Reply comments Referee #1:

This paper applies several novel statistical methods to assess the extent and variability of the ocean's three major "high-nutrient / low-chlorophyll" (HNLC) regions. Its novel contributions include articulating a new definition of HNLC in terms of the ratio NO3/Chl, and identifying new linkages between HNLC extent and some climate variability indices.

We would like to sincerely acknowledge RC1 for the outstanding review of this manuscript. It is rare these days to receive such detailed and appropriate feedback. His/her comments have undoubtedly contributed to a significant improvement of the manuscript. We highly appreciate his/her dedication.

I think this is a good paper that is publishable with revisions. The English is mostly good although there are some quirks. However, I lean toward major rather than minor revision for this reason: some papers that combine Results and Discussion cry out for the separation of the two, and this is one of them. I think it would be much stronger if it were rewritten in the standard I-M-R-D format. Start with: What did we learn and what is the evidence? (Results) and then: What does it mean in the context of the existing literature and potential future research? (Discussion). At present, the results of this research are mixed up with speculation and literature review in a way that detracts from the paper's core messages and makes it difficult for the reader to identify what exactly the research conducted demonstrates. There are also passages in the Methods that I think more properly belong in the Discussion (e.g., 124-129).

We have addressed most of the issues suggested and thoroughly reviewed English grammar. Admittedly, we were reluctant to rewrite the R&D section in the standard I-M-R-D format because the main text could become excessively long and, eventually, repetitive. This problem is common to many papers based on satellite information in which detailed descriptions of spatial fields of different variables are made. However, following the reviewer's suggestion we have segregated results from the discussion and we recognize that, in this case, it may be a good option. We have also trimmed the conclusions section to highlight the fundamental points of the manuscript, and, as the reviewer suggests, we have moved lines 124-129 to the Discussion section.

I also do not think that the statistical methods are adequately explained. On 182 we have "Statistically significant trends were considered those exceeding the 95% confidence level." This would seem to be a straightforward significance test of simple linear regression. But even here some more detail is needed, e.g., what is the decorrelation time scale and therefore the effective sample size? (see e.g., www.sciencedirect.com/science/article/pii/B978012387782600003X) When we get to the CWT (which term appears only twice and is defined differently each time), we are simply told that "The thick black contour designates the 95% confidence level" (Figure 6 caption). The text says nothing about how the confidence level is calculated (see also Figure 7). It is stated in the Supplement that "Monte Carlo simulations based on two uniform white noise time series are used to determine the significance level", but more detail is required and this should be at least mentioned in the main text. Ultimately, the variation in the total HNLC extent is found to be on the order of

5% (446). How do we know this is significant and not just noise? The coherence across regions and with the climate indices suggests that it is, but the lack of clarity regarding methods, and the sometimes too-good-to-be true correlations (see next paragraph) detract from the presentation.

We have improved some methodological descriptions. Specifically, linear trends were determined by Theil-Sen slope adjustment [Sen, 1968] of the residuals of the deseasonalized series. The Theil–Sen estimator is a well-known estimator of the true slope based on non-parametric that has low sensitivity to outliers, by taking the median of all possible slopes between pairs of data, instead of the mean. The significance test for the slope is a test for the trend. Correlation analyses were performed using Kendall's tau correlation coefficient.

The CWT (Continuous Wavelet Transform) is a signal processing and data analysis technique generally used to decompose a signal into different time scales and analyze its frequency content at each scale. This is achieved using functions called "wavelets", which are waveforms that can expand or contract in time to adapt to different scales. The CWT must first be applied to the signal to obtain a representation of the signal in the wavelet space. The amplitude or intensity values at each time scale can then be calculated. The significance of the CWT is assessed by comparing the background power spectrum with Monte Carlo generated red noise. The Monte Carlo method is used to generate a set of simulated data that has the same statistical characteristics as the original signal. This is done using a known probability distribution function, such as a normal or uniform distribution.

Finally, the amplitude values of the original signal are compared to the amplitude values of the simulated data to determine whether they are statistically significant or not. This is done using statistical tests, such as the Student's t-test or the Wilcoxon test. If the amplitude values of the original signal are statistically significant compared to the amplitude values of the simulated data, it can be concluded that there are specific patterns or characteristics present in the original signal that are not simply the result of noise. Using shaded regions to represent the level of significance in the final figure of the CWT analysis is a visual way to indicate which amplitude values are statistically significant and which are not.

To analyze the correlation between the continuous wavelet transform of two signals we compute the cross-wavelet coherence analysis (CWA), which has been properly mentioned in the new version of the manuscript. We skipped this sentence in the previous version of the manuscript. The significance of the wavelet coherence is also assessed using Monte Carlo methods, but now generating pairs of red noise data. We have improved the description of CWT and the CWA adding part of this discussion in the M&M Section.

The estimation of the interannual variation of the HNLC areas is based on the spatial patterns of HNLC obtained from the SOM analysis applied to global ocean data. This regional partitioning is made on a global scale with global criteria and therefore leads to a large-scale smoothing, which could impact the values of the variation of the areas. However, as this signal smoothing is common to all the areas, this should not have any effect on the regional comparison of the area variation.

In 3.3 we have "The relationship observed in interannual variations in HNLC areas suggest a global scale coupling between the equator and the poles. Good inverse correlation (r=-0.99, n=20) is observed between the interannual variations in the extension of EEP and the SO, and a weaker thought significant relationship exists between the SNP and the EEP (325 r= -0.75, n=20). Therefore, as the extension of HNLC in polar regions contracts (biomass increases), the equatorial region expands and vice versa. All three regions exhibit a shift in their extension after 2010 (Fig. 5)." I find a correlation of -0.99 difficult to credit. But when I look at Figure 5 the SO and EEP interannual time-series do indeed look like mirror images of one another. But how is this possible? What physical process could account for it? This is the kind of result that readers will dismiss without more attention to detail. But the discussion following this result is vague and speculative. I think the authors need to work harder at explaining how such a tight coupling could exist and convincing the reader that it is not just an artifact of their analysis method.

Thanks for pointing out this seemingly strange correlation which may raise doubts about the calculations. We have thoroughly reviewed the calculations and we stand that it is not an artifact or an error. However, the extent variability estimations were performed based on the number of pixels considered HNLC according to the criteria established in Section 3.1. We have recalculated the values of the areas in terms of actual surface (Km2), instead of the number of pixels, by considering the latitudinal variation in pixel extent. Figure R1 shows the results of these computations. While maintaining the general pattern, interannual variations are slightly different and, indeed, the correlation between NPP and EEP, and between SO and EPP reduces to - 0.49, -0.97. Beyond this correction, the correlation between SO and EEP extent is still outstandingly good, which suggests that both systems respond to a common forcing. Both ENSO and, as explained below, MOC are common to the EEP and the SO, and less so to the SNP. We discuss this in the new version of the paper. However, a clear mechanistic understanding of the coupling between the EEP and the SO would require information beyond the scope of the present study.

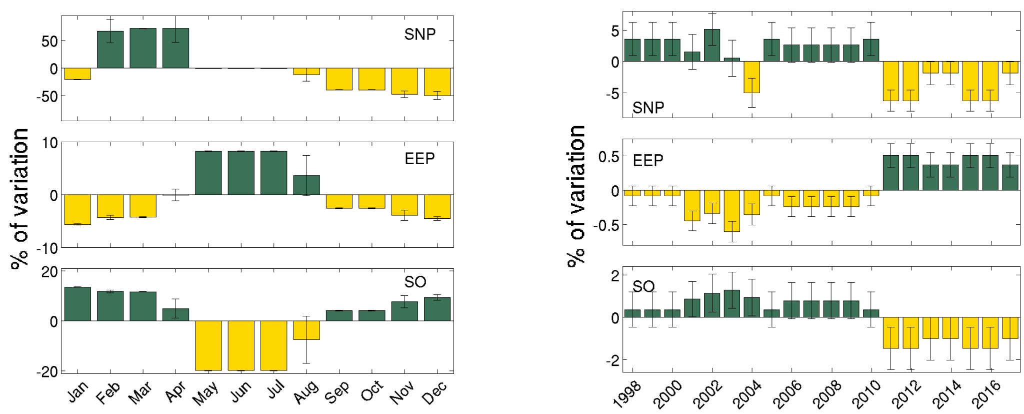

Figure R1. Seasonal (left) and interannual variations (right) in the spatial extension of the three HNLC regions. Variations are referred to the mean extension of each region. The blue dashed lines indicate the regime shift occurring after 2010.

The EEP is a peculiar region that integrates subregions with 6-month out-phased seasonal variations. To better understand its variability we have split the analysis of the EEP into North-Equator and South-Equator regions. In Figure R1 we show that the southern equatorial region contributes more significantly to the mean extent of the EEP whereas the Northern subregion determines a large part of the observed variability (see also Fig R2 and R3). This suggests that the Equatorial signal is dominated by the large variability shown in the northern equatorial region.

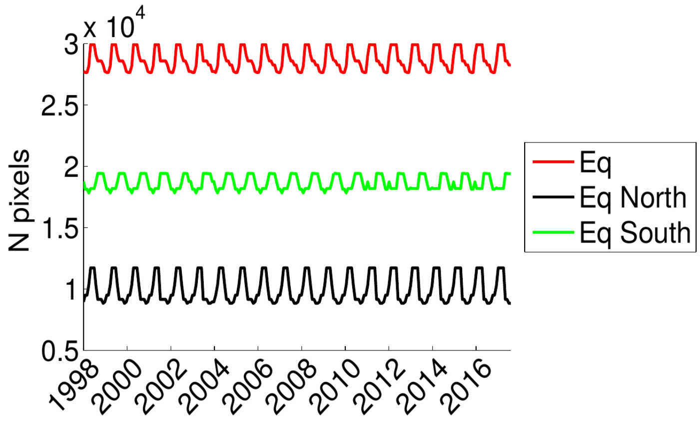

Fig R2. Time evolution of the total number of pixels covering each area: Whole equatorial region (in red). Pixels in the equatorial region which is located in the northern hemisphere are shown in black and those in the southern hemisphere in green. Note that while the mean value in the Southern hemisphere is larger, the northern region is more variable.

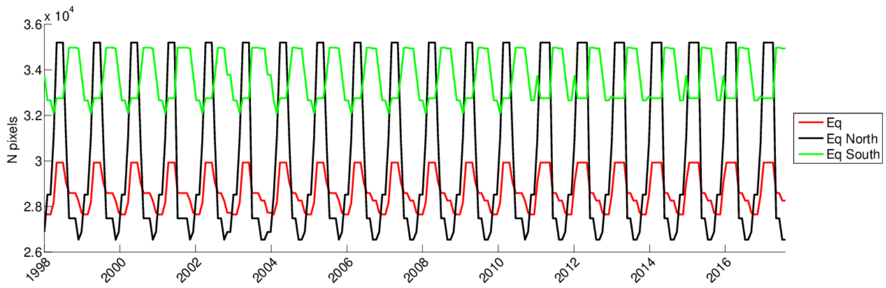

Fig R3. Time evolution of the total number of pixels covering each area scaled to the same range of values for better comparison: Whole equatorial (in red). Pixels in the equatorial region which is located in the northern hemisphere are shown in black and those in the southern hemisphere in green.

In Figure 7 we see an abrupt decline in the MOC around 2010 and then recovery to a level around 17 Sv which is both lower and more stable than before 2010. The authors seem to attribute a great deal of influence on oceanographic processes in all of the HNLC regions to this apparent "regime shift" (e.g., 404-406, 412-414). But given all of the higher frequency variability that is present in both periods, are the means for before and after 2010 even significantly different?

According to Moat et al. (2020), who suggest a previously unsuspected role for the AMOC in climate variability, the low AMOC event in 2009-10 coincided with a cold winter in Europe. MOI presents seasonal variability and it can be decomposed into a (1) seasonal, (2) irregular, and (3) interannual trend using different methods. We have used census-x11 method to identify each component (see R4). As shown in the figure below, the interannual signal for 2004-2009 yielded a mean value of 18.5±1 Sv, whereas the mean for 2010-2018 was 16.6±1 Sv. During the anomaly (2009-2010), a mean value of 14.4 ±1.7 Sv was obtained. A detailed analysis of this change in meridional transport including a change-point analysis can be found in Moat et al. (2020). They report a change in the trend between 2008 and 2010, depending on the criterion selected (Fig. R5).

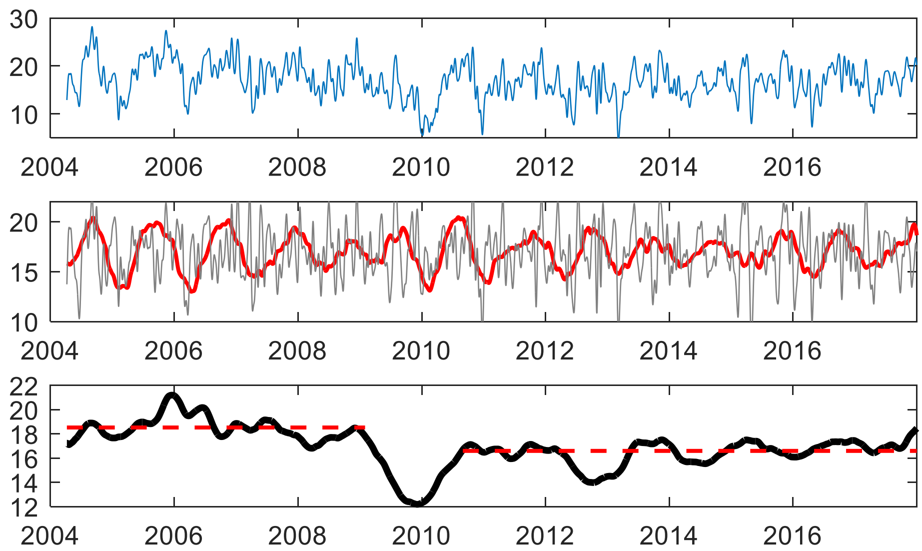

Fig. R4 Decomposition of MOI into (1) seasonal, (2) irregular, and (3) interannual trends.

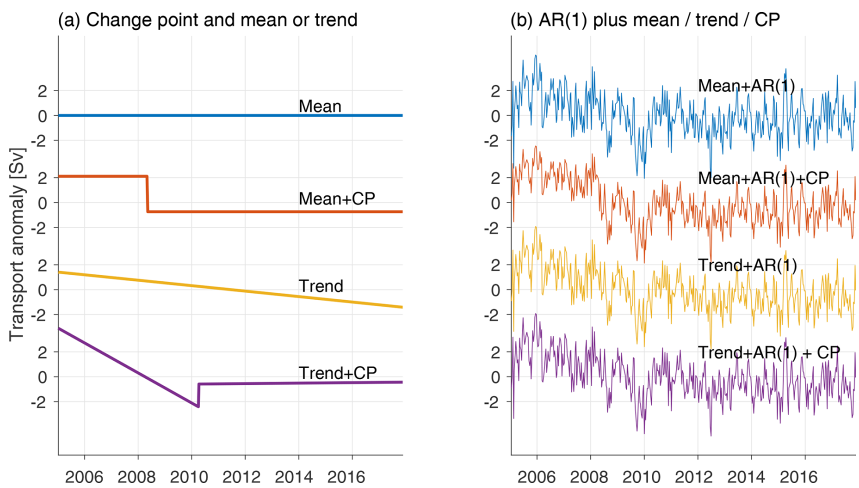

Figure R5. Change-point analysis of the AMOC–Ekman time series. (Moat et al. 2020).

The seasonal and interannual variability of the area are scales of variability that cannot be intercompared, or compared to the high-frequency signal. They represent different signals of the time variability of the HNLC area extensions. The statistical deviations of the mean area variability values have been shown as error bars in Figure R1. This allows us to better compare how the mean variability statistically spread from the mean value (at the same time scales).

This seems like a question one would wish to ask before attributing far-reaching effects to this rather modest decline. Here again the mixing up of presentation of the results with discussion and speculation undermines the credibility of some of the claims made. I am particularly skeptical regarding claims that the MOC affects the extent of the SNP HNLC (406), and to a lesser degree the EEP one, and find the discussion of the underlying mechanisms to be quite speculative.

As we mentioned before, we have segregated results from the discussion in this new version. We agree that there is a degree of speculation in our discussion since the analysis is based on observations and we cannot identify mechanistic relationships. We can just suggest plausible explanations for the observed variability. Nevertheless, our data clearly shows a change in 2010 in the extension of HNLC regions that is in agreement with an event of low AMOC transport suggesting an influence of meridional overturning on the productivity of HNLC regions(see figure below).

A dynamically changing meridional circulation has significant implications for equatorial upwelling since, among other effects, influences the supply of nutrients to the biologically productive surface layer of the ocean. Meridional overturning depends, to a large degree, on deep-water formation, which, occurs in the high latitudes of the North Atlantic basin but not in the North Pacific. The most fundamental distinctions are (1) that the SNP does not ventilate the deep ocean at significant rates (Warren et al., 1983) and (2) that PMOC cell at this latitude corresponds to a rather independently functioning intermediate water cell. Indeed, according to Sigman et al. (2021), the SNP possesses an analog to the Southern Ocean's "upper cell" but lacks a clear analog to the Southern Ocean's "lower cell." Also, because is a shallower process, is more influenced by wind-driven processes and, therefore, presents greater interannual variability. It seems straightforward the influence of AMOC in the SO since waters are upwelled in this region. As in the case of AMOC, most subducted North Pacific deep water also upwells in the Southern Ocean (Thomas et al., 2021) and, therefore, a similar effect is expected in the SO. I n the case of EEP, variability may depend on factors such as the strength of shallow overturning cells near the equator in the Indian–Pacific basin and on the inter-connectivity of the PMOC water with the rest of the global ocean.

In the case of SNP, the processes by which high-nutrient waters are maintained in the subarctic Pacific surface are not understood due to a lack of knowledge of the whole and detailed mechanisms by which nutrients return to the surface layer (Nishioka et al. 2020). The SNP lacks the deep circumpolar channel that characterizes the Southern Ocean and that allows the Ekman upwelling to draw large quantities of deep, dense, nutrient-rich water to the surface. Rather, the meridional overturning cell subsystem of the North Pacific Intermediate Water seems mostly unconnected from deeper overturning cells (Talley 2013). The nutrient richness of the SNP upper water column appears to depend partly on diffusion-driven upwelling at the base of the pycnocline, which is enhanced by turbulence near steep bathymetric features (Nishioka et al., 2020). As we mentioned, MOC in this region is reportedly weak (1-4 SV) and extends no further than 50oN (Fujio et al., 1992; Ishizaki, 1994; Yaremchuk, 2001). However, there is evidence showing the response of this region to changes in PMOC (Burls et al, 2017). For example, it has been observed in TOPEX altimeter data that MOC influences the basin-scale baroclinic circulation in the SNP (Kuragano & Kamachi, 2004).

In conclusion, the global overturning pathways for the well-ventilated North Atlantic Deep Water and Antarctic Bottom Water and the diffusively formed Indian Deep Water and Pacific Deep Water are intertwined (Talley, 2013). Accordingly, our results suggest a global scenario in which HNLC regions are susceptible to interbasin teleconnections rather than to local forcings. These general patterns can be modulated by feedbacks between different forcings. For example, PMOC variability and El Niño–Southern Oscillation are known to positively correlate (Tandon et al. 2020). In any case, we still need a clear understanding of the biological responses to these climate scale interactions in HNLC waters.

Fujio, S., T. Kadowaki and N. Imasato (1992): World ocean circulation diagnostically derived from hydrographic and wind stress fields, 1. The velocity field. J. Geophys. Res., 97, 11163–11176.
Ishizaki, H. (1994): A simulation of the abyssal circulation in the North Pacific Ocean. Part II: Theoretical Rationale. J. Phys. Oceanogr., 24, 1941–1954

Yaremchuk, M. I. (2001): A reconstruction of large-scale circulation in the Pacific Ocean north of 10°N. J. Geophys. Res., 106, 2331–2344.

Kuragano & Kamachi, 2004Balance of Volume Transports between Horizontal Circulation and Meridional Overturn in the North Pacific Subarctic RegionJournal of Oceanography, Vol. 60, pp. 439 to 451, 2004

Thomas, M. D., Fedorov, A. V., Burls, N. J., & Liu, W. (2021). Oceanic pathways of an active Pacific meridional overturning circulation (PMOC). Geophysical Research Letters, 48, e2020GL091935. https://doi.org/10.1029/2020GL091935

McPhaden, M., Zhang, D. Slowdown of the meridional overturning circulation in the upper Pacific Ocean. Nature 415, 603–608 (2002). https://doi.org/10.1038/415603a

Trenberth, K. E., & Hurrell, J. W. (1994). Decadal atmosphere-ocean variations in the Pacific. Climate Dynamics, 9, 303-319. doi:10.1007/BF00204745

Schneider N., Miller, A. J., Alexander, A. & Deser, C. Subduction of decadal North Pacific temperature anomalies: Observations and dynamics. J. Phys. Oceanogr. 29, 1056–1070 (1999).

Warren B. A., Why is no deep water formed in the North Pacific? J. Mar. Res. 41, 327–347 (1983)

Talley LD (2013) Closure of the global overturning circulation through the Indian, Pacifc, and Southern Oceans. Oceanogr 26(1):80–97.

The "three major climate variability signals" (20) or "three main forcings" (95) of SST, ENSO, and MOC seems like a list that combines apples and oranges. In global mean SST, the biggest component of variability beyond the annual cycle is ENSO. So why does SST need to be included in this list? Anyway it appears that mean SST as an independent variable controlling HNLC extent is never actually discussed in the paper anyway; the cross-wavelet coherence results in Figure 7 are for ENSO and MOC. So why is it given such a prominent place in the Abstract and Introduction? This could confuse the reader about what their overall purpose is. (It is also probably an exaggeration to state that they "quantify the ... dynamic relationship between the observed Chl variability and three main forcings" (95). They do quantify the cross-wavelet coherence of NO3/Chl with the climate indices, but the discussion of the underlying physical processes is quite speculative.)

We recognize that the reviewer has a point here. There is no rigorous need to include SST and, in any case, it is not analyzed or discussed. We have removed the SST analysis.

The model-data comparison for NO3 could be expanded on a bit, e.g., "we found good agreement between nitrate in situ data and model results (r2=0.98)" (123). I think there should be Supplemental figures or tables that show the space/time domain of these comparisons, and break them down a bit more by region. To reproduce the gross spatial pattern of surface NO3, or especially the surface-to-deep gradient, is a very weak test of model skill. If one throws together data from all depths and from HNLC and nutrient-depleted subtropical waters, of course, you will get a strong correlation. If one looked only at e.g., surface concentrations in the SNP, one would get a very different result. How about including a Supplemental table that shows the correlation coefficients for the three major HNLC regions, for surface concentrations only?

Correlation coefficients between measured and modeled data for SO, SNP, and EEP are 0.74, 0.74, and 0.77 respectively. Nutrient concentrations in the SO are overestimated in 7.2 mmol m-3, and this has been corrected in our data. This is mentioned in the reviewed manuscript. The average profiles for each region are shown below.

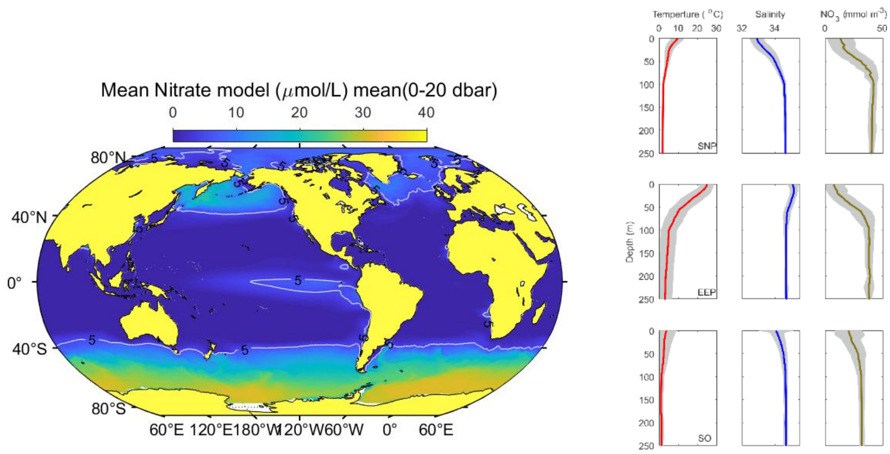

Fig. R3. Mean global nitrate concentration and vertical profiles for each region.

Some details (Note that I have not listed here the numerous passages of Discussion that are vague or excessively speculative, but the authors should take note that there are many of these and try to trim them down (or shore them up with detail) as they restructure the paper overall.)

32 "and, therefore, of the withdrawal of atmospheric CO2" Withdrawal by what process on what time scale? HNLC regions per se do not affect atmospheric CO2, unless their extent is altered by changes in external supply of iron as suggested by Martin 1990 (see also 329)

We agree that the sentence was not clear. HNLC extent influences the potential co2 withdrawal since an increase in their global extent would result in a less productive ocean. Now it reads: ‘their extent influences the potential withdrawal of atmospheric CO2 to the deep ocean’

53 "oligoelements" unnecessary jargon. Changed by ‘elements’

60 "coarsely known aspects" not clear what this means. The sentence has been rephrased to ‘Only general aspects such as expected shifts in phytoplankton community composition or changes in Fe-cycling rates have been addressed to date (Fu et al., 2016; Lauderdale et al., 2020).

73 "it is arguable if these ephemeral systems share structural and functioning similitudes with the large HNLC regions" it is uncertain whether these ephemeral systems share structural and functional similarities with the large HNLC regions

Thank you. Corrected

82 "reporting a positive North Pacific Gyre Oscillation (NPGO) and nutrient correlation" reporting that surface nutrient concentration was correlated with the North Pacific Gyre Oscillation (NPGO)

Corrected

89 change "nutrient outputs" to "nutrient concentrations"

Corrected

112 "a good indicator to describe the value overall phytoplankton trend" a good indicator of the magnitude of the overall phytoplankton trend?

Corrected

131 change "climatological indices" to "climate indices"

Corrected

218 change "the pauperized subtropical gyres" to "low-latitude oceanic waters"

changed

220-224 I think this discussion neglects Eastern Boundary Currents, which represent one of the largest areas of consistently high Chl + high NO3

Yes, we mention in the introduction these regions. Eastern boundary currents are not HNLC regions and therefore we do not discuss them.

225 delete "i.e."

deleted

231 delete "values"

deleted

243 "has remained elusive since ... requires coherent information" something missing here

The sentence has been rephrased. Now it reads: Systematically determining the boundaries of HNLC regions has remained elusive as it requires coherent information from both nutrient and Chl fields

245 I actually think Figure S2 could be in the main text. It would help the reader to understand what the authors are doing.

Figure S2 has been included now as Fig.1

246 change "corresponding" to "correspond"

changed

252 change "therein" to "there"

changed

260 add a comma after "ratios"

added

262 change "ice sheet" to "sea ice"

changed

265 "exhibits a differentiated dynamic" I can't tell what this means.

Changed by: exhibits distinct Chl variability patterns.

268 "phenological variations" I don't think this term is useful or necessary here

It now reads: but these variations

269 "This region is also subjected to zonal variations" How about "This region has distinctive eastern and western regions"?

Changed by: This region exhibits distinctive eastern and western provinces

275 "in which ocean productivity ... importance of advection of Fe" something missing here

It now reads: is a more enclosed basin in which ocean productivity is driven by the advection of Fe

286 "trend robustness is provided by the coherence in the time series obtained using SOM" I can't tell what this means

It now reads: In our case, robustness in trend analysis is provided by the spatial coherence in the time series obtained using SOM classification. This methodology is based on the similarities in the temporal variability patterns and it clusters regions with similar trends and variability.

290 "not exclusive of oceanic Fe-limited waters, since it has been also observed in the highly productive Patagonian shelf" Not clear what they are trying to say here. The Patagonian shelf is not oceanic and is not Fe-limited.

Correct. This is our point. We report a shift that it is also observed in other southern regions (not only in HNLC regions)

313 add ", respectively" after "100% in April and 70% in July"

Corrected

321 "The extension of the HNLC region in the boreal winter is the boreal winter is 25%" ???

Some words were missing. Now it reads: Indeed, the extent of the HNLC region in the boreal winter is 25% lower than the mean annual extent.

325 change "thought" to "though"

corrected

343 " As shown in Figures 6 a and b, the temporal variability of both the characteristic NO3:Chl ratios and SST at each region peaks at 12-month periodicity, being this seasonal modulation more intense and temporally consistent in the case of temperature at high latitudes and weaker in the equator." This is very poor scientific writing. The result being presented is rather mundane: the most obvious detectable periodicity is the annual cycle, and seasonality is stronger at higher latitudes. Please rewrite.

References to SST have been deleted, following the revierwers' recommendations

346 "transference from annual to semiannual periods since 2010" Not sure what the right word is here but I am fairly sure "transference" is not it. How about "display a semiannual mode, which accounts for a larger fraction of variance after 2010"?

Thank you. We have rephrased the sentence

349 change "in" to "on"

changed

353 delete "value"

changed

353 change "phytoplankton uptake" to "phytoplankton biomass"

changed

354 "Semiannual cycles" I try to avoid referring to variability at periods other than annual as "cycles" (excepting Milankovitch frequencies of course, but this paper is concerned with subannual to decadal scales) (see also 355, 433)

While we believe that conceptually is not incorrect, we agree that the use of cycles is arguable when a variation is not markedly repetitive, like tidal cycles. We have rephrased the sentence to: Semiannual variability generally occurs in regions where warming and cooling phases show different durations.

368 change "Contrastingly" to "Conversely" or "By contrast"

Changed

396 change "phase out" to "out of phase"

Changed

396 "suggesting a meridional propagation of the MOC effect" vague

The sentence has been rephrased

415 add a comma after "AMOC"

Added

416 change "more unclear" to "less clear"

Changed

417 sea ice or glacier ice?

Riebesel et al 2009 mention sea ice

419 delete ", also based on remotely sensed Chl,"

deleted

424 change "this effect is unlikely" to "this effect is unlikely to be important at the time scales considered here"

Changed

442 "retrieved from the increasingly improved and longer and longer time series of remote sensing observations" retrieved from time series of remote sensing observations of increasing duration and quality

changed

445 delete "through complex processes"

deleted

Figure 1 - the white contour lines are difficult to see in some places

Contourlines have been changed to black.

Figure 2 - the red lines that indicate linear trends don't look like straight lines to me, but it's hard to tell

Red lines (now blue) are straight. However, we acknowledge that because of the image resolution they appeared somewhat stepwise. We have increased image resolution and the colors have been switched to improve the visibility of the line.

Figure 5 - what exactly the y axis represents is not clearly explained; the meaning of the different colored bars is fairly obvious but should still be stated

We have modified the legend. Now it reads: Figure 6. Seasonal (left) and interannual variations (right) in the spatial extent of the three HNLC regions, are represented as a percentage of variation from the mean extent of each region. Dark and light coloured bars indicated positive and negative values, respectively. The blue dashed lines indicate the regime shift occurring after 2010.

English/formatting

One quirk of English usage that appears over and over is using the word "extension" instead of "extent". There are 27 in total and I think "extent" is more appropriate in virtually every case. Another is using "at" in place of "in" a region, e.g., "at SO", "at the EEP". I would write "in the XXX" in all cases. (Interestingly, it used to be fairly common to use "at" wrt cities, as in "I attended the AGU meeting at San Francisco". But this fell out of common use a long time ago.)

We apologize for the misuse of the noun extension (‘expansion’) instead of ‘extent’ which refers to a range of locations, being more appropriate in this case. It has been corrected all through the document. As non-native speakers, we also appreciate clarification about the use of ‘at’. This has also been corrected.

Numerous references are missing from the reference list, e.g., Garnesson/Grarnesson et al., 2019 (spelling varies); Green et al., 2017; Ibanhez et al., 2017; Kumar et al., 1995; Martínez-García et al., 2009; Qui, 2002 (probably Qiu). This is NOT an exhaustive list. I have doubts about whether Takeda 2011 is a traceable reference (searching on the doi turned up only stale links).The reference format is inconsistent in the sense that multiple references within a parenthesis are sometimes arranged alphabetically, sometimes chronologically.

All references have been reviewed and arranged chronologically in the parenthesis. Regarding Takeda, you are right. The paper is freely available from several sources (researchgate, semanticscholar) but the doi link does not work. We have removed this link.

Reply comments Referee #2:

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).

We are grateful for the generally positive review of RC2. We have improved some aspects of the M&M section and a figure from supplementary material is now included in the main text as Fig.1

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.

The aim of the SOM analysis is precisely to replace this type of traditional climatology analysis with more objective, quantitative, and precise approaches. SOM analysis allows the reconstruction of the spatial patterns and their temporal dynamics (seasonal and interannual) of the study variables (i.e. using the results shown in Figures 4, 5, and 6). The SOM mapping has advantages over other methodologies, e.g. monthly climatology, maps of inter-annual cycles), as relevant time and spatial scales are unveiled without any prefixed assumption. For instance, if two different spatial patterns occur in the same month with the same probability, using a monthly average only one spatial pattern will be obtained, while SOM should identify both patterns. We explain this more profusely now.

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).

We have removed the section explaining variations in SST because it is not a forcing. The term wind has been reviewed throughout the document and, particularly in section 4.2, and we specify now whether we refer to wind intensity, etc..

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.

Following the reviewer's suggestion, we have reformatted the results and discussion section into separate sections and we further discuss the presented methodologies.

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?

Bioregionalizations can be based on different variables and generally differ in that they obey different requirements. It is simply not possible to analyze certain environmental issues from regionalizations based on low-resolution information or criteria that do not respond to the object of the study. This is particularly true in the case of the study of pelagic organisms where the biome extent changes over time. The first regionalizations proposed by Alan Longhurst (1995 and 1998) obeyed geographic divisions based on the average distribution of phytoplankton biomass and primary production and provided a coarse but useful division of the global ocean. In subsequent analyses, other components of the trophic chains were incorporated. In specific cases such as HNLC, the biome presents highly dynamic boundaries and it becomes necessary to resort to techniques that incorporate this variability.

Self-Organizing Maps (SOM) and k-means are both unsupervised machine learning algorithms, but they have different characteristics and are used for different purposes. K-means is a clustering algorithm that groups similar data points together based on spherical clusters, while SOM finds the best matching unit (BMU) for each data point, which is the node in the grid that is most similar to the data point. Also, SOMs can handle non-linearly separable data.

One of the advantages of the SOM over k-means is that it performs the groupings based on the shape of the time series, which provides very coherent regions with similar dynamics, which allows each subregion to be characterized later based on robust statistics.

The SOM non-linear mapping has also advantages over linear methodologies, like principal component analysis, empirical orthogonal functions (EOF), and even K-means. If the data distribution on a two-dimensional space has a correlation close to zero, this can be difficult to find it using PCA or similar, however, by using SOM, the resulting weights will be adjusted in such a way as to match the shape of the data distribution. K-means is a special case of SOPM in which the neighborhood function is zero (not considered).

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

We do not run a nutrient model. Results are from the numerical simulation PISCES produced at Mercator-Ocean (https://www.pisces-community.org/ https://www.pisces-community.org/index.php/model-description/). It runs over NEMO hydrodynamic model, a primitive equations model https://www.nemo-ocean.eu/ and therefore, it does consider MLD. However, we did compare model results with available data globally and regionally and we provide now the correlations between model data and regional data for global and regional data. We also include now in the supplementary information the mean NO3 profile in each region depicted from in situ data,

The Mixed Layer Depth (MLD) is one of the main oceanographic indicators that can be used for the 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.

Yes, it considers the MLD. Answered above

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.

The reviewer makes a good point here. The GlobColour level-3 mapped products have a resolution of 1/24°, 0.25° or 1.0° (i.e. respectively around 4.63 km, 28 km, and 111 km at the equator) for global products. They consist of the flux-conserving resampling of the global level-3 binned products. The geographical location and extent of each bin is determined by the so-called Integerized Sinusoidal (ISIN) grid. In particular, we use data from the rectangular regular map product provided by GOBCOLOUR at global scale, with a regular grid in degrees, with a spatial resolution of 0.25 degrees, 27.82km at the equator that varies with the latitude. The spatial extent was given in pixels and it has been changed to 'real' areas in km2 in this new version. Data are not interpolated since they are structured in a regular grid.

As a result of these changes, small variations are observed in the mean climatology of the area anomaly for the three HNLC regions. Intra and interannual area variations in EEP and SNP regions are slightly larger when computing the area based on the linear dimension (i.e. km^2) than on the number of pixels.

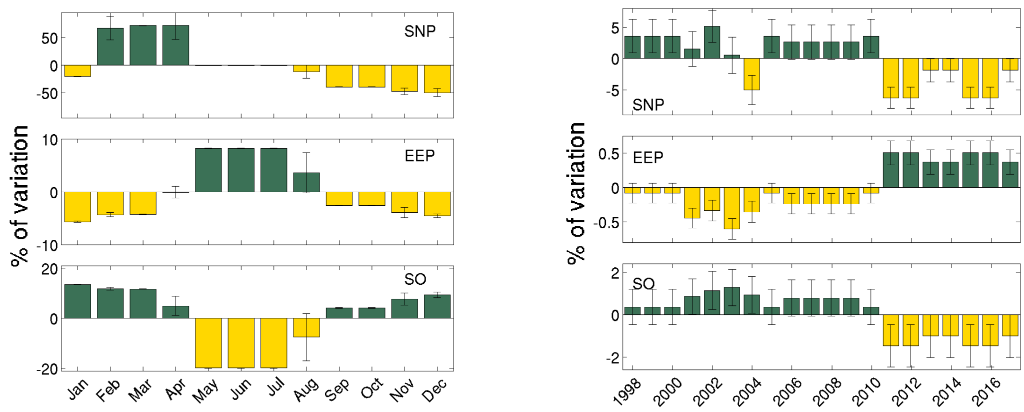

Figure R1. Seasonal (left) and interannual variations (right) in the spatial extension of the three HNLC regions. Variations are referred to the mean extension of each region. The blue dashed lines indicate the regime shift occurring after 2010.

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.

We now explain better matching between model and in situ values in each HNLC region. We also include nutrient profiles in the supplementary information.

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?

The SOM algorithm is composed of two main steps: initialization and training. In the initialization, the architecture of the neural network used in this study is set in a sheet hexagonal map lattice of neurons, or units, in order to have equidistant neurons (to avoid anisotropy artifacts). Each unit is represented by a weight vector with a number of components equal to the dimension of the input data, i. e. number of rows or number of columns in the Chl and NO3 matrices, depending on whether the analysis is performed in the temporal or in the spatial domain. We use an initial network composed of units of random values (random initialization). In the training process, the initial neural network is transformed by iteratively presenting the input data. In each successive iteration the neuron, or unit, with the greatest similarity (excited neuron), called Best Matching Unit (BMU) is updated by replacing their values with the Chl and NO3 values of the input sample data. The similarity is estimated by computing the Euclidean distance between the components of the input sample and the components of the weight vector of the unit. The unit most similar to the input sample is the one with the minimum distance. In the learning process, Chl and NO3 values of the neighboring neurons of the excited neuron are also updated replacing their values with values determined by a neighborhood function. In this way, the topological neighbors of the BMU are also updated through the neighborhood function. In this study, we use the imputation batch training algorithm (Vatanen et al., 2015) and a Gaussian neighboring function. We do not find important differences between linear and random initialization. However, linear initialization is more computationally expensive.

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?

The size of the neural network (number of neurons) depends on the number of samples and on the complexity of the patterns and an optimal choice is important to maximize the quality of the SOM. In the present study, the map size is set to be [4 x 3] with 12 neurons for the time domain analysis, and a [3 x 3] neural network is used in the spatial domain. Using larger map sizes, the patterns are slightly more detailed, and more regions of a particular variability emerge, but the occurrence of the probability of the patterns decreases, without affecting the results noticeably (Basterretxea et al., 2018; Hernandez-Carrasco and Orfila, 2018). If a reduced neural map, such as [2 x 2] is used, patterns are concentrated together with the occurrence probability in a few rough patterns but increasing, in this case, the topological error.

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

This has been corrected.

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.

Figure 6 has been removed following a reviewers suggestion

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

We indicate the shift with a blue dashed line in the figure.

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.

We have improved the figure including mean spectral peaks and further explanation is provided.

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.

We have segregated results from discussion and a more detailed discussion on teleconnections is now provided. This includes ENSO associated effects. However, the analysis of particular factors, such as rainfall, is beyond the scope of the present study.