Reply to reviewer 2

(our response **in bold font**)

We thank the reviewer for the helpful comments.

First, I am a physical oceanographer with insufficient knowledge on biogeochemistry, so I am not confident if I can properly judge this manuscript.
Biogeochemical measurements are definitely more difficult than physical ones, and the time series in each region shown in the manuscript must be valuable themselves. Nevertheless, Secs. 3.2 through 3.4, which should be the main result of this manuscript (inferred from its title), shows rough analyses with little plausible physical mechanisms.

Right, biogeochemical measurements are more difficult than physical ones, and also less biogeochemical measurements are available. Therefore, very few observations of long-term nutrient changes exist, which was the motivation for this manuscript. Changes are observed to be related to long-term trends and in addition to different climate signals related physical mechanisms but also to local biological conditions. The inclusion of the proposed 137°E section helped to put the observations better into the context of the PDO.

My biggest question is, in Sec. 3.2, why the authors show a linear trend for the whole period after 1976 (Fig. 3 and other figures) although they state that “the period 1998 to 2013 is dominated by negative seasonal mean PDO indices and is typically considered as a cool (negative) PDO phase” (Page 5, Line 3-5). If they are to see the relation between the biogeochemical variability and PDO, don’t they need to calculate the trend for each of three periods (-1976, 1977-1998, and 1998-2013)?

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 even did not show the nutrient trend 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. We added in the concluding results: “…the results might have larger uncertainties for the areas with low data coverage and the combination of the warm and cold PDO periods after 1976”. While the reviewer is correct that the overall trend here is not that meaningful with the underlying strong variability, never-the-less it is presented for constancy with the other areas.

Furthermore, although “it is expected that during cold PDO phases the oxygen will decrease and the nutrients increase in the eastern equatorial and tropical Pacific, while during warm PDO periods the oxygen should increase and the nutrients decrease” (Page 13, Line 11-13), the observed trends in areas E, D, G were opposite. So, what is the mechanism? As a non-expert in this field, I feel a bit hard to find what the new findings of this manuscript are.

The expectation mentioned on page 13 lines 11-13 is based on a possible PDO influence on the thermocline depth in a model by Deutsch et al., 2011 and a general Pacific Ocean description by Chavez et al., 2003. The new finding is that in real measurements these changes can’t be always seen, which means that other mechanisms are influencing the oxygen and nutrient distribution and local changes have to be validated by measurements.

Other comments:
Sec. 2.1: Subtropical cell (STC) is an ocean circulation component and is not temporospatial variability. Therefore, I feel odd to see that STC is aligned with climate variability such as PDO, NPGO, and ENSO as a controlling factor.

As mentioned in the text, according to Hong et al., 2014 the STC is strongly associated with the PDO. However, as model simulations by Duteil et al. 2014 described changes in oxygen and phosphate transport, we wanted to check this with measurements.
Still, the reviewer is correct, STCs can be modified and rely on the PDO, we modified the manuscript and now excluded STCs from our analysis, to focus more on the trends with significant impact.

Sec. 2.2: The authors’ data do not cover the western part of the North Pacific Ocean (Fig. 2). Why not the authors use the 137E repeat hydrographic section maintained by the Japan Meteorological Agency since 1967 although one of them belongs to the agency? With high temporal resolution and large spatial (meridional) extent, the section is expected greatly to fill the data gaps.

Now an area of the 137°E section is included to better cover also the Northwestern Pacific. The added area helped a lot to describe the results of the different areas in this manuscript in relation to the PDO.

Page 14, Line 19-20, “probably caused by water masses propagating by 5 to 15 years from Oyashio region into this part of the North Pacific”: why do the authors consider horizontal advection for the area P only?

This water would propagate further southeastward with the subtropical gyre towards the CalCOFIc region. The other regions in the North Pacific show a larger correlation with the PDO and this is now mentioned in the text. Of course water mass propagation might influence all areas, and this is mentioned now in the concluding remarks.

Secs. 3.2-3.4: If the authors are to extract decadal variability superimposed on the long-term trend (Sec. 3.1), it is better to examine the time series after subtracting the long-term trend.

Reviewer 3 proposed to go the opposite direction, remove first the PDO, NPGO and other climate trends before computing the long-term trend. The long-term trends might not be only related to ocean warming but also the PDO and other climate signals. Hence removing the long-term trend first might remove also the contribution by PDO and other signals, therefore we did not remove the long-term trend first and computed the PDO signal related to the observed oxygen and nutrient changes. For a time series of significant lengths, with several oscillations of the overlying signal this certainly would be the best approach, but since the data time series is short, any long term trend certainly is influenced by the phase of the oscillation at the beginning and end of time series, thus making this approach less ideal.