Reply to reviewer 3

(our response **in bold font**)

We thank the reviewer for the helpful comments.

What is new in this manuscript (ms)? It is difficult to determine how much is original. The authors have completed an excellent synthesis of many data sources over many decades. This manuscript covers changes in four ocean properties in seven ocean areas and compares these changes to five climate indices and the global ocean surface temperature, providing more than one hundred possible relationships to be presented and evaluated.

I ask the authors to examine years with sparse data to determine how representative and accurate they might be. For example, if oxygen and nutrients vary in the opposite direction, does this relationship hold in years of sparse data? If changes in nutrients are in the same direction, then does this relationship hold in years of sparse data?

We are not sure if we understand the request of the reviewer here. Intra annual or short term variations in oxygen and nutrients can have a very different origin than long term variability, and they are not necessarily connected. Such an analysis, while interesting, would be possible for areas with sufficient data like CalCOFI, though we think the focus of the present manuscript should be on longer term changes. As the reviewer said, with already many options we decided to exclude the STCs to focus more on larger-scale correlations.

Why not plot the standard error of the mean values of oxygen and nutrients for each year? These standard errors might provide visual insight into the impact of years of sparse data on the trends and correlations.

Despite a simple task at glance, the data processing and in particular gathering was programmed in a way to bin data into annul data point early in the progress during data aquisition. To accommodate this desirable extension, we would have to change the computation of all areas used. Due to the often low amount of available data we combined all available data from one year into one profile which leads to a different amount of data points at different depth layers at different years and the error bars would not be comparable. A better option would be to no bin the data and work with the raw data, applying statistics that can handle highly heterogeneous data distributions, though this would make the data handling significant more complicated to follow, with no gain for the outcome and results. For

more localized studies this would probably be justified, putting more emphasize on the study of regional variability.

Data deemed unacceptable by Schmidko et al. (2017) are included in this ms to avoid discarding already sparse data. The ms notes possible errors in density that might arise from including these data but does not give expected errors in oxygen or nutrients. Nor do they explain why errors in density are relevant to this ms.

We rewrote the text to describe in more detail why this was done and is justified from our point of view. The text now reads:

Quality control and handling is described in Schmidtko et al. (2017) for oxygen and used here similarly for nutrients. The only divergence to the described procedure was that bottle data with missing temperature and/or salinity were assigned the temporal and spatial interpolated temperature and salinity derived from MIMOC (Schmidtko et al., 2013). This was done to ensure all data were in μmol kg^{-1} and not requiring the discarding of already sparse data. In Schmidtko et al. (2017) this was not performed, since the error introduced near or in boundary currents and fronts can be significant. In contrast the areas here are chosen to represent homogeneous patches with significant amount of data in the open ocean, thus in the areas analyzed here, this may only lead to minor errors in density resulting in an error of less than 0.05%, therefore negligible small in μmol kg^{-1}, compared to the oxygen or nutrient data accuracy

In calculating correlations among time series, how are the number of degrees of freedom determined? Convince the readers that the number of degrees of freedom are determined appropriately.

As requested by reviewer 1 we modified the text explaining how the effective degrees of freedom were derived.

I prefer that the title state that the manuscript gives results for "... the depth range of 50 to 300 metres in selected areas of the North and equatorial Pacific". As for "influence of decadal oscillations on : : : trends", the trends over the full time series as shown in figures 3, 4, 5, and 7 do not take into account the impact of PDO and NPGO decadal signals on these trends from the 1950s to recent years. I believe the variability in the oxygen and nutrient time series related to PDO, NPGO and other climate decadal oscillations should be removed from the data before 50 to 70-year trend is determined. Such an adjustment would allow the manuscript to match its title.

Reviewer 2 proposed to go the opposite direction, remove first the long-term trend before computing the PDO and NPGO trends. The PDO and NPGO trends might contribute to the long-term trend. Hence removing the PDO and NPGO trends first might also remove the contribution to the long-term trend, therefore we did not remove the PDO and NPGO trends first and computed the different trends from the original data set. We changed the title to now state the depth interval from 50-300 metres.

Why are graphs for Area P, Peru and Aloha in the Supplement, whereas graphs for

other areas are in the ms? Please put them all together in the ms.

The figures all look similar, hence showing all figures, including now an additional area proposed by reviewer 2, in the main text would be redundant as the main information is in Table 1. For those interested in details of all areas can reference the supplement.

The years of two maxima and two minima seem to be close for the 18-year oscillation and the NPI. How correlated are these two series? Can their impacts be separated?

Different to the earlier version, we now use the same time period of the oxygen data sets which led to different results. The maxima and minima are close and cannot always be separated depending on data coverage. However, the correlation of the 18.6 year oscillation is much weaker than the NPI and we think it is an interesting result that these two oscillations lead to quite different correlations, reasons for this can bethe shorter-time fluctuations in the NPI or the sensitivity to phase shifts in the oscillation. This is now stated in the text.

The names of all agencies that provide data and time series of indices need to be given, rather than only their Internet sites.

'The names of the agencies/universities are included in the paragraph data availability with the web-pages'

The writing in many places is sloppy and sometimes wrong. Too much information is included that clutters an already complex ms. I have given a few examples of these features below, but all authors need to read all the ms carefully to deal with this issue. Here are examples of sloppy and sometimes incorrect writing. The manuscript states on page 2, last paragraph, that increases in ocean surface temperature influence oxygen concentration through changes in solubility of oxygen and changes in convection of oxygen to subsurface layers. This sounds reasonable to me. However, the sentence beginning on page 3, line 16, attributes oxygen changes to solubility changes only. This attribution is then contradicted in the following sentences.

As we mentioned convection on page 3, we thought we do not need to list all components again on page 3. Now we included on page 3: '…changes in convection and thermocline depth'. We also removed STC analysis from the manuscript to focus more on the indices with larger impacts.

On page 3 line 20, the ms notes that shoaling thermoclines during La Niña or cool (negative) PDO in the eastern Pacific enhance nutrient supply. Should this region be stated more accurately as eastern tropical Pacific? I expect there are regions of the eastern Pacific outside of the eastern tropical Pacific that behave otherwise during La

Niña and negative PDO.

The eastern Pacific was mentioned at the beginning of the sentence, however to make it more clear that the related changes appear in the eastern Pacific we added ' in the eastern Pacific' in the sentence mentioned as well as in the next sentence.

I was surprised by the definition of PDO as given on page 4 lines 21 to 23. It has been taken incorrectly from Dressler et al. (2010).

We assume Dressler et al. (2010) should be Deser et al. (2010). According to the definition of Deser et al. (2010) one word was missing, which is included in the revised version.

I believe the correct definition of El Nino and La Nina is "five consecutive 3-month periods .." (page 6, line 3).

Various definitions of El Nino and La Nina do exist, the three months running mean was mentioned at the beginning of the sentence, however to make the definition more clear we modified the text as requested.

On page 11, line 14, the sentence reads ": : :the linear trend of the oxygen content of the layer 50 to 300 m decreases for the entire time period : : :" Actually, the linear trend is constant and negative. The trend would not be linear if it decreased.

Thanks for this information, 'decreases' is now replaced by 'is negative'

I doubt that Station P was occupied continuously from 1943 and it was likely established as a weather observation site rather than an ocean measurement site. (page 7, line 13)

The description of Station P was taken from Wikipedia and is an example that in short summaries in Wikipedia wrong information might be included. Thanks for letting us know. Now the text reads:

Station P, located at 50°N, 145°W in the North Pacific, was established as a weather observation site with a weather ship in 1949 which was manned continuously until 1981, and routinely hydrographic measurements were started in the 1950's. After the termination of the weather ship program shipboard measurements were made on average 3 times a year since.

Solid lines in Figures 3, 5, etc, are described in the captions as representing positive PDO phase after 1977, despite the obvious negative phase from 1998 to 2012.

As the data base for nutrient measurements is small compared to oxygen and temperature measurements especially in regions with no continuous measurements, we think that another subdivision would stress the data set too much. As written in the text in the areas E and D the

nutrient data base is so low, that we did not show the nutrient figures. For the area 2-5°S 84-87°W (Fig. 4) only two measurements are available for the period 1998 to 2013 and in the Peru region (now Suppl. Fig. S6) there is a data gap between 1985 and 2008. Hence we decided to define the entire period after 1977 as positive PDO in the text and the figure legends refer to the definition used for this manuscript.

Insert names of areas into the graphs of Figures 3 and 4. Give the units of ocean
properties in Figures 4, 5, and 7, as well as the units of trends. Lines in gray on figures
will be more visible if black is used.

The names Area E,D and G are now included in figures 3 and 4. Units for the parameters are given now in the figure legends of figures 4, 5, 7 as well as S3 to S6 as: (in µmol kg^{-1}, red crosses) while the units for the trends were already mentioned in the figure legends. Yes black lines for the PDO are more visible, but as the PDO curve is just background information, we think that the PDO as black line dominates the figure too much (see figure below).
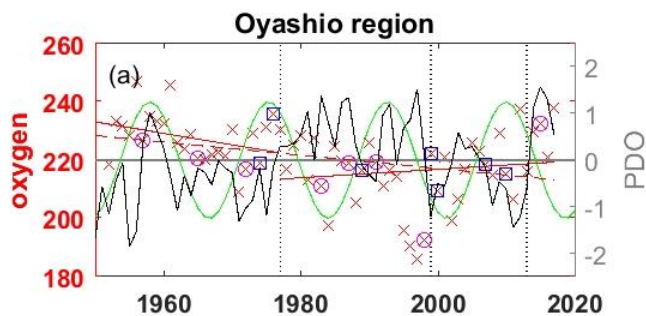
Examples of too much information:
I prefer that the Abstract begin with "Oxygen and nutrient time series since the 1950s
were investigated at 50 to 300 metres depth in seven areas of the North and equatorial
Pacific .." The sentences preceding this one in the present Abstract are not necessary
and divert the reader from the essential content of this manuscript.

According to the Geosciences guidelines for authors for the abstract: 'After a brief introduction to the topic, the summary recapitulates the key points of the article and mention possible directions for prospective research.' We prefer to start with an introduction instead of starting right away with the results.

The paragraph on page 11 from lines 11 to 18 notes the many areas in which the linear
trend decreases for the entire time period. (I assume the trend is negative rather than
decreasing). However, the final sentence notes that oxygen trends are not significant
for the entire time period except in two areas. Why describe insignificant trends at all?
There are sufficient significant trends to provide enough information to overwhelm most

readers.

As requested by reviewer 1 significance should be avoided and the decision of good or bad should not be related to one fixed value. Hence we mention now only that areas are within or out of the 95% confidence interval.

The first 9 lines of page 12 describe numerical differences between this ms and previous studies. However, the depth ranges are different, and the years are different in the two studies. The information is not useful unless the differences are attributed to the depth range or years. This paragraph could be eliminated.

As earlier investigations in this region exist it seems reasonable to mention these results. Despite the different depth layers the comparison with earlier investigations indicates that changes are related to time periods analysed and not just a linear trend. This in now explained in more detail in the text. The bi-decadal trend is no longer mentioned in the respective paragraph as this is the subject of the later paragraph.

Regarding the subtropical convergence cell (STC), on page 16, lines 10-13, the authors note that, "Due to the long duration of the STC phases and the sparse data set, it is not possible to perform a meaningful correlation analysis to investigate STC influence on the oxygen and nutrient variations." In addition, the authors note on page 20, line 20, that the STC showed no clear signal in the equatorial Pacific. Given this lack of impact, why devote any text to the STC at all, except to say it does not have significant correlation with oxygen and nutrient time series, despite an expectation that it might?

The reviewer is correct; we now exclude the analysis of STCs in the manuscript to focus more on the indices with large-scale impact. This made the manuscript more focused and the reader is hopefully less distracted by indices that we do not find to correlate significantly.