Biogeosciences Discuss., 12, C8830–C8839, 2016
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Biogeosciences
Discussions

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.

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Received and published: 5 January 2016

Author's response to comments by Referee #2

On behalf of the authors I would like to thank Referee #2 for the comments to our manuscript. In general we agree with the reviewer in that some of our observations can be depicted more clearly and in that the interpretation of some results could be condensed and written more precisely, in particular considering the station density of our survey. On the following I will list our replies to punctual issues raised by Referee #2, indicating how they will be addressed in the revised manuscript.

Major points

Comment by Referee #2:

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.

Reply by authors:

In attention to the suggestion by the Referee #2, we thoroughly re-checked the N2O depth profiles depicted in Fig. 3. After revision we found that the anomalies in N2O concentrations do persist through the water column, and that indeed the size of the anomalies in these stations can also be observed for temperature (T), salinity (S) and oxygen. The Fig. 3 in the new version of the manuscript will include depth profiles of T and S in order to illustrate this point. Likewise, error bars have been added to the plots showing depth profiles of N2O concentrations in order to depict the uncertainties of the measurements. The findings of Stramma et al. [2013] suggest that although the strongest effect of the eddies could be found in the upper 600 m of the water column, the associated zonal and meridional velocities could also be detected at greater depths and therefore N2O anomalies could also be expected below this depth. Nevertheless, we do acknowledge that the spatial resolution of the N2O sampling as well as the actual location of the stations might complicate the interpretation: in this manuscript we presented data from selected stations across the three eddies in order to illustrate the conditions outside and inside of the corresponding eddy (the center being the location with the largest sea surface height anomaly (SSHA); see Stramma et al. [2013]). However, if we include stations that we consider intermediate between, for example, the center and the edge of a given eddy, this would change the magnitude of the anomaly (difference between center and outside) despite having a similar N2O distribution. Thus, although we agree with Referee #2, in that observational constraints make it difficult to judge in absolute terms the anomalies in the vertical distribution of N2O due to mesoscale eddies, our results show that, as for other biogeochemical properties, the physical changes within the eddy can also affect N2O in the water column, and in particular within the OMZ. Fig. 3 in the revised manuscript will include anomaly plots in order to better depict the differences in N2O concentrations between stations in the center of the eddy and stations outside.

Comment by Referee #2:

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.

Reply by authors:

Section plots directly comparing the vertical distribution of N2O, O2, T and S during M90 and M91 will be included in the new version of the manuscript in order to support the discussion presented in the text. A visual comparison of the vertical distribution of the molecular markers amoA, nirS and hzo between M90 and M91 was not included in the manuscript since the data from M91 was already presented within the context of a discussion on sources and sinks of N2O in the coastal eddy A (Fig. 5, section 4.2). Thus, we considered redundant to use these data again in the subsequent section. Furthermore, unlike M91, for the M90 cruise we only have molecular data from stations located at the center of the eddy. This is the reason why we decided to focus the discussion in those stations and present the results as integrated values in the water column. As pointed out by Referee #2, there might be some caveats with the use of

that approach and therefore this point will be now explicitly addressed in the revised version of the manuscript (see also comments/answers below).

Comment by Referee #2:

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

Reply by authors:

We agree with Referee #2 in that comparisons of properties across the eddies could be better described in terms of density surfaces. Therefore in the new version of the manuscript we use both density surfaces and depths to refer to the vertical distribution of N2O and other physical and biogeochemical properties. However, we will keep water depth as vertical axis in all plots since we consider that this is more intuitive for the reader.

Comment by Referee #2:

(...) 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).

Reply by authors:

As pointed out before, in this study we presented a selection of profiles which we identified to be located within the center, edge and outside of the corresponding eddy. The inclusion of anomalies for each eddy will thus provide a more clear view of the extent of the observed differences between N2O concentrations inside and outside of the eddies. In order to establish a background concentration of N2O for comparison purposes we will use the data from Kock et al. [2015, this issue], since it includes data collected during the same cruises in which we performed the field work of this study.

Reference: Kock, A., Arévalo-Martínez, D. L., Löscher, C. R., and Bange, H. W.: Differences between coastal and open ocean distributions of N2O in the oxygen minimum zone off Peru, Biogeosciences Discuss., 12, 10167-10193, 2015.

Comment by Referee #2:

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.

Reply by authors:

We acknowledge that estimating integrated concentrations in the water column based on single profiles could be ambiguous due to the station density we had, and therefore one should be cautious in interpreting the results. However, even under these observational constrains, and as suggested by Stramma et al. [2013], the mesoscale eddies which we tracked and sampled during M90 and M91 were stable structures whose center could be clearly identified based on the SSHA data. Hence, we think choosing the center of the eddies based on SSHA data (as we do in this study) is a save assumption which allows us to provide a fairly good description of the N2O distribution across these features. Although we don't have a station density that fully represents the N2O vertical structure of the eddies during M90 and M91 (which would be optimal), our estimates represent a good approximation to the distribution changes that can be observed under

the influence of recently formed (coastal) and aged (open ocean) eddies, in particular since we will provide a more robust analysis of background concentrations of N2O, and provide anomalies in addition to the concentration values. As for the comparison between M90 and M91, we acknowledge that different portions of the eddy could have been sampled despite the fact that the definition of "center" of the eddy was consistent. However, analysis of T and S profiles during M90 and M91 suggests that the water masses within the eddy were the same at the time of sampling. Although this has been shown before by Bourbonnais et al. [2015], we will include TS diagrams in the revised version of the manuscript in order to further support this argument.

Reference: Bourbonnais, A., Altabet, M. A., Charoenpong, C. N., Larkum, J., Hu, H., Bange, H. W., and Stramma, L.: N-loss isotope effects in the Peru oxygen minimum zone studied using a mesoscale eddy as a natural tracer experiment, Global. Biogeochem. Cycles, 29, doi:10.1002/2014GB005001, 2015.

Comment by Referee #2:

(. . .)This can explain why the NO3- deficit appeared to decrease between the two time points (see next).

Reply by authors:

Please see our reply in lines 180-183. Part of the interpretation in this section will be modified accordingly.

Comment by Referee #2:

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.

Reply by authors:

We used “NO”, a quasi-conservative water mass tracer in order to independently estimate denitrification based on the available data for eddy A. Since this geochemical approach has been used for previous studies in order to provide quantitative estimates of N-loss and N2O production during denitrification (see corresponding references in section 4.3 of the manuscript), we considered appropriate to use the same methodology in order to compare our results. In order to avoid further confusion we will explain this in the new version of the manuscript and still use the N* approach in section 4.2 which is focused in N2O-cycling during M91.

Comment by Referee #2:

(. . ..)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.

Reply by authors:

Many thanks for pointing this out. Indeed, after re-checking the numbers we found a slight mistake in the N* computation which in turn shifted all our values out of the normal range. Thus, although the features of the water column distribution of N* remain the same, the absolute values need to be corrected. This issue will be addressed by presenting the corrected values in Fig. 4 and adjusting accordingly the main text.

Comment by Referee #2:

(. . .) 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.

Reply by authors:

This section of the manuscript has been rewritten considering the replacement of “NO” for N*, and the necessity of correcting out N* values from the M91 cruise. Nevertheless, we do agree with Referee #2 in that a potential caveat of the interpretation of this data is the fact that we couldn’t sample exactly the same part of the eddy center during M90 and M91. In the revised manuscript we will address this issue in order to put in perspective the N deficit changes between the two surveys and their likely impact in the vertical distribution of N2O. We also strengthen our arguments by reporting data of additional stations in which we did not sample for N2O but for nutrients and oxygen.

Comment by Referee #2:

(. . .) Finally, this section has a lot of speculation about the processes producing N2O and corresponding yield that is not substantiated.

Reply by authors:

We agree with Referee #2 in that some of the arguments provided cannot be supported with our data. As it is written however, our discussion warns the reader about it and furthermore highlights the need for multidisciplinary, highly resolved surveys in order to better understand the net impact of mesoscale eddies in the distribution of N2O, in particular when longer time scales (seasonal to interannual) are considered. This part of the discussion will be substantially shortened and only the main aspects that can be tied to our observational data will be included.

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.

Reply by authors: References will be added as suggested.

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.

Reply by authors: Sentence will be rewritten for more clarity

3) pg. 9251 line 27, need to be careful not to confuse substantiated findings with hypotheses/speculation in prior papers.

Reply by authors: Sentence will be rewritten for more clarity.

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.

Reply by authors: Given that enhanced concentrations of N2O can be found within the upper oxycline of the ETSP (e.g. Fig. 3), shoaling of the thermocline within mode water eddies would mean higher N2O concentrations for a given depth as compared to a background profile. Since coastal upwelling waters off Peru are a known source of extremely high N2O concentrations to the surface, one could get the impression that this eddy-driven shoaling of waters with relatively high N2O could also contribute to that effect. However, our observations show that the anomalies caused by these eddies seemed to be far from the reach of surface waters and thus did not contribute to the N2O fluxes out of the ocean at this location. Independent verification of our bottle data for the surface comes from underway measurements performed during the same cruises in the ETSP (see Arévalo-Martínez et al. [2015]). From these data we can say that there wasn't any appreciable variation of N2O concentrations in the surface during the several cross-eddy sections carried out in the M90 and M91 cruises.

Reference: Arévalo-Martínez, D. L., Kock, A., Löscher, C. R., Schmitz, R. A., and

Bange, H. W.: Massive nitrous oxide emissions from the tropical South Pacific Ocean, Nature Geosci., 8, 530-533, doi:10.1038/ngeo2469, 2015.

5) pg 9253 line14, not clear what is meant by "O2 minima" as the whole region as effectively zero O2 at depth.

Reply by authors: In this context O2 minima means the core of the OMZ (i.e. O2 concentrations < 5 μmol L-1). This will be explicitly stated in the revised version of the manuscript.

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.

Reply by authors: The word "concentration" has been replaced as suggested because in this context we meant to discuss the N2O, O2 and nutrient content in the whole water column. Likewise we agree with Referee #2 in that the apparent content of a given biogeochemical property can change due to the shoaling/deepening of isoclines. However, we assume this effect to be marginal for the time scales considered in this study.

Kind regards,

Damian L. Arévalo-Martínez

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