

Interactive comment on “The influence of decadal oscillations on the oxygen and nutrient trends in the Pacific Ocean” by Lothar Stramma et al.

Anonymous Referee #3

Received and published: 27 August 2019

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?

C1

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.

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.

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.

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.

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 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?

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

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

C2

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.

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.

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).

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

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.

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)

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.

Insert names of areas into the graphs of Figures 3 and 4. Give the units of ocean

C3

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.

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.

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.

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.

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?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-91>, 2019.

C4