

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

Anonymous Referee #3

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General comments

Cornejo and co-authors describe the results from a field survey of the Eastern South Pacific (ESP) Ocean conducted as part of the Tara Oceans expedition in austral autumn, 2011. The transect of 11 stations included CTD measurements, dissolved nutrients (NO₃⁻, NO₂⁻, and PO₄₃⁻), dissolved O₂ and N₂O. Observations of a subsurface peak in nitrite coincident with undersaturation of N₂O in a water mass that was anomalously salty and warm with respect to WOCE-derived climatologies at one station (E03) prompted analysis of the effect of mesoscale eddies on transport of water masses and transformations of nutrients within them during offshore migration of anticyclonic

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eddies.

The manuscript is well presented: it is easy to follow and the motivation for the work is clear. The figures and tables are generally effective and the methods appropriate. That being said, I have some concerns about the paper. 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 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 figure does not show standard error/standard deviation of replicate samples for nitrite or N₂O values. 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.

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 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. The authors indicate that measurements were taken from the subsurface within the source

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waters, which included depths of 104–352 m. Were the O₂ values taken from the deeper depths, or the shallower of these depths? 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. 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.

Methods

How were the N₂O samples collected? Was special attention paid to collecting gas samples?

What volume of water is typically incorporated into an eddy formed off the coast? How deep is the mixed layer in austral fall and winter?

What was the vertical profiling regime of the glider?

Approximately what volume of water is incorporated into a newly formed eddy? What is the typical mixed layer depth during austral fall and winter?

Results & Discussion

Figures

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Fig. 1: It would be helpful to show the N₂O vertical profile in terms of percent saturation, in addition to the values given (since the undersaturation is referred to in the text). Also, why were nitrite data from E02 not included?

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

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

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.

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