

Interactive comment on “Seasonal Variability of the Oxygen Minimum Zone off Peru in a high-resolution regional coupled model” by O. Vergara et al.

Anonymous Referee #2

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Biogeosciences review:

Summary: A high-resolution regional physical/biogeochemical model is used to examine the seasonal and interannual controls over the variability of the OMZ in the south eastern pacific. The competing influence of mesoscale activity in particular is explored. Specifically the role of the mesoscale activity on the OMZ in reducing biological production vs. providing increased ventilation – both would increase oxygen in the region. While the work is interesting and the question a good one – the work is muddled by lack of organization, clarity of approach, and grammar issues. The authors would greatly improve this manuscript by organizing it in a fashion something like – 1) problem description in the introduction – clearly stating issue of competing roles of

C1

mesoscale activity, 2) approach/methods used including specific details about analysis, 3) validation of mesoscale activity in the model (a section currently missing), 4) results beginning with the oxygen budget, bio vs physical, and moving into breaking it into subregions (this approach the authors took of subregional discussions was a good idea), 5) discussion of the possible mechanisms to explain these results and primary means of ventilation to the OMZ 6) conclusions. The discussion and results sections are muddled together currently. Without a clear distinction, the reader becomes lost. This work would benefit from some reorganization, clarifying elements in the figures, and a table with the budget analysis more clearly laid out. The biggest concern is the lack of validation of the model fields for the processes identified in this work as being important – the seasonal cycle, or the mesoscale activity, etc. Prior work is cited for validation, but it focused on an annual climatology.

Major comments: The additional section regarding the validation of the mesoscale activity within the model seems critical to identifying this mechanism. Specifically, the validation referenced in Gutknecht et al. 2013 seems to focus on validating the mean conditions, and Figure 6 of that work indicates some clear subsurface biases that need to be taken into account for these mechanisms. How do comparisons to the AVISO data set look? What about temperature? Can you rule out advection and confirm that the bias is in the biogeochemical model? The bias needs to be put in context – if the authors can describe the bias and then put their results in context that would provide much more information to the reader. The validation provided here is not only qualitative, but it also focuses on the average conditions. Some indication at to how well the model does at representing the variability and fluxes is necessary. Fluxes can include the SMS terms – like production. It is common now for Taylor or Target diagrams to be used to visualize the metrics of skill, this work would benefit from one that may also include some physical terms and goes beyond the climatology.

Also, the model climatology is an average over 10 years, while the data climatology is over 50 years. It seems a better apples to apples comparison could be made.

C2

The budget analysis is a powerful tool and has a lot of information within it. The authors appropriately use this tool to try and tease out the relative contribution of biological processes to the physical processes. It seems necessary to show the budget balances in the regions that this tool is used to discern the relative processes. A table of the budget terms would be one suggested way to achieve this goal. What does the climatological base budget look like? How big are the anomalies and residuals focused on in this work?

The methods section ends with a paragraph that seems more like discussion - describing vertical model decomposition. This doesn't seem to be very well woven into the rest of the paper and comes off as a bit of a distraction or outlier.

Given the paper's premise that the high resolution study provides further information than a coarse simulation would, some comparison seems warranted showcasing that result.

Some of the framing in the abstract about greenhouse gases is isolated there. While it is interesting, the authors never return to that idea in the discussion. Either return to it or remove those ideas.

Minor technical comments:

Lines 10-12 on Page 9 of the introduction points blame at the resolution of the CARS data set – the authors could add this resolution to some of their figures in order to better make this point, but without proper validation of the model, it is hard to blame the observations.

Line 4 on Page 3 – in introduction – understand should be understanding.

Lines 10-11 on Page 3 in intro – The sentence beginning with Furthermore refers to a process called “habitat compression”. The phrase may be more clear than the current explanation.

Line 12 page 16 – Relatively should be Relative.

C3

Figure comments: The main comment in all the figures from the model fields is about the white space – what does it indicate?

Figure 1 seems to indicate the model does not achieve the same onshore/offshore gradients in oxygen around 10 deg S. This seems critical to some of the points in the paper about transport mechanisms and goes unaddressed in the text.

Figures 1-3 would benefit from additional dots on the figures to identify where the samples the made up the climatology from the observations were made. The reader could discern from that addition the errors in interpolation to errors in the model.

Figure 4 would benefit from isobaths contours so the shelf region was highlighted.

Figure 5 seems to indicate the model is biased high – that the model underestimates the hypoxic volume. Is that because of advection, temperature, or the bio model? Or could it be the different time periods compared between the model and obs?

Figure 6 – this figure and discussion are out of context and need to be reorganized at a more appropriate time in the results. These numbers need to be put in context with the other important fluxes as you do in the budget. So this needs to follow the budget.

Figure 8 a and b panels do not appear to be on the same scale. Refer to Equation 2 from the text in the caption.

Figure 10 is confusing but important. Clarity to the reader would be achieved through a longer caption explaining the different colorbars in a-c and d-g, as well as the percentages – which don't add up. Why is the undercurrent identified? Is it referred to in the text? Are these all model results? What transect is this in the domain? Does the choice of the transect change the results? The text refers to this figure describes seasonality, but this figure just says climatology – is it a seasonal climatology? In that case, what time of year is shown?

Figure 11 requires more explanation as to how it was made. No explanation of residuals etc is provided in the text. How should the reader interpret the residual? Is it meant to

C4

just be physical?

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