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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.

Anonymous Referee #3

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The manuscript by Saranga José and coauthors uses a physical-biogeochemical model of the Eastern Tropical South Pacific (ETSP) to investigate the effects of interannual variability (ENSO) on the dynamics of the oxygen minimum zone in the region. They find that ENSO variability has a significant impact on volume, strength and other properties of the OMZ, and that this control arises mostly from a combination of changes in water transports to the region, especially from the subtropical gyre through the southern boundary of the ETSP. In particular, the Authors highlight an increase of subtropical water transport during el Nino, which reduces the volume of suboxic

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waters and pushes them deeper, and an opposite change during la Nina, when the suboxic zone expands and shoals. Oxygen minimum zones are important for the marine ecosystem and for ocean biogeochemistry âĂŤ for example, they host large rates of water column denitrification and nitrous oxide production. Observational and modeling studies have shown that interannual to decadal climate variability strongly affects OMZs and their biogeochemical cycles. The manuscript by Saranga José adds to the literature, by using an eddy-permitting model that includes a fairly realistic representation of the biogeochemistry of OMZs, run for two decades with realistic forcing (i.e. in "hindcast" mode). This set up is fairly novel, as is the detailed focus on the ETSP. The result of a significant influence of water transport from the southern boundary is also fairly novel and adds a different perspective to the existing literature. This finding make the paper in principle suitable for publication in Biogeosciences. However, there are also major flaws in the presentation and discussion of the results that make the paper at times unclear or incomplete, and the bibliography is missing several fundamental references against which the results should be critically evaluated. Therefore, I recommend a substantial revision before the manuscript can be published.

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 robustness 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.

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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.

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 âĂŤ I 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.

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

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should be carefully evaluated in the context of mechanisms clearly identified by others before.

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 O2 concentrations, which would give a clear sense of the O2 distribution at low O2 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 O2 changes driven by ENSO and the related density structure and circulation reorganization, as compared to a much larger anoxic volume.

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 questionable. 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 timeseries, and focusing on variability in detrended time series only.

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 $\sim$ 0?

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.

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.

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:âĂÍ$\sim$10,000 * 10ˆ3m * 100m * 0.01 m/s * 10 mmol/m3 = 10ˆ5 mol/s âĂÍThere 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.

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.

Page 6, lines 2-4. This is just an example of a speculative statement that could easily

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be checked in the model, since all transports are known. I suggest to support this type of statements with more direct and relevant analyses.

Page 5, line 20: should “eastward” be “westward”?

Page 5, lines 21-23. This statement should be rephrased, as it is phrased here it doesn’t make much sense.

Page 5, lines 29 onward: this, as many other sections, would benefit from some objective statistics to support the signals described.

Discussion/conclusion: a schematic of the processes discussed, and the main mechanisms at play, could help guiding the reader through the main results.

Page 9, line 17: “shoaling”, should it be “deepening” instead?

Fig. 1: why is topography missing in panel f?

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

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

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