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Interactive comment on "Linking diverse nutrient patterns to different water masses within anticyclonic eddies in the upwelling system off Peru" *by* Yonss Saranga José et al.

Anonymous Referee #3

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General comment : The paper focuses on the biogeochemical characteristics of mesoscale eddies in the Peru upwelling system. Due to the instabilities of the boundary currents, eddies from near the shelf break and slope trap water masses in their core and transport them offshore. Recent measurements have shown that contrasted nutrient conditions are encountered in the core of anticyclonic eddies. The goal of the paper is to investigate the nitrate and nitrite formation and evolution within two anticylonic eddies simulated by an eddy-resolving coupled dynbio model. The goal of the paper is sound and interesting as coherent eddies have an important role in the transport and mixing of properties in upwelling systems, in particular in the Peru region which hosts an intense OMZ favoring denitrification and anammox. The paper is rel-

atively well written, and the figures are of good quality (some can be improved). The simulation is carefully validated using observations, however the comparison of simulated and observed biogeochemical concentrations could be more precise, given the available observations. However, the paper has several flaws : 1) Only two eddies are studied in the model, while the model could be used to establish more robust statistics about the tmodelled eddies that are investigated. Do modelled eddies always behave as the ones that were chosen? The authors should conduct a more comprehensive eddy census with their model (what about cyclonic eddies?) 2) The two eddies that are studied are located in different regions of the domain. One is relatively close to the shelf while the other is far offshore. It was not clear whether the age of the eddies differ (I think they do), where and during which season they formed. Actually it seemed to me that the two eddies could be the same type of eddy but at different stages of its existence. 3) The discussion of the results is non-existent, and parts of the conclusion section do not reflect what has been studied in the paper. Given these remarks, I think that the paper requires a major revision before its publication.

Specific comments : P1, L17 : I do not understand the link between the processes enhancing vertical transport and basin scale effects. Please be more specific.

P1, L25 : Some references would be needed here for OMZ and denitrification/anammox

p2,L5 : Spell DNRA

p2,L27 : I do not understand the citation here.

P3,L5 : 'At the surface, the surface.. '. Rewrite.

P3,L6 : On the contrary, the EKE is reduced at the coast, which is not reproduced by the model. Why this reduction? The patterns are not really in agreement.

P3,L8 : Geostrophic currents are hard to see in this Figure as there are few isolines of sea level, so that it is impossible to see the intensified gradients associated with the currents. POC, PCC and SEC are not identifiable, and currents do not propagate (waves do). Maybe a plot with arrows would help.

Fig1 : The vertical and horizontal black lines in panels c) and e) need to be described in the legend.

P3,L18 : A lot of other things could also impact the poleward currents: impact of the smoothed bottom topography of the model, spatial resolution of the model, resolution and temporal variability of the open boundaries, climatological run vs observations over 2008-2012, underestimated wind stress curl... I do not think you can single out one effect from the bunch at this stage.

P3,L20 : presents

P3,L24 : why is the high O2 consistent with the observed dynamics ? This sentence is unclear.

P3,L27 : in spite of => except for the deeper nitrate

P3,L28 : The observed nitrite distribution is very different from the modelled one. Also the cross-shore gradients are very different, and very difficult to see in the data. Maybe you should try to change the color scale of the observations to show a qualitative agreeement between model and observations. From this figure it is clearly not the case.

P4,L3 : 'Consistent with the patterns presented by Stramma.. ': Please elaborate the comparison with Stramma's observations. Summerize what Stramma et al. found in these eddies.

P4,L6 : 'analysed their life history. An analysis of the eddy's evolution into the future.. ': This is repetitive.

P4,L9-10 : 'the first method,.. the second..': I thought that one method was used, with two steps. Rephrase.

P4,L15 : Why not display the SSH and/or Okubo Weiss parameter, instead of meridional velocity in color scale?

P4,L17 : It would be nice to use the model to verify if indeed denitrification is on-going.

P4,L18 : I do not understand what suggests exchanges at the edge of the eddy. There is a gradient of NO3, which is expected as NO3 reduces in the eddy. Please explain what you mean here.

P4,L21 : At the time of identification by the tracking algorithm

Figs 4 and 5 : isolines for some specific O2 values would be helpful in panels b)

P4,L27 : How low ? In comparison with O2 concentration in Asim (Fig 4b) ? Please be more specific. What intermediate depths ?

P4,L28 : It is not clear from the figure that the surrounding waters are particularly well oxygenated.

P4,L30 : Please add contours in Fig 5d and 4d and be more quantitative in the text and comparison with Stramma's observations.

P4,L30 : shows

P4,L1 : The asymetric flow associated with both eddies does not no strike me as something neither very clear nor very relevant for your study. You could skip that, also in the Asim description. P5,L7 : The denomination 'identification instant' sounds a bit awkward. It should more or less correspond to the date of the eddy formation, shouldn't it ?

P5,l8 : reduction by denitrification, and production by nitrification should be in text (it is in the legend)

Figure 6 : I think it would be nice to overlay a few contours of O2 to better see the edges of the eddy.

P5,L10 : How do the production and reduction rates compare with observations ? Same remark for nitrite rates.

P5,L19 :Figures 7 and 8 are a bit puzzling: - In Figure 7a the magnitude of the nitrate changes is low in comparison to the biogeochemical trend during the first phase (8 juin-15 juillet) thus likely due to physical processes, but comparable later on. It is somewhat also true for the slow nitrite evolution after August in Fig 7b.

- I do not see a clear link between the dark blue curve (=net production) which, when cumulated in time, should be be equal to the difference between the black and the cyan linear curves. I find it surprising that the trends (black, cyan curves) are so linear. Were they computed from daily model output ?

-The quality of Figure 7 needs to be improved. Labels are deformed, it is difficult to read the dates on the x axis. Also the cummulative nitrate and nitrite consumption appear as positive values, which is misleading. How was the eddy volume defined? Which criteria was used for the subsurface?

P5,L23 : I do not see anything at the edges of the eddies in Fig 8c. Are you referring to the tiny peaks near 200-250m depth ? Is that relevant ?

Fig9ab: what happened at the end of january ? What explains these nitrate/nitrite peak ?

P6,L2 : I am not sure I clearly understand what is meant here. Do you mean eddies capture surrounding waters in their core when they are formed, and then propagate with the trapped water mass ? When the eddy has been formed, the surrounding waters are entrained on the edges of the eddy, which creates horizontal stirring.

P5,L3 : could you explain the process here ?

P6,L10 : I did not understand where Asim was formed. Is it further north or south ? The eddy is still quite close to the coast. It would be really useful to add a figure which clearly shows the trajectories of the two eddies, since you study their temporal

evolution.

Figure 10c : the colors are a bit confusing, as the y axis of the TS diagram already indicates the temperature of the WM. The magenta points in Fig 10c suggest that the eddy was formed further north, away from the area as its original WM differ from the surrounding WM.

Fig 11 : would be easier to read if there was a zoom on the eddy. No need to show the whole domain. The eddy is far from the coast.

Fig11ef : I do not see saltier water entering the eddy, but rather fresher, slightly cooler and nutrient richer water entering the eddy in Fig11e-f-h.

P6,L31 : I do not see the exchange of waters with the environment in Fig 11k. Please explain.

P7,L1-2 : These lines are not convincing. You say that there is exchange, then that it is not strong. I do not see clearly where you are going with that.

Conclusions section :

P7,L10 : I think that it is not clear why the WM are different when the eddies are formed. Is it the location ? The season ? An interesting and possibly more convincing diagnostic would be to show the nitrate/nitrite concentration in the newly formed eddies in the same Figure. Also I don't understand how you can compare these 2 eddies, which obviously have different ages. Bsim is only 2 months old according to Figure 9, and is located very far from the coast. It seems to me that it should be older than Asim which is 3.5 months old (according to Fig 7) and closer to the coast. How could the two eddies be in such different places with such age difference ? This needs clarification.

P17,L12-13 : This may be true but it remains to be demonstrated based on dedicated diagnostics. I also do not think that you can base your conclusions on the examination of only two eddies. There must be plenty of eddies in your multi-annual simulation, from which you can compute some more robust statistics.

P17,L14 : Weakened flow relative to what ? You seem to imply the WM contained in the eddy depend on the formation mechanism or site, but this is not clear.

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