



EGUsphere, referee comment RC1
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Comment on egusphere-2022-827

Anonymous Referee #1

Referee comment on "Global variability of high-nutrient low-chlorophyll regions using neural networks and wavelet coherence analysis" by Gotzon Basterretxea et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-827-RC1>, 2022

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 NO_3/Chl , and identifying new linkages between HNLC extent and some climate variability indices.

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 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, 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 actually demonstrates. There are also passages in the Methods that I think more properly belong in the Discussion (e.g., 124-129).

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.

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 though 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.

In Figure 7 we see an abrupt decline in the MOC around 2010 and then recovery to a level around 17 Sv that 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? 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.

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 NO_3/Chl with the climate indices, but the discussion of the underlying physical processes is quite speculative.)

The model-data comparison for NO_3 could be expanded on a bit, e.g., "we found good agreement between nitrate *in situ* data and model results ($r^2=0.98$)" (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 NO_3 , 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?

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)

53 "oligoelements" unnecessary jargon

60 "coarsely known aspects" not clear what this means

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

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)

89 change "nutrient outputs" to "nutrient concentrations"

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

131 change "climatological indices" to "climate indices"

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

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

225 delete "i.e."

231 delete "values"

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

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

246 change "corresponding" to "correspond"

252 change "therein" to "there"

260 add a comma after "ratios"

262 change "ice sheet" to "sea ice"

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

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

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

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

here

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

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.

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

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

325 change "thought" to "though"

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.

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"?

349 change "in" to "on"

353 delete "value"

353 change "phytoplankton uptake" to "phytoplankton biomass"

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)

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

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

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

415 add a comma after "AMOC"

416 change "more unclear" to "less clear"

417 sea ice or glacier ice?

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

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

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

445 delete "through complex processes"

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

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

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

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.)

There are numerous references 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.