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Interactive Comment

Interactive comment on "Biogeochemical characteristics of a long-lived anticyclonic eddy in the eastern South Pacific Ocean" *by* M. Cornejo et al.

M. Cornejo et al.

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We thank the reviewer for his/her time and constructive comments. We carefully read each of the comments and will address them as stated below:

First, the premise that eddies constitute hotspots of denitrification is still not robustly confirmed in this work, although the evidence presented supports the idea. This is because the conclusion is based largely on one observation in a single vertical profile and some comparisons between an observation made in austral fall 2011 and measurements retrieved from observations in June 2010 (nearly 3 months after the estimated time of formation for the eddy under study). While there is substantiating

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evidence of N2O consumption and production, rates were not directly determined and the observation of high nitrite concentrations was not seen at any other depths.

The reviewer raises a valid concern. Although denitrification rates were not directly measured by experiments, the sampled variables in the center of the eddy, such as extremely low oxygen concentrations, nitrogen deficit, nitrite accumulation and N2O subsaturation in the core of the OMZ of the eddy are four proxies which, combined, strongly suggest the presence of active denitrification, such those observed in the Oxygen Minimum Zones. However, as the reviewer says, is only one sampled profile in this eddy, so we had the precaution to say that, and that this issue needs to have further studies. About the nitrite accumulation observed not in whole OMZ, this is a typical vertical pattern observed in other OMZs, where this nitrogen species is accumulated just at the top of the OMZ, which depends on several factors, such as organic matter and nitrate concentrations. The same is reported for the N2O subsaturations (Codispoti et al., 1985, Zamora et al., 2012)

The figure does not show standard error/standard deviation of replicate samples for nitrite or N2O values.

This is an important comment. We will add standard error to the figure.

Similarly, the fact that rates of change in biogeochemical properties (dissolved oxygen) were calculated based on estimated conditions at the time of origin imparts a degree of uncertainty on the rate magnitudes.

For that reason we referred to the calculations as ‘back of the envelope’ type calculations and cautioned in the text that they should be taken with a grain of salt.

The significant separation in space and time between observations at the study site (E03) and the glider data calls into question how representative the comparisons are when calculating rates of change. The authors do a reasonable job of reconstructing the likely point of origin of the mesoscale eddy they sampled at E03, but making biogeochemical observations 3 months apart from the time the eddy likely formed (June vs. April) may not be appropriate for characterizing source water characteristics and determining rates of change. Perhaps more discussion about the possible ranges of observations would be helpful here.

The reviewer has right about the time between sampling by glider and the origin time of the eddy (although it was 35 days of difference and not two month). However, we wanted to have a rough estimation about the consumption rate. For that reason, we used the oxygen data (from glider) from the origin formation zone. The oxygen profiles of intrathermocline eddy from Hormazabal et al (2013) show similar oxygen concentrations that we measured by glider, so we took these values as characteristics from the eddy formation zone. We were very careful to specify that our calculations are rough and we need more studies in order to estimate more precisely these rates.

The authors did not consider the potential importance of convective mixing during austral winter, which could influence O2 and N2O concentrations in the source waters; the choice of initial values could affect calculated estimates of production/consumption rates of biogeochemical constituents.

No, with our data we can not consider the mixing. However, we recognize that this process should be very important. For that reason, we mentioned that our oxygen and nitrous oxide consumption rates are probably underestimated, if we consider that there are mixing of oxygen and other gases.

The authors indicate that measurements were taken from the subsurface within the source waters, which included depths of 104âAËŸT352 m. Were the O2 values taken from the ËĞ deeper depths, or the shallower of these depths?

As it is mentioned in the manuscript, the oxygen values were taken by CTD as a profile from surface to 1000m.

What is the winter mixed layer depth in these waters? It is unclear from the presentation of the data whether the subsurface may have been influenced by ventilation. Specific comments Introduction The introduction is well written (although the first paragraph is a bit long and contains several different ideas) and the motivation for the work is clearly outlined.

This region is not characterized by a deep winter mixed layer (20-30 m in the coastal station 18, COPAS program, Sobarzo et al., 2007). Offshore, the mixed layer could be deeper (50-90m) in winter. In order to illustrate the mixed layer in winter in the region we plotted four density vertical profiles at different locations which were taken between may and august (Figure 1). See also our response to a previous comment concerning not including mixing processes above.

The introduction would benefit from some description of the properties of water masses in the region that are later referred to as the source region for the mesoscale eddy. For example, in the last paragraph of the introduction the nutrient properties of the ESSW are discussed, but not the temperature/salinity signatures. This would be a good place to describe the fact that mesoscale eddies in this region are typically found within the thermocline and that they are characterized by salty, warm waters relative to the surroundings.

Good suggestion. we added this information in the text.

Methods How were the N2O samples collected? Was special attention paid to collecting gas samples? We added a description of sample collection in the methods as follows: "N2O samples (20mL) were taken after the dissolved inorganic carbon samples by a tygon tubing avoiding the bubble formation. The samples were quickly fixed with 50 μL of saturated mercuric chloride and stored in the dark"

What volume of water is typically incorporated into an eddy formed off the coast? Although the volume of water transport by an eddy is variable, there is some estimation which calculate this volume as high as 1.2 Sv (Hormazabal et al., 2013). In our manuscript, We calculated this volume according with the diameter of the sampled

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eddy seen by altimetry, and the thick of salty and low oxygen waters.

How deep is the mixed layer in austral fall and winter?

See response to similar question above.

What was the vertical profiling regime of the glider?

The vertical profiling regime of the glider is detailed at Pizarro et al., (accepted; 10.1175/BAMS-D-14-00040.1). The transect was a length about 150-180 km. The glider had a speed of 40 cm s-1 and a dive angle of 91 26°. The glider can reach 1000 m depth.

To keep the method section short and precise we chose not to include this type of information, but We will included the reference in the manuscript. We will include it if the editor finds it necessary.

Approximately what volume of water is incorporated into a newly formed eddy?

The volume estimated to be incorporated into a newly formed eddy is about 0.7-1.2 Sv

What is the typical mixed layer depth during austral fall and winter?

See our response to the same question above

Results Discussion Figures Fig. 1: It would be helpful to show the N_2O vertical profile in terms of percent saturation, in addition to the values given (since the undersaturation is referred to in the text).

We included N_2O saturations of the station E03 in the figure 6b.

Also, why were nitrite data from E02 not included?

We have only one depth (surface) with nutrient samples for nutrients in this station. We will include this point in the figure.

Fig. 4: This figure is a bit misleading, since the transect stations were obtained qua-

sisynoptically and associated with the SLA map; however, the eddy trajectory and point of origin come from different points in time relative to the 2011 transect. Therefore, the eddy trajectory doesn't match up with what the SLA would have looked like during the eddy's westward journey into the open ocean. It might be better to show this in two figures, one with the transect overlaid on the SLA and another with the eddy trajectory and likely point of origin.

See the response to the same issue above.

We included here the figure of altimetry at the moment of the glider transect (Figure 2).

Fig. 6: The right hand plot seems to be missing a caption.

The caption of the figure 6b was included.

Fig. 7: From the contour of sigma-theta, it looks like the eddy nearest shore was cyclonic, rather than anticyclonic. The authors do not comment on this in the text.

It is possible that that eddy was cyclonic. However, the work is referred to the eddy seen at 90°W, which was low in oxygen and presented low N* concentrations. This eddy was selected because it was one of the few stations with nitrite concentrations. Also, we wanted to show that eddies are a common pattern in the Eastern South Pacific.

Interactive comment on Biogeosciences Discuss., 12, 14481, 2015.

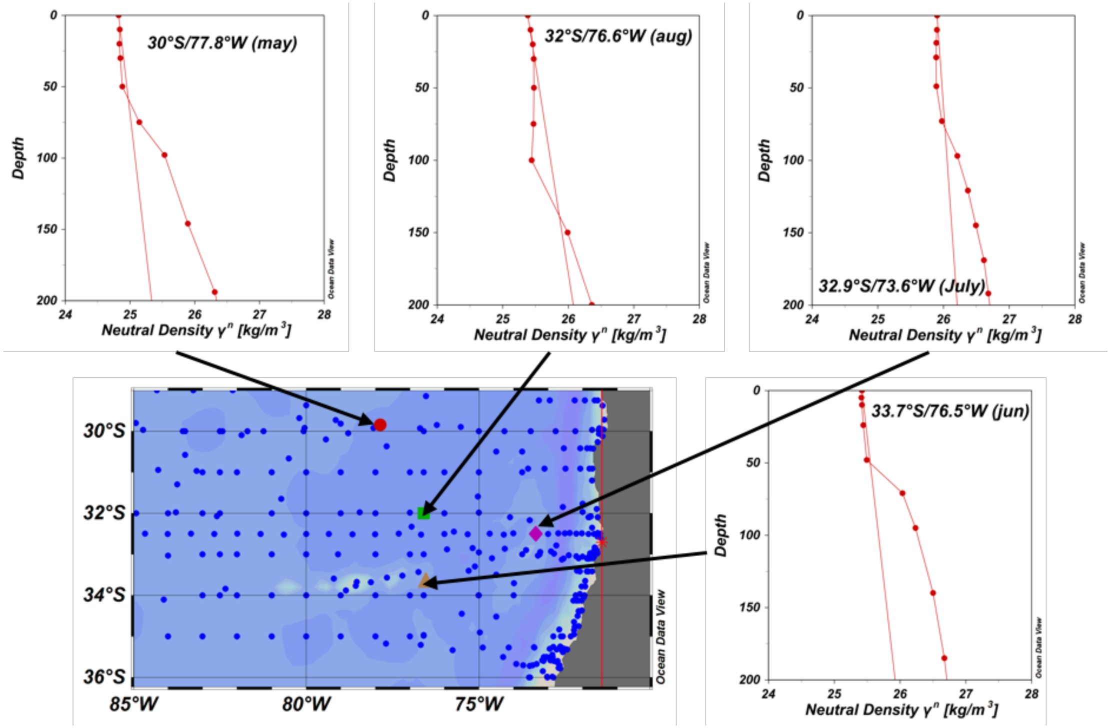
30°S/77.8°W (may)
32°S/76.6°W (aug)
32.9°S/73.6°W (July)
33.7°S/76.5°W (jun)
Depth
Neutral Density γ^n [kg/m^3]
Ocean Data View
30°S
32°S
34°S
36°S
85°W
80°W
75°W

Fig. 1.

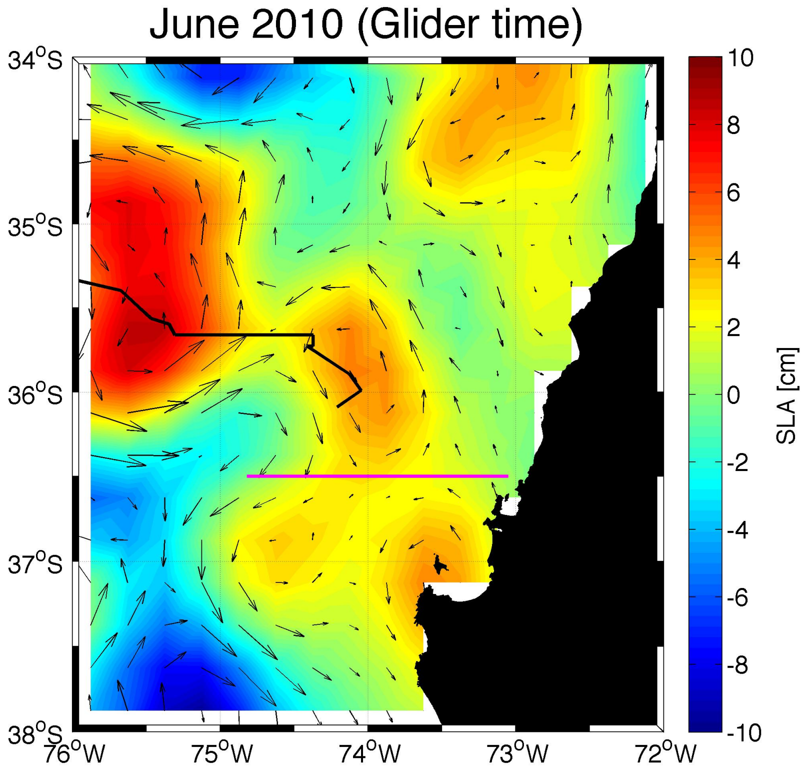

Fig. 2.

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