This paper relies on the spatio-temporal variability of the HNLC regions identified by SOM over the three major ocean areas using satellite-derived chlorophyll-a and modeling outputs of nutrients. The authors have performed the NO3:Chl as an indicator of the distribution limit of HNLC. They have demonstrated the linkages between HNLC extent and some climate-driven factors and teleconnections.

As a first very general comment, I would say that this is a valuable case study that can be published with some major corrections. The authors presented a lot of data and analysis procedures that need accurate processing schemas and precise interpretation, which they handled well.

The introduction section is well written and presents an adequate understanding of the presented work. The methodologies are adequate, but need some improvements. Some supplementary materials may be inserted into the main text because of their essential investigations and frequent references to them (e.g., Fig. S2).

The spatio-temporal variability of HNCL/NO3:Chl should be presented more precisely and quantitatively. For example, maps of monthly climatology, charts and maps of inter-annual cycles, and spatial-temporal cross section such as Hovemoler chart may present valuable results.

Some of the findings of the results section have not been well demonstrated in this study (e.g., section 3.4.1 Influence of SST variations – the global power spectra of CWT are needed to explain the intra-annual and inter-annual cycles). Some contents of Figures need to be better framed/explained. Some words would be expressed in accurate form. e.g., it is not clear in many places in the text that the “wind” word is mean wind speed, or wind vectors, or wind stress, or wind components (zonal and meridional).

I think a discussion section is required to explain the performance and limitations of the presented methodologies and results, which are not seen in this manuscript.

Bioregionalization analysis was used in previous studies to classify the global oceans (e.g., Longhurst, A. (2007), Ecological Geography of the Sea, Academic Press, London). Could the author highlight the need to new ocean’s regionalization which are not accessible from available global regionalization? And why SOM and not the other classification methods such as k-means?

I think the findings of nutrient model are not presented well. Some additional information is required.

The Mixed Layer Depth (MLD) is one of the main oceanographic indicators that can be used for interpretation of nutrients and phytoplankton variabilities. Does the nutrient model consider the MLD? If yes, please indicate in the text. And if not, I think it is required to consider the global ocean MLD in your work.

Looking to be constructive, in addition to the overall comments above, which should be taken into account in a possible review, I would like to point out the following Remarks.

1- In the 2.1 Ocean color data:

The GlobColour data are presented in 25 km spatial resolution globally. The authors have mentioned that the composites have a 0.25º spatial resolution. We know that the spatial resolution of 25 km and 0.25° are not the same specially at higher latitudes. If the 0.25° is true, please explain the spatial interpolation methods. If not, please correct.

2- In the section: 2.2 Nitrate data

The authors have made some essential assumptions that need to be approved precisely. May be explain more in the Results.

3- Please provide more information about the setup of the SOM algorithm (which are available not in the text nor supplementary material), in particular about the initial configuration: linear or random initialization, sheet or toroidal network, etc. Which neighbor function (Gaussian, Ep, et) is used to update the neighbors of the excited neuron (BMU) after each iteration during the training process.? Did the authors check the sensitivity of the SOM pattern to linear and random initialization?

4- The output of the SOM is also not well defined beyond being a map or topology; for example, how are the errors computed? The number of neurons is chosen not only depending on the topological errors (or topographic errors), but also on quantization errors. How different are the patterns when the number of neurons is, for example, 9?

5- Figure 6 and 7. The arrows are too small.

6- I recommend to show the time-series anomaly of HNLC and teleconnections at the top of the Fig. 6 and indicate the significant inter-annual cycles.

7- It is hard for readers to infer the shift after 2010 (Fig. 5). I think more visualization/explanations of data are required.

8- There are some abruptions in the significant annual cycles (1.5 year, from year 2006 to 2010) in the Fig. 7 which need to be explained. I suggest perform the global spectral cycles graphs.

9- The authors have considered SST and the teleconnections as a factor controlling HNLC variability. Are there any environmental factors such as precipitation and wind stress that may affect HNLC variability? Please discuss.