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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.

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Enclosed is our reply to the reviewer 1 for ms BG-2013-255: Reply: We thank reviewer 1 for the helpful comments. Our reply to the reviewer's comments is following after each reviewer's comment.

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 describ-

ing 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 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 anti-cyclonic 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.

Answer: The eddy-eddy interaction is now mentioned. We are convinced that the cyclonic feature C reaches to 600 m depth and not only 200 m. The southeastern boundary of the eddy C was placed at 16°55'S (eddy boundaries shown by the black v's; the boundary is not at the left end of figure 8) and from figure 8 it is obvious, that the bending of isolines is quite symmetric for the defined eddy boundaries and reaches down to at least 600 m depth. The method used is quite different to the one in Chaigneau et al 2011 and revised text makes now clear, that not one method is bad, but only 2 completely different approaches were used which lead to different results. The location of eddy formation is now mentioned for eddies A and C. Eddy B could be followed back in time for 5 months, but could not be followed completely back to the formation region near the South American continent. The satellite data changed considerably in eddy strength and size, probably caused by the interpolation schemes used to create the satellite images. Due to these variations and our focus on the real measurements we did not try to include an investigation on size variations

and trajectories. However, the behavior of eddy A with a westward movement of 1.5 months after formation and 1.5 months stationary location before the eddy left the area near the shelf-break is now described. We tried to collect the SST satellite data during the cruise. However, as we were always under cloud cover except very close to the shelf, no complete SST satellite figures exist for the time of the cruise. NCEP-wind data have a small resolution and strong smoothing, hence not much local details are resolved. Hence we try to focus more on the real measurements made during the cruise. For figure 3 the difference between the mean of the defined outside stations and the station in the center is plotted to show the amplitude of changes. For AHA and ASA the maximum of swirl velocity from the ADCP was defined on both sides of the eddy for each depth (similar to Chaigneau et al. 2011, and then temperature and salinity were interpolated on an equidistant grid between these boundaries from all existing profiles along the section and the gridded field was used for the AHA and ASA computation, now explained in the text.

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 NO_3 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- 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.

Answer: The statement about the 'significant contribution' by eddies to the productivity was modfied.

Discussion of Gruber et al. (2011): Our main point here is that the depth where high

nutrient concentrations are found in eddy B is significantly deeper compared to eddy A. This implies that nutrients such as NO3-, PO4- or silicate are indeed subducted as suggested by Gruber et al..

The NO3- concentrations in eddy B (open ocean) are indeed higher as observed in eddy A (coastal ocean). However, the history of the two eddies is different. That means that the higher NO3- observed in the aged eddy B most probably resulted from a different initial higher initial NO3- conc. when the eddy B was formed. We modified the text to clarify this point.

We marked all chlorophyll units with (cc) for company calibrated only and explain it in the text, hence the reader should be aware, that these numbers are not exact chlorophyll data.

We agree, it would have been very useful to have NH4+ been measured on M91. But, unfortunately, NH4+ was not measured during M90, thus, no NH4+ data are presented in the article.

We added a sentence (see introduction) citing Fernandez et al. 2009. The locations of the stations # 9, 12, and 14 of Fernandez et al. nicely match the sections of eddy A. Unfortunately, a few data points for NH4 and NO3 uptake/regeneration are presented only for stat. #9. Thus, we do not see the point to discuss Fernandez et al. in the context of our results.

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.

Answer: We tried to strengthen the abstract. Values were added for AHA and ASA and a factor is added for the reduction in chlorophyll. We modified the text of the abstract as suggested by the reviewer.

Introduction 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 4th 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 5th paragraph (connection between eddies and low oxygen conditions in the region) should follow (as modified 3rd 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 4th paragraph when they are strongly related. The most relevant characteristics of coastal upwelling off Peru (3rd paragraph) in terms of productivity and low oxygen-N cycling can be placed in connection with the modified 3rd paragraph suggested above.

Answer: The introduction was rearranged accordingly.

Observational data 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.

Answer: The focus of this manuscript is to present the measurements taken. The existence of the eddies was quite clear from the SSHA figures when we started the measurements during the cruise. For the boundaries we had to stay with the stations available, hence objective methods to identify the boundaries might define boundaries not available from the measurements. In addition the subsurface distribution of the parameters might be different to the satellite products produced with interpolation schemes from the available satellite observations might not fit the actual measured parameter distribution, and in case of mode water anticyclones the SSHA signal is weakened any-

way. Hence we prefer to define the boundaries from the available in-situ measurements related to the high resolution ADCP velocity data. This fact as well as the method to define the age of the eddies is described now in the text.

Page 5, Line 27: include the dates when the eddies were sampled

Answer: Unfortunately the page and line number on our print version of the ms differed from the reviewer's version but we hope we found all correct locations mentioned. The dates for the sections are included now in the data paragraph and in the figure legends of the section figures.

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

Answer: We marked all chlorophyll units with (cc) for company calibrated only and explain it in the text, hence the reader should be aware, that these numbers are not exact chlorophyll data.

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

Answer: NH4 was not measured during this cruise, hence we can't present NH4 results.

Line 31: "Aviso", use capital letters: AVISO

Answer: We checked the web-page and on the web-page Aviso is not written in capital letters. E.G. in The glossary: 'Aviso: Aviso distributes satellite altimetry data from. . .'. Hence, we keep the old writing but see no problem to change it to capital letters in case there is a definition that Aviso has to be written as AVISO.

Page 7, Line 1: correct "spacial" for "spatial";

Answer: Word is corrected.

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

Answer: In chapter 2 on the data the SSHA data used are specified.

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

Answer: Text modified accordingly.

Line 8: grid cell 1/24°x1/24°

Answer: Text modified accordingly.

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.

Answer: The computation of AHA and ASA and the comparison to Chaigneau et al. 2011 is moved to the discussion section as proposed by the reviewer.

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).

Answer: We rearranged section 3 and start now with on overview on all eddies. The subparagraphs were renamed. As we describe the formation region when possible and the transition within the paragraph, we could not shift the information on the age to paragraph 3.1. As described above we tried to collect the SST satellite data during the cruise. However, as we were always under cloud cover except very close to the shelf, no complete SST satellite figures exist for the time of the cruise. NCEP-wind data have a small resolution and strong smoothing, hence not much local details are resolved. However, a figure 1a for the mean distribution of satellite chlorophyll-a is added as proposed by the reviewer. The information on the separation from the coast at the beginning of paragraph 3.2 was removed. The formation of this eddy and the time when it stayed stationary is described later in paragraph 3.2.

Page 8, Line 1: "typical for a mode-water eddy" does not need a reference here since this is already in the Introduction section.

Answer: Reference to McGillicuddy Jr. et al. 2007 was removed.

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

Answer: The value for the chlorophyll maximum is included in the text now.

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!

Answer: The age was determined after the general description from the hydrographic measurements. As the description of young and old eddy is later in the text and the subsection titles were renamed the description of age should be well in time for the general understanding of the text.

Page 9, Line 1: "The two anticyclonic eddies subject to our study . . . " but this section is focused on one of them.

Answer: This sentence was reworded.

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.

Answer: The text was modified and coast and shelf is no longer used.

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).

Answer: We omitted the sentence “Nitrite concentrations levels . . .”.

Discussion 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.

Answer: A better description of the eddy formation and movement is now included. After formation the eddy moved westward for 1.5 months and it than stayed about stationary for about 1.5 months before it started to move continuously westward.

This indeed is an interesting and important point; however, in view of the fact that we do not know the initial biogeochemical settings of the eddies at the time when the eddies have been formed, we cannot discuss the effects of the stationarity on the N loss (at least any discussion would be highly speculative). The initial settings, in turn, strongly

depend on the strength of the upwelling which shows a temporal variability.

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?

Answer: We can’t determine the differences from our measurements but we added a reference (Claustre et al. Biogeosciences 2008) and discuss possible causes for the higher concentration.

Page 14, Lines 26-this paragraph: N* calculations and interpretations should e part of the Results Section

Answer: The paragraph on N* calculations was moved to the results section as proposed.

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

Answer: We modified the scales on some figures and a better resolution is applied for the coordinates of figure 1. In addition figure 1 is now showing also the mean chlorophyll distribution as requested by the reviewer (see above).

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

Answer: The color scale was modified (same color scale as figure 1a) and more tick marks added.

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.

Answer: We used the right hand panel to describe the near-shelf parameter distribution e.g. at the Peru Chile Undercurrent which could have influenced the water masses of the eddy when the eddy formed. The text was modified and we think it is clearer now, that the right hand panel is presented to describe the near-shelf parameter distribution. In a new figure 9 salinity profiles are shown to describe that the anticyclonic eddies A and B contain water from the Peru Chile Undercurren

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