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

Reviewer(s)' Comments to Author:

Reviewer: 2

The manuscript describes changes in dissolved oxygen and nutrients which might have various and important impacts on the ocean ecosystem. As the authors note, the manuscript confirms conclusions done in the previous work by Stramma et al (2008), which describes trends in the same selected geographical areas.

1) My main concerns regarding the significance of the presented results relates to the poor observational basis. In the concluding remarks the authors themselves acknowledge this fact and point to the necessity for further verification in the future.

The visual inspection of the calculated trends shows high variability of the yearly parameter concentration values imposed on a much weaker climatological signal. Some of the trends are not significant at 95 percent level. The others, even formally significant, leave the impression that the removal of just few data points would lead to significantly different trend estimates. Here, increasing the number of investigated areas of similar size, or increasing the size of the investigated areas could help to confirm (or not to confirm!) the robustness of the presented results.

The main focus of this manuscript is to compare the extended time period with the Stramma et al. (2008) results, this paper is even today highly cited. Therefore, we used similar methods to make the results fully comparable (mentioned now in the additional paragraph in the data and Methods chapter).

The data base for the manuscript was similar low as for the investigation of Stramma et al. (2008). Hence there is in this manuscript and in the earlier paper a larger uncertainty possible. For the 2008 paper later measurements in literature confirmed the decreasing oxygen trends. One focus of this manuscript is to compare the trends for the longer time period with the shorter one in the 2008-paper. For additional areas we would need an additional area with measurements covering a large part of the 1950 to present period, we are not aware of, as in the tropics there are very limited areas with a larger data base. Due to changing oxygen on the geographical locations an extension of the areas would include more uncertainties. Hence, we stayed with the same areas as in Stramma et al. (2008), but we describe these limitations more specific in the Chapters Data and methods, Discussion and Summary and in a supplementary file.

2) Using optimally averaged yearly values which in turn are used to estimate trends is justified, as the averaging procedure acts as a smoother. However, I would appreciate the elaboration about the possible errors related to the mapping scheme, and, because of the data paucity, even these averaged data might be linked to relatively large errors.

The reviewer is correct, this procedure smoothes the dataset to a degree. We added more information to the supplemental methods discussing the possible errors. In general, this mapping scheme is quite conservative and is more likely to show no trend than a trend for two reasons. First with a non-smoothed data set an oversampled anomalous year has significant impact on the whole timeseries, second with lower overall smoothed data points the uncertainty of a trend analysis is larger. With those points in mind, one can assume that the results, if statistically significant are a robust find.

3) The authors do provide measurement precisions for the modern measurements and offset estimates for the older data. The reference is done to the paper by Tahua et al., 2010.
Even for temperature which is easier to measure compared to other parameters, systematic instrumental errors pose a big problem in estimating the ocean heat content changes. Possible instrumental biases in oxygen and nutrient data is even a bigger issue, which was treated, for instance, during the WOCE time. Here, references to Johnson et al., 2001, and to Gouretski and Jancke, 2001 could be added to the reference list. From the manuscript text it is not clear, whether the original data were corrected for systematic biases, or not. More discussion of possible bias impact is needed.

The offsets derived from an inter-cruise comparison described by Gouretski and Jancke (2001) is now included as well as the initial standard deviations of cross-over differences from Johnson et al. 2001. These papers are now used to describe a possible bias from the measurements. For heat content the systematic bias is one directional and addressed mainly expandable thermographs, such data is not used in this analysis. In the case of our data, we have evidence as referenced that the bias is more likely to be noise than systematic, thus not impacting any trend analysis.

4) Several areas show a certain shift in oxygen concentration for the data after 2000 (Fig.2a,b,d,f; Fig.3 a,e) Could these change in concentration be due to unexplored biases?

We think that these changes in oxygen concentration are related to the climate signals changing in all three oceans shortly before the year 2000. However, we included a discussion related to these changes in the supplementary text.

5) it is interesting to know, how sensitive the calculated trends are to the thickness of the layers (the fixed thickness of 250 and 400 meters are used for the presented results).

**As mentioned above, one focus of this manuscript is a comparison with the Stramma et al. (2008) paper, therefore the layer 300 to 700 m was selected again. Depending on the parameter gradients at the boundaries of the layer, the trends will be different. However, as the oxygen trends for the 50 to 300 m layer and the 300 to 700 m are all negative (except for the 50 to 300 m layer of area C due to a local effect) the result of oxygen decrease is not related to the depth layer chosen.
This is now described in the Discussion and Summary chapter.**

6) Can the yearly parameter values be seasonally biased considering the poorness of the data basis??

As the annual data were computed independent of the season due to the poorness of the data basis, a seasonal influence might be possible. However, as the seasonal cycle in the tropics is weaker than in most subtropical and subpolar regions (Louanchi and Najjar, 2000) we mention it near the end of the Introduction and now also in the supplementary text.

Minor comments:
1) I do not see much use in presenting mean parameter values for the investigated areas

without providing the number of available original profiles and standard deviations
(Table 3)

The mean parameter values for a depth layer and year are used to compute the mean parameter values for the time period covered and here the number of years and the mean standard deviation are presented. The standard deviation is often large, as it is related to the variability of the annual mean parameter value and the strength of the trend during the measurement period, which is now explained in the text.
As for each year a different amount of data is available, computing the standard deviation for each year would provide only information with regard to variability within a year, but not for the mean values presented in table 3. The fact that the mean parameter is derived from the annual values is now mentioned in Table 3.

2) Please, indicate in the figure captions, that crosses denote the annual
parameter values used to calculate trends

Thanks, it is now mentioned that the annual parameter values are used to calculate trends.

Line 13: change indicates to indicate

As proposed changed to 'indicate'

Lines 125-126: probably the word "interpolated" is missing after the word "profiles"
(Line 126)

Thank you, word "interpolated" added

Line 267: change nutrient to nutrients

As proposed changed to 'nutrients'