RC1: 'Comment on egusphere-2022-827', Anonymous Referee #1, 25 Oct 2022 reply
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.
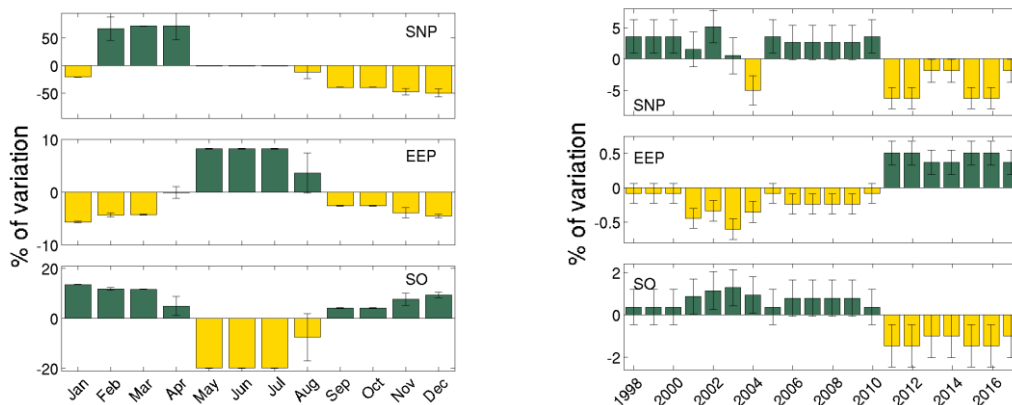

Figure R1. Seasonal (left) and interannual variations (right) in the spatial extension of the three HNLC regions. Variations are referred to the mean extension of each region. The blue dashed lines indicate the regime shift occurring after 2010.

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.
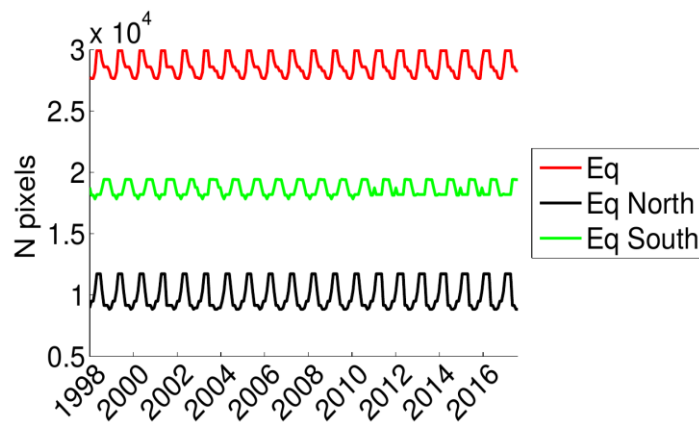
Fig R2. Time evolution of the total number of pixels covering each area: Whole equatorial region (in red). Pixels in the equatorial region which is located in the northern hemisphere are shown in black and those in the southern hemisphere in green. Note that while the mean value in the Southern hemisphere is larger, the northern region is more variable.
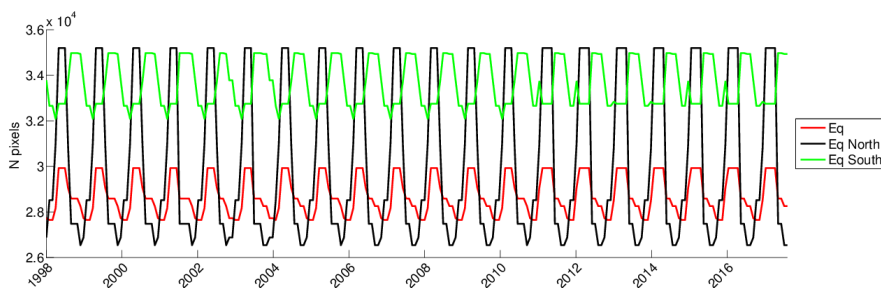
Fig R3. Time evolution of the total number of pixels covering each area scaled to the same range of values for better comparison: Whole equatorial (in red). Pixels in the equatorial region which is located in the northern hemisphere are shown in black and those in the southern hemisphere in green.

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).
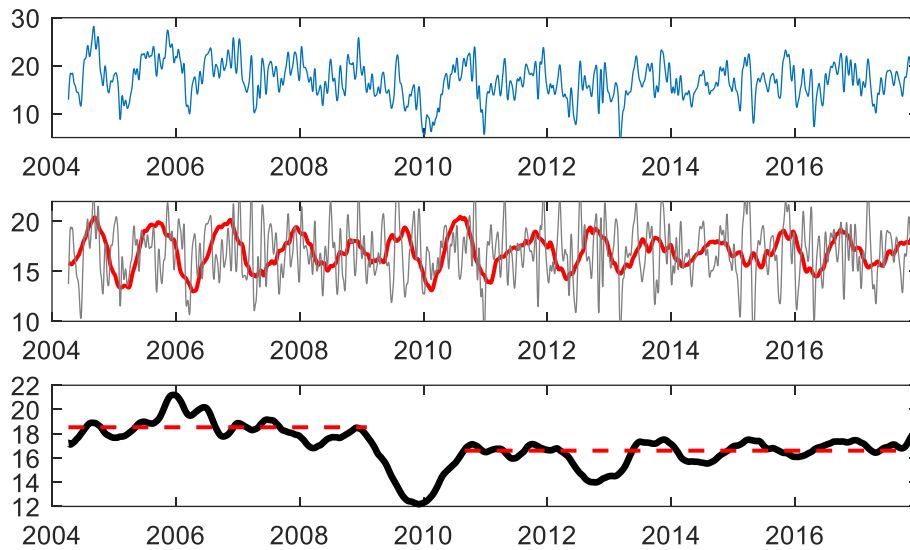

Fig. R4 Decomoposition of MOI into (1) seasonal, (2) irregular, and (3) interannual trends.

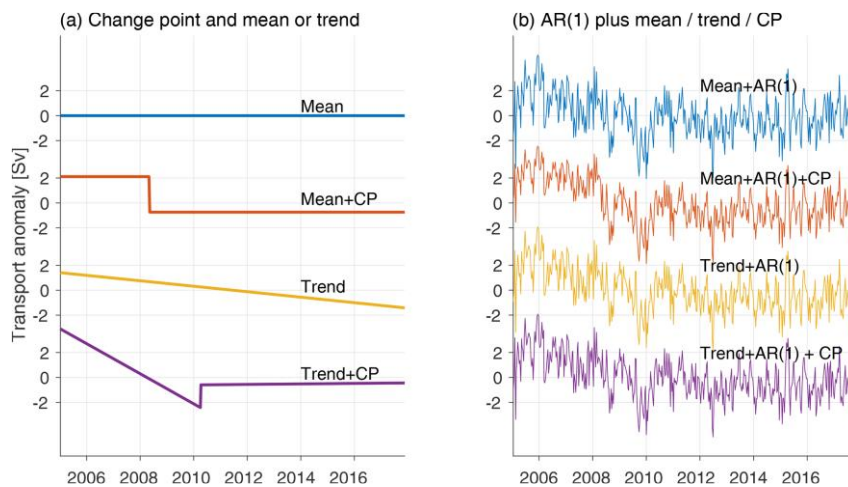Figure R5. Change-point analysis of the AMOC–Ekman time series. (Moat et al. 2020).

Moat, B. I., Smeed, D. A., Frajka-Williams, E., Desbruyères, D. G., Beaulieu, C., Johns, W. E., Rayner, D., Sanchez-Franks, A., Baringer, M. O., Volkov, D., Jackson, L. C., and Bryden, H. L.: Pending recovery in the strength of the meridional overturning circulation at 26° N, Ocean Sci., 16, 863–874, https://doi.org/10.5194/os-16-863-2020, 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. Also, the seaways in the Pacific are more complex, and determining the overturning signature is more challenging. Some studies have suggested that temperature anomalies subducted into the pycnocline at subtropical latitudes may not reach the Equator with any appreciable amplitude (Schneider et al., 1999a). However, mass water balances in the equatorial pacific reveal that the strength of the equatorial upwelling is related to variations in the PMOC (McPhaden and Zhang, 2002) and, therefore, an influence on EEP extent is expected.

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. 20201). 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.
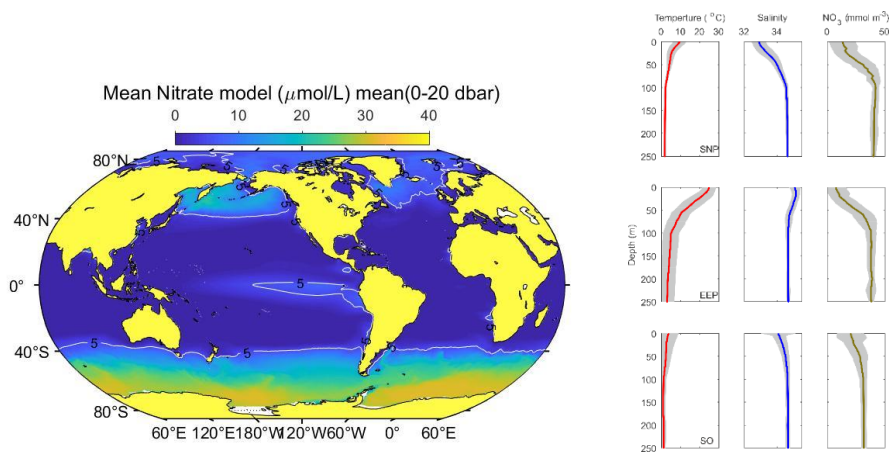

Fig. R3. Mean global nitrate concentration and vertical profiles for each region.

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.