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Biogeosciences Discussions

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Interactive Comment

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 #1

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SUMMARY

This MS focuses on the hydrographic and biogeochemical characterization of 3 mesoscale eddies located in the eastern tropical Pacific (ETSP) off Peru (ca. 15-18 °S) with the aim of understanding the implications of these features on nutrient distribution and biological productivity in the region. For this purpose, data were obtained during one oceanographic cruise with the opportunity to sample eddies (November 2012); also, satellite data were used to characterize the location and evolution of selected eddies. The main results described by the authors (Abstract) are the following: a) larger temperature and salinity anomalies in the eddies, as well as swirl velocities, compared to previous mean estimates in the ETSP; b) larger heat (AHA) and salt (ASA) anoma-

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lies in the eddies compared to mean values from previous estimates; c) significant contribution to productivity indicated by pronounced subsurface maxima of chlorophyll in the eddies; d) aging of eddies from the coast to open ocean considerabley influences their properties (weaker chlorophyll maxima, subduction of nutrients); e) the role of eddies as hotspots of fixed N loss would depend on the type of eddy.

BACKGROUND

Mesoscale eddies in coastal transition zones (CTZs) have engaged our attention during the last decade because of their potential to transfer heat, salt, nutrients, and inorganic and organic matter from the coastal, productive, ocean to the open, oligotrophic ocean. The characterization of these features and the evaluation of their impacts on biological communities and biogeochemical cycles have mostly been analyzed through satellite and/or modeling approaches whereas in situ observational/experimental data have been less frequently obtained through profiling buoys (hydrographic sensors) and, in still too few cases, dedicated oceanographic cruises.

In the eastern South Pacific (ESP) region, Chaigneau et al. (2008, 2009) made a comprehensive characterization of mesoscale eddies using satellite altimetry data. The limitation in using this approach is that only the surface characteristics of eddies can be described and, therefore, subsurface anticyclonic eddies (also, known as subthermocline, intrathermocline or mode water eddies) may be overlooked (poorer surface signature) or be confused with surface anticyclonic eddies (same surface signature). Recently, Chaigneau et al. (2011) provided an even more complete description of eddies in the ESP by combining satellite and buoy-profiling data. They estimated the mean characteristics of hundreds of cyclonic (CE) and anticyclonic eddies (AE) in the region; they also reported standard deviations, most of which were relatively large. In parallel, the work by Johnson & McTaggart (2010) described subsurface eddies in the region using profiling buoys.

In terms of the impact of mesoscale eddies in the ESP region, some of the authors in this MS recently published an article which refers to the discovery of a N-loss hotspot in the Oxygen Minimum Zone (OMZ) associated with a coastally-trapped mesoscale eddy in the area off Peru (Altabet et al., 2012). Also for this region, Correa-Ramirez et al. (2007) and Morales et al. (2012) have described the role of mesoscale eddies in terms of phytoplankton biomass distribution and productivity in the CTZ.

GENERAL COMMENTS

This MS presents valuable in situ data on mesoscale eddies in the ESP region and, in this sense, it contributes to new knowledge in the field. There are, however, two main themes contained in this MS that deserve further analyses/revision before publication in this or another journal.

1. Characterization of mesoscale eddies and comparison with previous studies in the ESP region

   In describing each of the 3 selected eddies, the authors implicitly assume that their dynamics are more or less independent of other concurrent processes. For example, the authors did not consider the potential influence of submesoscale processes on the dynamics of eddies. In the case of the CE (Fig. 8), it is apparent that eddy-eddy interaction is occurring. The bending of isolines on the left side of the CE appears to be the result of an AE (downward bending) located next to this CE (interpreted as upward bending). On the other hand, the bending of isolines on the right side of the CE was less evident in the 600 m water column (asymmetry), suggesting that CE was mostly a surface feature (<200 m depth); the distribution of dissolved oxygen in particular suggests this is the case (Fig. 8). In contrast, the authors argue that this CE had a vertical extent of 600 m (Table 1), 3 times more than the mean value suggested by the study of Chaigneau et al. (2011; CH11 from now on). Furthermore, they state that this CE "is quite unusual given its elongated shape and should not be considered the typical cyclonic eddy". The vertical extent reported for this CE is the cause of almost an

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order of magnitude higher AHA and ASA values compared to the mean in CH11. Altogether, I think that the description/analysis of CE must be reconsidered.

In the characterization of the 3 eddies, it is most important to analyze their evolution (age, trajectory, size variations, etc.) and the location of eddy generation, using satellite data, in order to understand their structure at the time of in situ sampling. The authors estimated the age of each eddy but not in all the cases were the coordinates of origin, size (diameter) variation, or detailed trajectories mentioned. For example, they indicate that the oceanic AE was first detected at 78°15'W on August 2012 but no latitudinal position is provided. On the other hand, they indicate that the coastal AE remained for 2 months in the coastal area (stationary eddy). I think this is a very relevant piece of information in terms of its dynamics and impacts compared to other eddies which readily moved offshore. The authors, however, do not discuss this point, nor do they provide information on the meteorological-oceanographic setting under which the selected eddies (or this AE eddy in particular) were evolving until the sampling took place.

An important part of this MS refers to a comparison of the hydrographic characteristics and dynamics of the 3 eddies with the corresponding mean values described in CH11. The results of the comparison are not surprising because the mean values in CH11 were derived from a very large number of eddies and large deviations from the mean values were found. Moreover, CH11 described the mean characteristics of cyclonic eddies (CE) and anticyclonic eddies (AE) at the core of the features (150 m for CE and 400 m for AE). At the same time, eddies are highly dynamic features which change their characteristics (evolution) through their lifespan and, with it, will change their impacts. On the other hand, the estimates of anomalies in this study were computed in a relatively simple way: a comparison between a station within the eddy with 2 selected stations around the eddy; this makes the estimation highly dependent on the choice of the outside station. For example, it is not clear to me how this was calculated; as a mean value between the two outside stations perhaps? When there is a marked asymmetry on both sides of an eddy, as was the case in CE (Fig 8) and generated by a neighboring AE, this calculation can be heavily biased. In addition, the authors did not use an automated eddy identification method to characterize the size or borders of each eddy.

2. Contribution of eddies to an enhancement of biological production and N-loss

   The offshore transport of chlorophyll by the coastal AE is clearly associated with a small filament at its northern border (Fig. 5), exemplifying the interaction between mesoscale or submesoscale structures/processes. In contrast, mesoscale eddies contribute to local increases in chlorophyll via Ekman pumping of nutrients to shallower depths (Fig. 4). The authors assume that if chlorophyll concentration can be taken as a (qualitative) indicator for primary productivity, their results suggest that mesoscale eddies off the coast of Peru contribute significantly to overall productivity. I have no doubt that mesoscale eddies have that potential but I think that there is no demonstration in this MS of a "significant contribution".

   The authors also conclude that the aging of mode water eddies is associated with a significant subduction of nutrients within eddies (from 50 to 150 m depth), suggesting that these type of eddies may reduce the adjacent open ocean productivity off Peru. I think this is contradictory because the coastal AE (Fig. 2) had almost no NO3 in the top 200 m whereas the oceanic AE (Fig. 7) had higher NO3 concentrations in that range. Moreover, the authors say that their "finding supports the model results from Gruber et al. (2011)". The main proposition of Gruber et al. (2011) was that mesoscale eddies in eastern boundary currents contribute to reduce the productivity in the coastal upwelling zone by transporting nutrient-rich waters away from it via mesoscale eddies. On the other hand, this implies that the eddies would fuel primary productivity in the CTZ via upwelling (eg. eddy pumping) in these eddies as they travel offshore. The result on N-loss in the coastal AE in this study (and in Altabet et al., 2012) is the most interest-

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ing bit of information since it implies that, under certain conditions, eddies will contribute to enhance N-loss before they move offshore.

In terms of phytoplankton biomass, the authors used non-calibrated chlorophyll-a data derived from a fluorescence sensor; instead, they used the calibration provided with the instrument to transform the values. I think it is preferable to use the fluorescence data in relative units rather than the conversion because the values obtained are quite high and may get wrongly cited in future papers. There is, however, no explanation in this MS concerning the problem in having made these measurements during the cruise, but maybe it was not a cruise dedicated to the study of eddy impacts. In the same way, no ammonium data are provided and if the idea is to estimate N-loss from the system, I cannot understand why this measurement is not included (the same is true in the paper by Altabet et al., 2012).For example, Fernandez et al. (2009) found that the contribution of N regenerating processes to primary production in terms of DIN could represent as much as 50% of NH4 assimilated in surface waters (through ammonium regeneration) as well as a variable fraction (2–16%) of nitrate through nitrification in coastal upwelling waters off Peru. These findings have to be incorporated in the discussion of the impacts of fixed N loss in the CTZ ecosystems off Peru or ESP in general.

TECHNICAL COMMENTS

Abstract

General: This section can be greatly improved by adding values/significance levels, etc. to the main results (only two are provided). For example, ”. . . a much larger variability than the mean AHA and ASA . . . ; “. . . eddies contributed significantly to productivity . . . ”; “chlorophyll maxima are weaker . . . ”; or “. . . hotspot of N loss..” are not informative in quantitative terms.

Introduction

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This section can be greatly improved by making a better connection between the paragraphs; it was difficult for me to follow the line of thinking leading to the focus of the paper. The first paragraph (general characteristics of eddies) should be followed by the 4^{th} paragraph (previous studies on eddies in the ESP region) for continuity of the theme. The 2nd paragraph (factors governing primary production, etc.) is too general and disconnects the adjacent paragraphs. The 5^{th} paragraph (connection between eddies and low oxygen conditions in the region) should follow (as modified 3^{rd} paragraph) but lines 20 to 27 are out of place and should be moved to the first, most general paragraph (non-linearity of eddies). Paragraph 6 (eddy characteristics in the ESP) appears disconnected from the 4^{th} paragraph when they are strongly related. The most relevant characteristics of coastal upwelling off Peru (3^{rd} paragraph) in terms of productivity and low oxygen-N cycling can be placed in connection with the modified 3^{rd} paragraph suggested above.

<u>Observational data</u>

General: There is no description here of the use of an objective method for the identification of eddies and their boundaries (eg. Chaigneau et al., 2008; Kurczyn et al., 2012); also, no description is given of the method used in the estimation of the age of the selected eddies, later referred to in Results.

Page 5,

Line 27: include the dates when the eddies were sampled

Page 6,

Lines 16-19: chl-a calibration – “the absolute values may have a somewhat large uncertainty”; please use relative values then as more appropriate

Line 20: explain why NH4 data were not included if N-loss has to do with the dynamics of this and other N-compounds

Line 31: “Aviso”, use capital letters: AVISO

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Page 7,

Line 1: correct "spacial" for "spatial";

Line 2: specify the frequency of the SSHA data (eg. daily, weekly, other)

Line 3: a grid should be referred as 0.25°x0.25°;

Line 8: grid cell 1/24°x1/24°

Results

General: Comparisons with the results of Chaigneau et al. (2011) and other publications should be placed in the Discussion section; in fact, some of the sentences here are repeated in the next section.

Page 7,

Lines 11-17: section 3.1 includes a short paragraph on the AE which are subsequently detailed in sections 3.2 (coastal) and 3.3 (oceanic), followed by section 3.4 which includes the CE. Instead of such a short section 3.1, I suggest starting the Results with a general sentence on the presence of several mesoscale eddies in the region of study, the position of the 3 selected eddies, and their age. The inclusion of the general atmospheric/oceanographic conditions in the area of study during the survey (eg. in terms of wind stress and wind stress curl, and/or SST distribution) would have provided a wider perspective on the setting under which eddies were sampled. Moreover, I strongly suggest including a subfigure in Fig. 1 on the mean distribution of satellite chlorophyll-a in the area of study (only partly represented in Fig. 5), together with the 200 m isobath.

Line 20: the sentence "Anticyclonic eddy A had already separated from the shelf in late November 2012" is confusing. First, mesoscale eddies in eastern boundary currents are usually not formed over the shelf but at the shelf-break; second, the age of the eddy and its movements is information relevant here to understand its dynamics. Instead, this information is provided on Page 8 (lines: 20-30).

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Page 8,

Line 1: “typical for a mode-water eddy” does not need a reference here since this is already in the Introduction section.

Line 20: “a very large chlorophyll maximum”, specify what represents “very large”.

Lines 26-30: this aspect should be moved to the first part of Results so as to justify the categorization as “young” and “old” eddies (section 3.1, see comment above). Coastal eddy was 2 months old!

Page 9,

Line 1: “The two anticyclonic eddies subject to our study . . . ” but this section is focused on one of them.

Line 13: “this eddy was separated already from the coast given that near the shelf the southward flowing Peru-Chile Undercurrent flows . . . ”; as before, the PCU flows along the shelf-break area and if it is the source of mode-water eddies, AEs would necessarily be found at depths greater than the shelf.

Line 19: this sentence should be modified for clarity: “Nitrite concentrations levels near the shelf and in the anticyclone at 50 m to 400 m depth were very high as a consequence of the productive shelf waters off Peru having the highest nitrogen transformation rates due to anammox processes (Kalvelage et al., 2013).” First, in the open ocean, concentrations increase at about 100 m depth, not 50 m; second, define “very high” by adding a value in parenthesis; third, the explanation and reference included here pertain to the next section. However, I do not understand the point; anammox is supposed to explain the observed high NO2 levels but, in fact, this process consumes NO2. Also, the panels on the right in this Fig. are not really showing part of the AE (see below).

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General: repetition with the Results section in several places

Page 12,

Lines 18-19: the stationarity of this AE is not at all discussed and I think it is an important part of the story dealing with intensive N-loss.

Page 13,

Lines 14-18: lower chlorophyll - decreasing eddy pumping; too much of a qualitative statement. In Fig. 4, for example, how do you explain that AE-B had higher concentrations of chlorophyll around the area of the eddy compared with the coastal AE in A1 and A2?

Page 14,

Lines 26-this paragraph: N* calculations and interpretations should e part of the Results section

Figures

In general, you need to use uniform scales for all the variables in the different figures

Fig. 1: it would be most helpful to have a better resolution of the axis with the coordinates

Fig. 5: the distribution of chlorophyll-a can be better represented.

Fig. 6: the only graph where different scales were used for the left and right panels; as a result, it is difficult to interpret it. The reason (page 9, lines 16-17) is that no nutrients were taken for part of section A2. However, the panel on the right does not contain the AE positioned at around 76.5°W, so I do not understand the point of showing these data when the main focus are the eddies.

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