogeochemistry. Accordingly, there are still only a handful of papers documenting that role, and this one is the first to discuss the role of eddies on N2O dynamics. Needles to say, understanding N2O dynamics in OMZs is particularly important given N2O's role as a greenhouse gas and sink for ozone combined with OMZ's (and adjacent coastal upwelling zones) as marine source regions to the atmosphere.

While the data presented in this paper make a significant contribution, I believe the authors have 'overplayed their hand' a bit and major revision is needed to bring interpretation and conclusions within the limitations of their observations. There are also portions of the text which are confusing with inferences that just don't seem to be logical

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Interactive Discussion

Discussion Paper

(see below).

The observational crux of the paper are the comparisons of observations between 1) eddy interiors and surrounding waters and 2) between the M90 and M91 observations. There are several issues that need to be resolved, the first being data quality assurance. While I acknowledge that the primary research group involved is internationally recognized for its N2O work, I am concerned about the N2O concentration data in Fig 3b. Here the center profile is lower than outside throughout the water column with the difference greatest at 1000 m where eddy influence should be minimal and I have similar concerns for Fig. C. I suggest double checking these data and if they stand up explicitly address this point in the text. Along these lines they are other key data comparisons that need to be explicit to the reader to be able to judge quality and robust of the inferences drawn by the authors. Since much is made of the temporal evolution of N2O in Eddy A, profile plots for comparing M90 and M91 data should be included. I have similar concerns about the gene abundance data as much it appears noisy and there is not visual comparison between M90 and M91 results. The text needs to include and evaluation of the reproducibility of these data.

The profile comparisons all use a depth scale. Eddies are characterized by raising or lowering of isopycnal surfaces and it would more accurate to make comparisons of properties between eddy interior and exterior in sigma-theta space. Having said this, a more general issue is that the station density for which N2O data are available are too sparse to well characterize distributions. The authors need to satisfy themselves with just establishing whether N2O concentration is significantly different inside eddies and admit that discussion of any mechanisms are speculative. In this regard, more statistical rigor is needed in terms of establishing an average background N2O profile for comparison and the authors have substantial data of their own to draw upon (e.g. Ryabenko et al., 2012). Because the distributions of N2O within the eddies are not well characterized, I don't see how there can be any certainty in the integrated values in Table 1. Clearly they cannot be taken as representative of the entire eddy. Even if representative of eddy center, it is unknown if the center represents the point of maximum difference (especially given the transect data in Fig. 4) regardless of whether the center was actually sampled. These problems also lead to difficulties in making comparison between M90 and M91 observations of Eddy A since differences are just as likely to be the result of sampling different portions of the eddy. This can explain why the NO3- deficit appeared to decrease between the two time points (see next).

I found the whole last section (pg 9256 line 20) of the Discussion, which assessed changes in integrated N-loss over time in Eddy A, rather confusing. First, after having shown N* data, a switch is made to "NO" to assess N deficits. N* relies of deviation from Redfield N:P and is the current standard so the switch to "NO" (which assumes a relationship with O2) is unclear. Perhaps it is because the N* scale in figure 3 is well beyond the bounds typically observed, but these calculations need to be rechecked as reasonable N* data for these cruises has been published. If the authors used N deficit data only from the stations with N2O data (not clear), then they still have the same issues here regarding insufficient sampling and characterization of the eddy. There are also logic gaps here as a reduction in N deficit could only come about by mixing with water with little or no N deficit. This parameter represents an integration of N-loss rate over time, but the authors interpret the apparent result as a change in rate. The apparent decrease in N deficit is probably due to 1) having sampled different regions of the eddy at each time point or 2) problems with using "NO" instead of N* as erroneously including any region with O2 present in the integration will reduce the deficit. Finally, this section has a lot of speculation about the processes producing N2O and corresponding yield that is not substantiated.

Other points

1) In many locations citations can be improved to include a broader selection of relevant literature (e.g. Frame and Casciotti, 2010; papers from Bess Ward's group) as well newer highly relevant literature that one or more of the authors are also co-authors of (e.g. Ryabenko et al., 2012.) In particular, Bourbonnais et al., 2015 (GBC) needs to be

referenced as they examine N deficit distributions in Eddy A during M90 and M90. 2) pg 9251 line 7-8, the claim about higher N2O in the center as opposed to other, within eddy locations is not well substantiated. 3) Pg. 9251 line 27, need to be careful not to confuse substantiated findings with hypotheses/speculation in prior papers. 4) Pg. 9253 line 5-10, not clear what is the basis of the assertion of lack of eddy impact on surface layer, as this depends on vertical velocity and exchange rates. Satellite Chl a often shows impact from eddy circulation. 5) Pg 9253 line14, not clear what is meant by "O2 minima" as the whole region as effectively zero O2 at depth. 6) Pg 9255, last line, appears to be confusion between 'concentration' and 'content', this may be behind the problem in #5. Content derives from depth or volume integrated parameters but local concentration is one factor determining rates of processes. The biogeochemical significance of depth integrated parameters can also be distorted by vortex stretching.

Interactive comment on Biogeosciences Discuss., 12, 9243, 2015.