Dear Editor,

Please find attached a revised version of our manuscript egusphere-2022-827 entitled "Global variability of high nutrient low chlorophyll regions using neural networks and wavelet coherence analysis " which we have modified according to the suggestions and criticisms of the Referee. We also provided a detailed letter that addresses point-by-point all reviewers' comments.

We acknowledge the Reviewers' constructive comments which helped us to improve significantly the discussion of the results regarding the impact of the large-scale forcings on the variability of the NO3:Chl ratios. We also thank them for the overall positive appreciation of our work. We sincerely hope that, with the careful revision we have made, our paper will be found suitable for publication in Ocean Science.

Detailed response letter:

This paper has improved markedly in the revision. I congratulate the authors on having made a serious effort to address the previous critiques. I have a few further (mostly minor) comments below. (Note that the page numbering scheme is odd: it is not sequential but it doesn't reset to 1 on each page either; possibly it is sequential but the first digit got cut off for #'s >100. In any case, when I refer to X/Y I mean page # / line # and there should be no ambiguity.)

Major points:

(1) I think these authors have done a very good job of separating the Discussion from the Results. There are one or two passages of what I would call Discussion in the Results (e.g., 10/00-02, 11/45-48), and some parts of the Discussion are vague or too speculative. But generally, they did an excellent job.

We appreciate the recognition from the reviewer. We have modified/or eliminated these paragraphs to be more assertive and robust in our arguments. For example, the sentence in 10/00-02 was unnecessary and it has been deleted.

(2) There are a few places where "facts not in evidence" are referenced, or prematurely introduced before the data are shown. For example, the step change after 2010 shown in Figure 6: I think it would be better to just say "see next section" when this topic is introduced on 11/33, rather than referring the reader to Figure 3, because this step function is difficult to discern in Figure 3. This topic is discussed again on 11/54-55, and another ambiguous data reference is introduced, "i.e., P3 substitutes P1". This may be true but it's hard to tell from the plot: it is obvious that P1 disappears but less obvious that P3 becomes dominant in the months when P1 previously had been. It might be better to just say "P1 is no longer observed".

We recognize that some aspects regarding the step change in 2010 are scattered throughout the results. They recurrently show up in the different analyses carried out. We have modified P 11/33 and P11/54-55 as suggested.

(3) Parts of the Discussion are excessively speculative, or appear to contradict the previous text, and I have doubts about whether some of the literature cited is interpreted correctly. I commend the authors for their thoroughness; they cite quite a few references I had not heard of. But some that I am familiar with, including classics like Gargett 1991, are not necessarily interpreted correctly. The large-scale net Ekman transport is upward, albeit sluggish. I'm not saying localized upwelling associated with either atmospheric or oceanic

cyclones is unimportant, but that's not what Gargett (1991) is about. In the case of Cullen (1991) (14/47) I also have difficulty connecting the assertion made to the contents of the cited reference. The assertion that " that generate enough Ekman pumping to maintain high nutrient concentrations in the near-surface waters (Gargett, 1991; Harrison et al., 2004)" is questionable and likely to be misinterpreted. The Alaska Gyre IS a cyclone (whereas the NPSG is an anticyclone).

Gargett 1991 is cited because, while certainly, it is not focused on cyclones, it refers to the necessity of strong wind forcing required to debilitate the strong density stratification hampering vertical nutrient delivery in the North Pacific. We acknowledge that his analysis extends beyond the effects of wind-forcing and it could be misinterpreted, therefore, we have removed the sentence. Besides this paragraph, we have removed other sentences which could be considered too speculative.

Boyce et al 2010 drew some rather vigorous criticism (www.nature.com/articles/nature09953). Boyce et al (2014, 10.1016/j.pocean.2014.01.004) address some of these criticisms and could be cited here. They claim that the basic conclusion that a long term secular (downward) trend is detectable remains sound, but this conclusion remains controversial and I think that the authors of the current contribution should treat it skeptically.

We agree with the reviewers' view on Boyce 2010. We have substituted this reference in the introduction by the 2014 publication, which is more robust, and rephrased the sentence on chlorophyll trends to avoid being conclusive.

The results of Polovina et al (2008) represent too short a time series to be inferred to represent a long-term secular trend or anthropogenic warming signal.

We agree it is a short record (9 years), but they are published results that, we believe, should be cited in the introduction as an example of previous studies. Anyhow, we have toned down the relevance of this paper.

The entire paragraph on 14/53-60 contains a number of questionable assertions. The source of dissolved Fe in the northwestern Pacific is primarily from shelf and slope sediments, not river discharge, and the primary source of vertical mixing is from tidal currents (cf. Nishioka).

We do not think that the paragraph is questionable, yet we recognize that it provides a rather simplistic explanation. We have expanded our argument based on the review by Nishioka et al (2021) who in detail discuss the sources of iron and nutrient supply in the subarctic Pacific. We agree that most references indicate that shelf sediments are a main source; however, some authors (ie. Nagao et al 2007), cite both atmospheric and river discharges along the western coast may be important. Indeed, atmospheric dust has been considered to be the most important source of Fe in the North Pacific affecting biological production (Uematsu et al. 1983, Duce and Tindale 1991 and Mahowald et al. 2005, Boyd et al. 1998) though, more recently, Boyd et al. (2010), questioned dust-mediated phytoplankton blooms.
Nishioka (2021) proposes a coherent explanation to conciliate the putative sources for the biological response in the SNP that incorporates knowledge of both the atmospheric and oceanic Fe supplies. Blooms in open waters would be controlled by the sedimentary Fe supplied from the continental margin circulated laterally through the intermediate layer and upwelled to the surface by several mixing processes (winter mixing and eddy diffusive mixing), including interactions of tidal currents with the rough topography, whereas sporadic and

patchy surface phytoplankton production observed in the absence of vertical mixing in spring and summer is attributed to the atmospheric input of Fe dust (Nishioka et al., 2021).

The section on 16/97-03 is also confusing. I agree that the extent of the EEP HNLC is primarily a function of ocean upwelling rather than aeolian dust deposition. But what is stated here ("we observe a reduction of the extent of the HNLC region in the EEP during an enhanced wind intensity period") is actually the opposite of what is expected, and what is shown in Figure 6. In the EEP the period of strongest upwelling is in the boreal summer (e.g., Philander and Chao, 1991, JPO 21: 1399), which is associated with an expansion of the HNLC in Figure 6. The same should be true on interannual time scales: a period of increased trade wind intensity should be associated with an expanded HNLC, not a reduction (see also 17/44-45).

We agree that that sentence is confusing and contradictory with the pattern observed in seasonal variations (Fig 6b). This has been clarified in the revised version of the ms. Nevertheless, the extent of the HNLC regions is not necessarily solely dictated by the strength of the upwelling, but it is also defined by the interaction with adjacent circulation patterns (i.e. subtropical gyres in this case). This has been observed in coastal systems where upwelling intensification is correlated with water temperature but not with surface extent. In the case of HNLC regions, upwelling strength is likely to control excess nitrate concentrations but the extent obeys to more complex equilibria.

The reference to a "meridional propagation of the MOC effect" on 17/33 also seems backward to me: if the fluctuations in HNLC area arise from fluctuations in the strength of the MOC, should they not appear first in the SO and last in SNP? And then on 17/42 we have "weaker MOC is related to increases in the extent of the EEP and contraction of the SNP and the SO HNLC regions". This appears to contradict what was said above about stronger MOC implying stronger SO upwelling (17/29). Maybe that's just me making a simplistic assumption that stronger upwelling leads to an expanded HNLC. But maybe this is not the case in the SO, and the data do seem to show that the SO HNLC contracts after 2010. Possibly these things are related: the reason there is a lag between the EEP and the SO is that the changes in the SO HNLC extent are only affected by the declining phase of MOC fluctuations, not the strengthening phase. Why this would be I do not know, but it may have something to do with nutrient stoichiometries in the upwelled waters (e.g., 10.1038/nature02127; 10.1038/nature04883).

There are several aspects regarding this question. First, MOC influences upwelling rates and coupling appears to be more significant when it debilitates after 2010, as shown in Fig. 7f,g,h. Indeed, NO3 is reduced but CHL is enhanced, particularly in the declining phase of MOC. A plausible explanation for increased CHL values is that the plankton community can cope with Fe recycling rates under not strong upwelling conditions (i.e. weaker MOC), as reported by Rafter et al. 2017, and thus a better agreement. This occurs at decadal scales or, as a general pattern in the analyzed series.

Secondly, it should be also considered that, as we mentioned above, HNLC extension is not necessarily related to upwelling intensity. However, in the case of the shift in 2010, our analysis reveals a major change in the NO3:CHl patterns. Pattern P1 disappears in favor of P2 and P3 which show both less extension and reduced NO3:CHL ratios, especially in the SO (see Fig. 4 and 5a).

A more complex question is that regarding the phase relationship inferred from the wavelet coherence. Here the analysis reveals the coherence at monthly scales. During a weakened

MOC, there is an antiphase coupling in the SO at ~1 year time scale. This means that increases in MOC rapidly produce NO3:CHL increases in the SO, i.e. HNLC intensification. The pattern in the SNP is more complex because it is on antiphase. This is, MOC increases reduce NO3:chl ratios which would indicate that MOC intensification putatively affects Fe availability in this region, therefore, increasing ocean productivity. In general, our results reveal that, although significant after 2010, MOC variations produce short-time (<1 yr) responses in the three systems.

(4) The assertions regarding model-data agreement for NO3 (e.g., 5/50) are still unconvincing. Again, if you look at concentrations over depths where a strong vertical gradient exists, you are always going to get a strong correlation. My previous suggestion to look at surface concentrations only was not followed.

We believe that this is not well explained in the M&M section. We are comparing 'mean 0-20 m' NO3 values. Average values are used because surface sampling depths vary between cruises. Multiple casts do not contain surface (0m) values. While a 0-20 m average may somewhat filter undetectable NO3 levels at the surface we do not think that it should highly influence overall correlation. Indeed, CMEMS has published a quality assessment document of their modeling products in which NO3 data are compared with measurements of Argo data with similar results (see Page 27/ 49, https://catalogue.marine.copernicus.eu/documents/QUID/CMEMS-GLO-QUID-001-029.pdf). 'Overall, the model and WOA-2013 climatologies are in good agreement. Exceptions can be pointed out in the Southern Ocean, where nitrate levels are too strong in the model, and along the Arabian Peninsula (Yemen and Oman) and in the Bay of Bengal where nitrates are significantly weaker in the model'. General correlation is similar to our analysis (0.98), however, they use the complete dataset (all depths).

(5) On p. 10 it is stated that "It is noteworthy that nutrient concentrations are generally lower in the SNP (i.e. <17 mmol m-3) than in the SO while biomass is comparatively higher (see Table 1)." This is true, but when I look at the Table, the near-constant value of [NO3] within each Rx cluster is quite remarkable. I interpret this as evidence for the robustness of the method, and I think this is something that could be commented on in the Discussion.

We appreciated this comment that has been included in the first paragraph of the discussion section.

(6) Is there any evidence for a "regime shift" in 2009-2010? I am wondering if any of the cited references discuss this.

There are multiple pieces of evidence in the literature of this event, including changes in MOC, variations in the Pacific sea level, and changes in chlorophyll. However, we have not found any in-depth analysis of the causes and long-term consequences of this episode.

Some details:

1/32 change "budgets" to "inventory"

Changed

2/35 Brindley misspelled (see also 25/26); Tyrrell misspelled

Corrected

2/48-49 change "iron is required in largest amounts than any of the trace metals" to "iron requirements are the largest among the trace metals"
Changed
2/53 change "nutrient" to "macronutrient"; delete "often"
Changed
3/70 change "biological structure" to "ecosystem structure"
Changed
3/73-82 could add some additional literature citations here e.g., 10.1038/s41598-018-37436-3 (note that the method appears to detect two patterns that are seasonally HNLC: see Figure 3)
Ref to Birchill et al has been included.
3/81-82 change "structural and functioning similitudes" to "structural and functional similarities"
Changed
3/97 change "in" to "on" and "response" to "responses"
Corrected
4/21 delete "that varies with the latitude"
Deleted
4/35-5/40 Something is wrong here: the text takes an abrupt zag from model description to climate indices and back again. Some text was accidentally spliced in the middle of another paragraph.
The reviewer is correct. In this version, for unknown reasons, the paragraph was spliced in the middle of another paragraph. We apologize.
9/81 "is not more exceptional" I can't really tell what this means
It should read 'is more exceptional'. Corrected
9/84 I would consider also citing 10.1038/nature07716 here
We have included th suggested reference (Pollard et al., 2009).
9/94 change "distribution" to "mode"
Changed
10/15 delete "coupled"
Deleted
10/16-17 I would replace "since" with a ; or a : and change "duplicates" to "doubles"
Changed as suggested
10/19 change "biomass" to "chlorophyll"
Changed
10/22 I would consider also citing Harrison et al 2004 here
Done
11/38 "Most differentiated patterns, also displaying the highest probability of occurrence (the probability to find a pattern similar to the input data)" The most differentiated patterns, also displaying the highest probability of occurrence (the probability of finding a pattern similar to the input data)
Corrected
11/41 "scenarios" seems like an odd choice of terms here
Changed by patterns
11/52 "winter" should be spring (April)?
Yes, it is April (spring)
11/64 the positive anomaly appears larger in April than in March
Corrected

12/65-66 "boreal winter" should be "austral winter"? 25% exceeds the limits of the graph which shows max deviation as <20%
Corrected
12/78 delete "the year"
Deleted
12/78-79 "when ENSO variability intensified" Is there a data or literature reference for this? I have not heard of this before.
There is a paper by Cai et al 2022 on ENSO variability. However, we refer to Fig 7b. The sentence has been rephrased.
12/92 delete "the analysis of"
Deleted
13/98 change "discriminate" to "delineate"
Changed.
13/04-05 mention the physical ocean as well here (e.g., upwelling)?
A mention to physical ocean processes has been included
13/10 delete "fields"
Removed
13/12 change "complex" to "diverse"
Changed.
13/17 delete "unproductive"
Removed
13/17-18 "the SO is the largest region presenting clear latitudinal variation in the characteristic Chl patterns" This seems to conflate two separate points: the SO is the largest HNLC region and it is the ONLY one that shows clear latitudinal variation in the characteristic Chl patterns (Figure 3).
It has been rephrased to: 'the SO is the largest region and it is the only one presenting clear latitudinal variation in the characteristic Chl patterns'
13/21-22 change "Both the SNP and the EEP respectively constitute 8% of the total HNLC" to "The SNP and the EEP each constitute ~8% of the total HNLC"
Corrected.
14/30 2019 should be 2009?
Yes. Corrected.
14/31-33 "This non-linear enhancement in phytoplankton, which is not exclusive to oceanic Fe-limited waters (see for example Marrari et al., 2017), positively biases the Chl increase rate in these subregions." Meaning is not clear, and the entire sentence is probably expendable.
We agree. It was intended to be emphatical but it is not necessary.
14/61-62 "where seasonality is marginal" This does not appear to be the case in Figure 6.
We changed 'marginal' to 'weak'. As shown in Fig. 6, the maximum values of % variation at EEP are 8% while the other two regions display much higher variability (20% and 50%).
15/63 "subregions with 6-month out-phased seasonal variations (north and south of the equator)" data reference? where is this shown?
We do not think that a reference is required here since it is quiet straightforward (although may be not well expressed). Seasonality is 6-month out-phased in the northern and southern hemispheres (i.e winter in the N is summer in the S). The EEP extends over both hemispheres, and although the seasonal signal at these latitudes is weaker than at higher latitudes, it is not negligible.
15/71 "the seaways in the Pacific" circulation pathways?
Yes, corrected

15/78-79 "corresponds to a rather independently functioning intermediate water cell" rather corresponds to an independently functioning intermediate water cell

Changed as suggested

15/79-82 references to MOC here are ambiguous: if they are talking about the global MOC rather than PMOC it might be a good idea to put "global" before "MOC" for clarity (see also 15/87 "meridional circulation")

It refers to PMOC. Corrected

15/88 change "variations" to "variability"

Changed

16/06 "in this region" unclear antecedent; if still talking about the EEP here, please specify

Yes. It is specified now.

16/08 "the ENSO index" which index? there are many ENSO indices to choose from

This is detailed in M&M section, Pag 3. 'El Niño Southern Oscillation Index (MEI.v2), hereafter ENSO index'

16/12 delete "events"

Deleted

16/12-14 I think this whole sentence is expendable.

Deleted

16/20 change "ENSO-related equator-originated sea surface height anomalies" to "ENSO-related sea surface height anomalies originating in the tropics"

Changed as suggested

16/23 change "which nicely explains the fluctuations of salinity, nutrients, and chlorophyll" to "which explains strongly correlated fluctuations of salinity, nutrients, and chlorophyll"

Changed as suggested

17/34 "Figure 8b also reveals a decline of the MOC until 2010". This Figure does not appear to exist. (and change "until" to "around")

It should have read Fig. 7b (now 7e). Corrected

17/46 "the slowing down of the overturning circulation in the Pacific Ocean since the 1970s" This statement seems incongruous given that the cited reference is > 20 years old. Do we know that this trend continued or consolidated? Or is this a case of interdecadal variability that manifests as a slowdown over the period studied but has since reversed?

Detailed information on long-term PMOC variability and its influence on EEP biogeochemistry is scarce. We are aware that the data reported by McPhaden and Zhang, 2002 is old and probably reflects a subdecadal variability. Nevertheless, it provides evidence of the proposed relationship between PMOC and equatorial upwelling. We have rephrased the sentence to evidence that this is not a general trend.

17/52 "an atmosphere's energy balance indicator" I don't think this is necessary.

Removed

17/58 change "reduce" to "reduced"

Changed

18/63 change "is considered" to "occurs"

Changed

18/63 change "Paleoceanographical records reveal a strong correlation between proxies of aeolian Fe flux and productivity has been reported" to "Paleoceanographical records showing a strong correlation between proxies of aeolian Fe flux and productivity have been reported"

Corrected

18/65 "in present times, dust deposition in this area has notably varied" Is there a literature reference for this (e.g., 10.1073/pnas.0607657104)?

Reference to McConell has been included

18/76 "further evidence of the global scale coupling and feedback between the atmosphere, the ocean, and global productivity variations" Possibly this is true, but nothing shown in this paper necessarily depends on ocean-atmosphere feedbacks. Similarly with "anomalies in global forcing intensity". It's not clear what is meant by "forcing intensity" here, but I do not think anything shown here requires a change in e.g., global net forcing of climate by GHGs, or a change in the global mean surface ocean wind stress.

"further evidence of…' has been removed and "forcing intensity" has been changed by global ocean circulation patterns

191/4 URL for CARINA is outdated (should be ncei.noaa.gov)

Corrected

19/22 Aumont reference cites Discussion paper; should cite final published version

Corrected

20/50 Nojiri misspelled

Corrected

Reference format is still inconsistent in that journal titles are sometimes abbreviated, sometimes not; sometimes all words capitalized, sometimes not; author names sometimes spelled out, sometimes not;

The reference format has been corrected following the bibliographic style of the journal.

Table 2 caption should define "Max variation" and state monthly or annual mean data

It now reads 'Basic statistics of the annual extent of each of the SOM-defined HNLC subregions during the analyzed period (1998-2017). Maximum variation (Max. Variation) is calculated as the difference between the maximum and the minimum extent'.

Figure 1 caption "SOM time-domain analysis at global and regional means" meaning not clear; means of what?

It should read 'and' global and regional mean series of each region. Corrected.

Figure 2 caption "isolines are drawn at 4 mmol m-3 intervals" are they?

No, the reviewer is right. Some of the isolines were removed for clarity. It has been removed.

Figure 4 caption state monthly or annual mean data

We state now that, consistently with the description in M&M section, the spatial patterns were obtained from monthly data.

Figure 6 caption "extent" is still spelled as "extension"

Corrected

Figure 7 - the numbering scheme is unusual. I see the logic of it but it may violate journal standard.

The numbering scheme in Figure 7 has been changed to a more standard notation (a, b….,h)