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Interactive comment on "ENSO-driven fluctuations in oxygen supply and vertical extent of oxygen-poor waters in the oxygen minimum zone of the Eastern Tropical South Pacific" *by* Yonss Saranga José et al.

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Dear anonymous Reviewer III

We thank referee #3 for the constructive comments, which have helped to considerably improve the manuscript. Detailed responses to the referee 3's concerns are listed below. Respective changes are highlighted in bold in the accordingly-revised manuscript.

Specific comments: The discussion of the main results and the mechanisms behind them is incomplete and at times obscure, which makes one question the overall ro-

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bustness of the conclusions. In particular, I'm referring to figures 6, 7, 9, which are dense and hard to interpret. A problem is that the timeseries are dominated by a strong El Nino in 1997-1998, but the remaining events are much weaker, and the correlations discussed by the authors often very hard to see. Most of the signal discussed indeed is only noticeable if one focuses on that event specifically, but it is unclear from inspection of the figures how much the same dynamics occurs during other events. A composite approach (e.g. Yang et al., 2017, GBC) would better support the applicability of the findings to more events, and thus their generality; unfortunately the hindcast simulation only covers 20 years, so composites may not be very robust. Even focusing on the single 1997-1999 ENSO cycle is hard, because the figures are particularly dense with information and hardly legible. I suggest that, if a composite approach is not possible, the Authors consider a better way of presenting the result, e.g. showing and discussing the 1997-1999 cycle, and then discussing generalizability to other cycles. This would make the mechanistic interpretation and the story presented more clear.

We regret that the figures were hard to follow. We have improved the figures presentation by differencing the El Niño and La Niña events with different color patches. We have also reduced the amount of information presented in each figure.

Much of this "story" is indeed based on somewhat subjective analyses: the Authors discuss the timeseries and rely on the reader to extract the same messages for one struggled several times through sections 2.2.2, 3.1, 3.2 to arrive to the same conclusions as the Authors. I suggest adding a more quantitative assessment of the timeseries, for example based on correlations (R2) between the variables discussed. This could be done when discussing ENSO driving variability in the SWL, O2 content, fluxes etc. This way, every time a mechanism is proposed, and its signal discussed based on the timeseries, a quantitative and objective support for it is also provided. (Of course one need to distinguish correlation and causal mechanisms.) Without some form of quantitative analysis, the robustness of several of the results remain open.

It would be indeed good to show more quantitative analysis. However, the processes in-

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terplay. While in the coastal region the changes in the suboxic waters can be explained by the changes in lateral oxygen transport, the dynamics in the offshore regions are controlled by multiples processes (Figure 1). In this case, the correlation / scatter plots do not show a relationship between the variables (Figure 2).

Page 9, line 6-12 read: We show here that ENSO-driven changes in oxygen supply are geographically different. In the onshore region, variations in SWL respond primarily to changes in the supply of subtropical and tropical waters, with a correlation of 0.63 and 0.5 respectively (Figure 9). The supply of subtropical waters in the early stage of El Niño reduces remarkably the southward extension of SW, especially in the coastal region of northern Chile (Figure 10-a). The offshore region, variations in SWL result from an interplay between different supply pathways and processes. In this region, both physical and biogeochemical (Figure 8-b) processes can contribute to changes in the SWL. With no surprise, the relation is statistically not significant (Figure 9).

The discussion of the problem and of the results in the context of previous work is particularly poor, and does not make justice to the work of many authors who addressed O2 tropical variability before. In particular I suggest the Authors give careful consideration to several important papers that came out in recent years, including Yang et al., 2017, GBC; Deutsch et al., 2011, Science; Ito et al., 2013, GBC; Cabre et al., 2015, BGS, which discuss mechanisms and drivers of this variability. The most relevant reference is Yang et al., 2017, which tackles a very similar problem in a much more general way, and against which the results should be compared. What surprised me in the manuscript is the lack of discussion of changes of O2 along isopycnal surfaces, which provide a natural framework for oceanic variability, and their drivers due to ventilation (e.g. water mass age) and remineralization changes. The Authors here take a different approach, by looking at the transport in and out of a fixed volume in time, but this should be carefully evaluated in the context of mechanisms clearly identified by others before.

We disagree with the reviewer on the comment that we should compare our results to

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Yang et al 2017 study. The Yang et al 2017 does not discuss the mechanism responsible for the variability of the O2 variability, but the ENSO-driven variability in water column denitrification. The water column denitrification is not a direct measure of oxygen consumption (which are remineralisation of organic matter, zooplankton respiration and nitrification in our model formulation) and this analysis is not in the scope of our manuscript. Nonetheless, we now refer to the very concise analysis of Yang et al 2017 in the introduction section of the manuscript.

In addition, the magnitude of simulated respiration within the suboxic waters presented in this manuscript is comparable to the respiration rates presented by Ito et al., 2013 (see their Figure 9). Note that the respiration presented in their study only accounts for the remineralisation term and is calculated below the euphotic zone in a control volume which includes the northern and equatorial regions of the east Pacific OMZ.

Moreover, the analysis is performed in boxes in a cartesian coordinate system. An alternative approach would be use an isopycnal framework. Because of fluctuations in the depth and volume of density classes during ENSO events, this approach would be conceptually and computationally more demanding.

The analysis is based on a model, and like all models the one the Authors use suffers from biases. However, the importance of these biases for the results has not been adequately assessed in the manuscript. A rough validation is presented, but it appear very limited, and is not connected directly to the questions asked. The model seem to have a large suboxic volume, quite larger than observations. This is apparent from comparing Fig. 2a and 2b, but should be better quantified. For example the Authors could plot volumetric histograms of O$_2$ concentrations, which would give a clear sense of the O$_2$ distribution at low O$_2$ values, and has become a fairly standard diagnostic for OMZ studies. This is important, because as discussed in Deutsch et al., 2011, variability of a small volume of anoxic water (as observed) can be excited much more effectively by small O$_2$ changes driven by ENSO and the related density structure and circulation reorganization, as compared to a much larger anoxic volume.

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Indeed simulated oxygen concentrations are lower than the CARS observations. This difference can not however be only linked to model biases, as the simulated O2 concentrations as well as its magnitude is comparable to the in-situ observations presented by Thomsen et al. (2016) and Czeschel et al. (2015). The difference between the simulated and CARS O2 are more likely to be due to the bias in the Winkler titration method, which according to Thomsen et al. (2016) fails to detect lower oxygen concentrations. In addition an histogram showing a O2 values is now added to compare the simulated and observed oxygen concentrations.

Page 5, line 3-13 now read: Oxygen-poor waters are present from 50 m to 700 m depth (Figure 2-d,e) and reach values below 5 μmol/l shown in the vertical structure of observed concentration (Czeschel et al. 2011, 2015) This low oxygen waters are not present in the CARS data (Figure 2-b). The absence of these poor-oxygen waters in CARS data is likely to be due to the bias in the Winkler titration method used during the oxygen measurement, which fails to accurately detect lower oxygen concentrations such as those recent in the ETSP OMZ (Thomsen et al. 2016). Despite this overestimation of oxygen levels at a local scale, the simulated basin scale mean values are comparable to the observations. Significant differences are present at 90-100 μmol/l range, with the model values being about 10 μmol/l lower than the observed oxygen levels (Figure 2-g).

The model spinup is short, and is not discussed carefully. Most biogeochemical models, even regional ones, show important trends in bgc tracers due to model drift, for example in the subsurface and deep ocean (as is the case here for the lower SWL boundary). These trends can be can be corrected with long spinup time. If they are not evaluated and corrected, interpretation of trends in the model is not credible. Specifically for this paper, I question the interpretation of the trends in Fig. 4 as being physical, and in particular as being due to climate change (last paragraph of section 3.1). A specific link is proposed to observed deoxygenation trends for the same period, but a true quantitative comparison is not presented, so the trend attribution remains question-

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able. This attribution could be possible by comparing a model with climatological vs. interannual forcing, but probably would require longer integrations. However, since the main point of the paper is not the trends, I suggest just removing them from all time-series, and focusing on variability in detrended time series only. Following your above comment, we have ran the simulation with 20 years spin-up time and used the final 20 years of this longer run in the analysis in the revised paper. We have also removed the trend analysis from the paper.

Fig. 6-7 are based on O2 budgets within a fixed volume, which perhaps is not the best way to look at OMZ changes, but at least is easy to interpret. Looking at the figures, the lateral transports dominate, though there are massive compensations between positive and negative transport values. I was also surprised by how small the remineralization terms are, when other work shows an impact of respiration, especially in the upper ocean. In general one expects that over long-term averages, O2 transports exactly balance remineralization. Are the Authors able to close the O2 budget in the region, e.g. showing that dO2/dt-Transport-Respiration 0?

We would like to clarify here that the simulated oxygen consumption (which is the sum of remineralisation, nitrification and zooplankton respiration) shown in the manuscript represents the respiration within the suboxic water volume and not at the upper ocean.

The oxygen budget in the region is closed (Figure 3).

Page 4, lines 14-27. This whole analysis of model vs. observed ENSO events is hard to follow, probably because Fig. 3 is not showing the information very clearly. I suggest pairing it with an additional analysis, e.g. scatter-plots, or correlations (R2), to provide a more quantitative sense of the strengths of the model. We have refined the text with the aim to make it easier to follow. Also, a scatter-plot is now added in the new version of the manuscript.

Page 5, line 16-17 now read: This coherence can be clearly seen from the scatterplot, which show a positive relation between SST anomalies from our simulated and NOAA

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reanalysis product (Figure 4-b,d).

Page 5, lines 9-10 and related figures. I suggest picking a single O2 threshold, since the larger thresholds don't add any info, and only clutter the figure. The O2 seems anyway biased to have large volumes at low O2, so the threshold chosen may not make much of a difference. The main goal of this figure is to show that large differences occurs in the lower oxygen levels. All analysis was carried out for the 20 μmol/l range. The low oxygen analysis were removed from the new manuscript.

I am confused by Figs. 6-7, in particular by the O2 budget. First of all, they signals are hard to discern in the figures, as discussed in a comment above. Second, the quantities shown seem off by orders of magnitude. E.g. doing a conservative scaling calculation for the transport through the western boundary, one would expect a O2 transport at the SWL boundary on the order of: A 10 000 $\times$ 10$ˆ{31}$m $\times$100m $\times$ 0.01 m/s $\times$ 10 mmol/m3 = 10$ˆ5$ mol/s There is no way to reconcile this transport which which what is shown in Fig. 6a,7a, which is 3-4 orders of magnitude smaller. A similar rough calculation can be done for the vertical fluxes, Fig. 6c-7c with similar mismatches. The Authors should check carefully the order of magnitude of their transports.

We regret this confusion and lack of information in the figure caption. The quantities shown in the Figure 6 and 7 were the transport across the OMZ control box margins averaged over the box surfaces . The calculations addressed above is the integrated transport across the boundaries of the much smaller SWL. To avoid confusion, we have revised our calculations and we now display in the new Figure 7 the integrated transport across the SWL boundaries.

Page 5, lines 28-30. Unclear why a reduced transport from the oxygenated subtropical pathway would cause a thinning of the SWL layer, instead of making it thicker. The referee meant page 6, lines 28-30? If this is the case, we are saying that the poleward transport is reduced. This reduction of poleward transport is due to shift in direction of

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transport in the southern boundary from poleward to equatorward.

Page 6, lines 2-4. This is just an example of a speculative statement that could easily be checked in the model, since all transports are known. I suggest to support this type of statements with more direct and relevant analyses. The referee meant page 7? If so, we agree with the referee that this can be checked in the model. However, the investigation of the dynamics along the southern OMZ boundary would require the investigation of the dynamics of the subtropical gyre. This subject, which is vast and a stand-alone topic is beyond the scope of the present manuscript. We have, in the revised text, toned down our statement in response of the reviewer's valid point.

Page 5, line 20: should "eastward" be "westward"? Presumably, page 7? We thank the reviewer for pointing out this mistake, corrected accordingly.

Page 5, lines 21-23. This statement should be rephrased, as it is phrased here it doesn't make much sense. The referee meant page 7? If is the case, the sentence in now deleted.

Page 5, lines 29 onward: this, as many other sections, would benefit from some objective statistics to support the signals described. The referee meant page 7? If so, please refer to the second specific comments

Discussion/conclusion: a schematic of the processes discussed, and the main mechanisms at play, could help guiding the reader through the main results. A schematic of the processes discussed is now added in the manuscript, in the new Figure 10

Page 9, line 17: "shoaling", should it be "deepening" instead? We thank the reviewer for pointing out this mistake. Corrected accordingly

Fig. 1: why is topography missing in panel f? Because of difference in spatial resolution of CARS dataset and model results. The CARS data are now interpolated into the model grid and the topography is displayed.

Fig 2. The data O2 distribution in the map seems inconsistent with the section in d,

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which shows darker colors, i.e. lower O2. Very little of the domain is also shown in the sections; there is much more data that can be used for this validation over a broader region.

Indeed. As we explained in a comment above, the CARS data underestimates the oxygen content, especially at lower levels.

Fig. 3a: this could be split into two panel, one for each index comparison. The current version is crammed and hard to read. Changed accordingly

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Yang, S., Gruber, N., Long, M. C., and Vogt, M.: ENSO driven variability of denitrification and suboxia in the Eastern Tropical Pacific Ocean. Global Biogeochemical Cycles, 31, 1470-1487, https://doi.org/10.1002/2016GB005596, 2017

Figure caption:

Figure 1. Time series of oxygen budget components including (red) zonal advection, (blue) meridional advection, (black) vertical advection, (magenta) diffusion and (cyan) biogeochemistry source-sink. Red shading refers to El Nino conditions, green shading to La Nina conditions. The same time axis applies to both panels.

Figure 2. Relationship between oxygen transport and variation of the suboxic layer volume. Data correspond to onshore (left and middle left panels) and offshore region (right and middle right panels).

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Figure 3. Time series of oxygen budget components including (red) total transport+biogeochemistry source-sink and (black) the oxygen variation with time. Data correspond to onshore (upper panel) and offshore region (lower panel).

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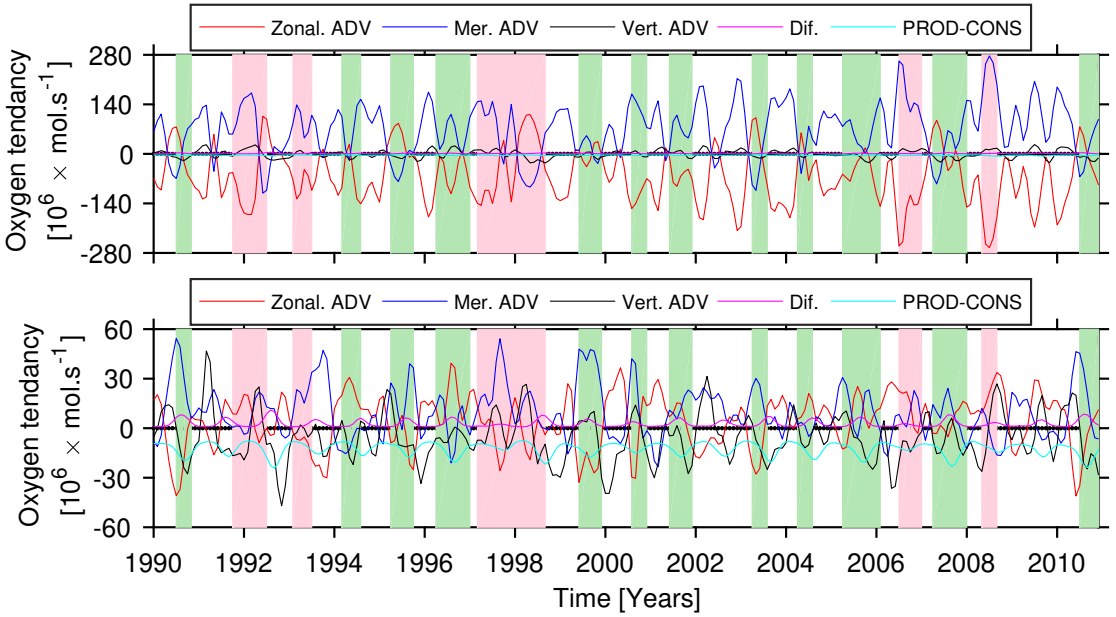

Fig. 1.

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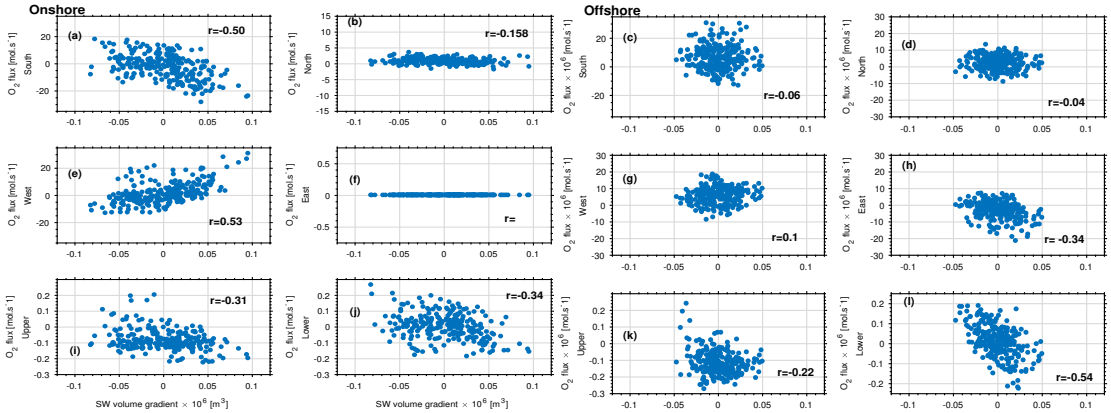

Fig. 2.

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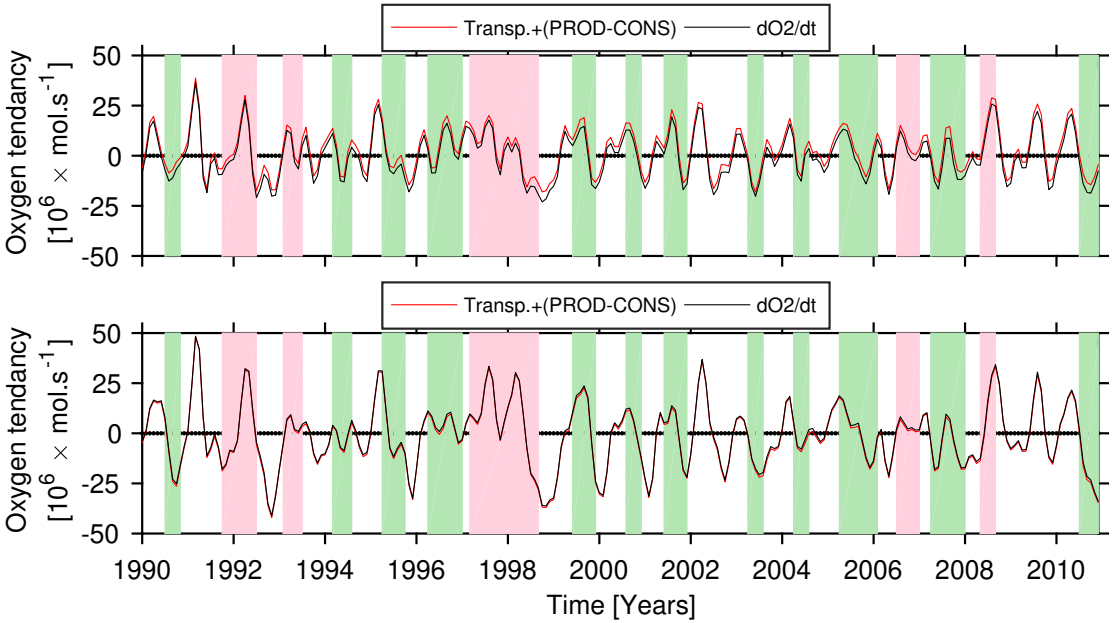

Fig. 3.

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