We thank both reviewers for their comments, which helped to improve the revised manuscript.

Reviewer(s)' Comments to Author:

Reviewer: 1

For final publication, the manuscript should be
accepted subject to **technical corrections**

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Excellent paper. There are still a few minor copy editing and English language issues, especially in the new Supplement text, which should be fixed before publication.

Referee Report:

The authors have addressed the concerns of the first review in a substantive way. I especially appreciate the new figure S1 and the addition of pertinent details on the methods. There are still a few minor copy editing and English language issues (especially in the new Supplement text).

Thank you for accepting our revision of the first version of the manuscript. Please note, as reviewer 3 mentioned that the title of the manuscript did not fit well the focus on the 6 tropical regions investigated in a 2008 paper, we changed the title to better fit to the text of the manuscript. We read the text carefully and hopefully improved the text and supplemental readability.

Reviewer(s)' Comments to Author:

Reviewer: 3

Review of the manuscript, entitled "Oxygen and nutrient trends in the Tropical Oceans" authored by Stramma and Schmidtko.

In this manuscript, the authors have intended to describe trends of oxygen and nutrients using long time series spanning from 1950 to 2018. This work is an extension of the authors' previous paper published in 2008 by including a more recent dataset and incorporating more variables into the analysis.
The manuscript presents an extensive set of dataset and therefore, contribute in enhancing our existing understanding of the said processes. However, as noted in the earlier comments by other reviewers, I have few major concerns and they are listed below:
1. The title of the manuscript says "trend of the Tropical Oceans". But actually, the analysis is limited to very few locations in the tropics. This is particularly true for the Indian Ocean. I

can understand that data limitation is the major problem to extend this study to a broader region, but then the title is grossly misleading.

We changed the title to "Tropical deoxygenation sites revisited to investigate oxygen and nutrient trends", which clearly relates the new manuscript to the areas investigated in 2008.

2. Another major problem with the analysis is that the calculated trends for oxygen and nutrients for most regions are either not statistically significant or the error associated with the trend is more than the trend itself. This indicates that goodness of fit for this trend calculations are very poor (can be seen easily from the plots as well), which means the spread of the data point is too large for a sensible linear fit. This casts a doubt on the reliability of these trends. However, there seems to be a definite decreasing (increasing) trend for the oxygen (nutrients) in the 300-700 layer, but the calculated trends may not be reliable.

To make the readers aware of the unreliable trends based on the low data base we mention this already in the abstract as well as in the text and in the supplemental material. Nevertheless as it is pointed out, the poor data coverage do not allow a statistical proof of these trends, but the data distributions points to a possible change that is worth discussing.

3. Authors have interpolated data into standard vertical depths and then averaged over 50-300 m and 300-700 m to calculate the trends for the respective variables.
(a) What are these standard depths?

Most data is observed close to standard depths with variations. The approach here is using a 5dbar interpolation as now added in more details to the methods.

(b) I believe, the depth ranges are chosen as an analogous to the ocean heat content approach. However, unlike temperature data, oxygen and nutrient data are much more sparse and therefore, may pose a problem while averaging. This is particularly problematic for the upper layer as some part of it is above the thermocline and some part within the thermocline and therefore, experience a strong gradient in oxygen concentration. Now, it may possible that in some years data above the thermocline is available and for other years within the thermocline. This will result in scatter concentration in time series similar to what we see here. Moreover, as the thermocline itself show a large shift due to ocean warming and various climate modes, the averaging across the depths (in case of data coverage is not uniform across the depth and year) will itself lead to a trend. How authors have addressed this issue is not very clear.

Thank you for pointing this out again, we now explicitly state in the text that we start below the first nutrient/oxycline at 50m. And also mention now the possible influence of the thermocline shift and a possible influence on the trend computations for the upper layer more explicitly.

(c) For vertical interpolation, a uniform Gaussian weighting method is applied with a uniform cut-off range in space and time across all the boxes and (top and intermediate) layers. Since the variability in the top layer is very different from the bottom layer, different weighting mechanisms may suit better for the different layers.

As we interpolate the data in both depth layers on 5 dbar steps and the depth layers connect at 300 m depth, we think it would be good to keep the same weighting mechanisms to increase comparability. For computing a nutrient or oxygen budget this would definitely be appropriate, but since the trend analysis uses the same distribution over time we are confident that we would not gain more reliable trends with varying mapping schemes.

(d) In order to better appreciate the data inventory and the trends, is it possible to show number of data points available for each of the standard depths for each box? Maybe a plot with y-axis as depths and x-axis as years with the number of data points as colour will serve the purpose.

The CTD data were reduced to 5 dbar steps and all data from one year within a 5 dbar interval were combined to one value in this 5 dbar interval. Due to different data sets from bottle or CTD data such a plot of the original data would be very variable for different years and could lead to confusion, but could not help to lead to better trend computations.

4. Tables showing the trends are just too many details and may not be very useful to most readers. Is it possible to convert them as barplot with error bar for each variable? This will make life easy for the readers. The detailed tables can be provided as supplementary information for interested readers.

We think in Table 1 and 2 the trend and 95% confidence interval should be presented to show which trends are not within the 95% confidence interval. The details on the number of profiles and the standard deviation were added to the table based on the request of one reviewer to add these values to the table.

Overall, the trend calculations are not quite robust and also doubtful. There are a large number of caveats present in the data analysis. Therefore, I believe the title and abstract are exaggerated in nature. Authors need to spell these caveats upfront in the abstract so that the readers are not misled while interpreting these results.

Yes, we write now in the abstract: "Due to the low amount of data available the results are often not in the 95% confidence interval, but nevertheless indicate existing trends" and we changed the title of the manuscript, that the reader much better know that the results focus on few tropical areas and that the results have to be regarded with care.