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Biogeosciences Discussions

Interactive comment on "On the role of mesoscale eddies for the biological productivity and biogeochemistry in the eastern tropical Pacific Ocean off Peru" *by* L. Stramma et al.

Anonymous Referee #2

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This manuscript addresses two themes of considerable topical interest in recent years, the impact of mesoscale physics on biogeochemistry and the dynamics of oxygen minimum zones. Diverse biogeochemical analyses of 3 eddies in the eastern tropical Pacific provide a basis for a conclusion that eddies exert a significant control on the dynamics of the region. There is much here of interest but I feel that the manuscript could be significantly strengthened if a more thorough interpretation of results were carried out.

In additional to minor technical comments that I list below I have a number of more significant general comments that I hope will be of use in revising the manuscript.

A key component of the manuscript is comparison: of one eddy to another and of an eddy to background values. Although some of the evidence for differences is already compelling there are some important ways that I think this should be strengthened.

First, the authors choose to use profiles immediately outside eddies as background reference. Where an eddy ends is a difficult thing to judge and there are instances where it looks like the 'background' profile is not outside e.g. for eddy C in Fig 8. In order to have confidence in the background 'baseline' that the authors are using for comparison I recommend the use of a common background profile for all eddies. More specifically, there should be a single figure of property versus depth for all profiles currently used in the paper for 'background'. If they are truly background, they should all be similar and an average of them them constitutes a more robust background for comparison to eddy profiles. If a hitherto used background profile differs significantly from others then it is an indication that it may not be sufficiently outside the eddy to be used for comparison. If there is evidence that the background profile changes across the region e.g. from coast to open ocean, then there are many profiles not currently used in the manuscript, judging from Fig 1, and these should be used to construct a more robust background for each eddy's immediate location.

Second, when comparing the two mode water eddies an assumption is made that B is an older 'version' of A. While this seems a sensible hypothesis, little is done to justify it despite the fact that the conclusion of decreased activity with age rests upon it. On the simplest level this simply requires a quantitative discussion of things such as salinity of core and the density surface that the anomaly resides on to demonstrate they are consistent. However, I also have some concern over the analysis of eddy A. The velocity components shown in Fig 2 do not seem to be consistent with the flow around an eddy. For an eddy, the peak velocity in the along-transect component should be at the eddy core (assuming no transect will ever perfectly go through the eddy centre). However, both components demonstrate a dipole structure, flipping from positive to negative either side of the core. This is more consistent with a jet than an eddy. Only one velocity field is shown for B.

Third, particularly given the comment above about the velocity structure of eddy A, more care needs to be exercised extrapolating general results from studies of just 2 mode water eddies. Without knowing how much variability is seen in the biogeochemistry and physics of the mode water eddies in the area, comments inferring changes due to ageing need to be balanced with a discussion of the uncertainties. As part of this, greater use of the literature would be helpful. The popular eddy-pumping hypothesis is used already but eddies can influence local biogeochemistry in many more ways: see for example work by Amala Mahadevan, Marina Levy and Leif Thomas in addition to other work by Dennis McGillicuddy and Patrice Klein. Furthermore, there is currently no discussion of how conclusions may have been affected by the transects not passing through the centre of the eddy, which could bias the estimate of properties associated with it.

Technical comments: a. there should be consistency in the figures presented for the 3 eddies. Figs 2, 7, and 8 should show all the same fields (as Fig 2 preferably) or have reasons given for why the data weren't available.

b. 3rd paragraph of introduction would be clearer if it was broken up into shorter sentences.

c. a definition/description of a 'typical mode water eddy' would be useful as context e.g. for p9182, lines 19-20

d. dates need to be given for each of the transects presented

e. p9185, lines 23-24: a brief description of the method is still needed here e.g. is extent of eddy determined from density or velocity structure?

f. it would be useful to also have bathymmetry on Fig.1

g. p9188, lines 3-4: what is the justification for this definition of eddy extent? Why should an eddy be considered 'shorter' just because it is moving? More generally, is

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swirl velocity a reliable indicator of the size of a mode water eddy. If there is a barotropic mode then the swirl will go much deeper than the displaceemnt of isopycnals and the latter are more closely linked to properties such as heat and tracer anomalies.

h. a little more explanation is needed when comparing observations to the Chagneau et al. 2011 paper. In the latter velocities are determined from geostrophy. In this manuscript they are observed directly using ADCP and will include other contributions in addition to the geostrophic component. As a further comment, the argument put forward on p9192 lines 5-9 does not explain all of the discrepancy.

i. p9193, lines 11-15: the definition used for the eddy extent of U/c>1 is also the criteria for an eddy to trap its contents. Hence, according to the standard interpretation of 'eddy-pumping' there should be no continued supply of nutrients to the euphotic zone within the eddy. Instead, the decaying signal would be due to the original trapped nutrient supply being consumed (if other dynamical process were ignored).

j. p9193, lines19-20: this statement needs quantification to support it. What is the primary production within eddies and what is that for the area?

k. p9190, line 22: Fig 3 does not seem to support the claim about silicate anomalies.

l. Fig 1 would seem to imply that transect A1 missed the eddy core by some margin

m. the depth of the oxygen anomaly for eddy B seems difficult to reconcile between Figs 3 and 7.

n. Fig. 3: more tick marks needed on x-axes. Also need to state which transects/CTD casts were used to calculate these anomalies.

o. Fig.4 would benefit from having density contours added.

p. Fig.6: the left hand panels suggest that the eddy is centred at ∼76.6W with edge of core at 76W. This would mean that the right hand panels are almost all outside the eddy core.

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q. Fig.7: velocity structure seems noisy and not very coherent. Has the transect missed the core?

Interactive comment on Biogeosciences Discuss., 10, 9179, 2013.