



EGUsphere, referee comment RC2
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Comment on egusphere-2022-17

Anonymous Referee #2

Referee comment on "Ecosystem impacts of marine heat waves in the northeast Pacific" by Abigale M. Wyatt et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-17-RC2>, 2022

I think this is a good paper that is publishable with more or less minor revisions. Some aspects of the methodology are insufficiently explained. The terminology is confusing in some places, and some unnecessary jargon is used (see details below).

Major points:

1) There are some important details missing from the description of the methods and the data. Most importantly, a marine heatwave is defined as "anomalies that exceed 1 standard deviation for 5 months or more". But standard deviation of what and anomalies relative to what? The obvious answer is relative to a climatology calculated over the period of the ERSSTv4 data product, but that needs to be stated explicitly, and which years of this data product were used does not appear to be stated anywhere. And is there an area threshold? Is the criterion applied point-by-point, or only to the regional mean? Would it be a heat wave if only 1 grid point exceeded the threshold? (And why does the "MOM6-COBALT climatology" in Figure 10 appear to have interannual variability?)

"The model was spun-up using three repetitions of ... 1958 to 1985" (112). But spun up from what? From rest? 81 years doesn't seem very long to spin up a global ocean model. And why go to the trouble of initializing short-lived (i.e., insensitive to initial conditions) biological tracers from an ESM piControl (116-117), but not the physical ocean? I think it would make more sense to use the ESM piControl data to initialize the physical ocean, or 1958 of the historical run.

I find it difficult to believe that there are no Line P chlorophyll data before 2011 (175-176). Line P is one of the longest-running ocean time series programs, and the basic methodology for chlorophyll concentration has not changed in half a century. Satellite chlorophyll data should be available back to 1996 or 1997. "GlobColour" is referred to several times in the figure captions but never in the main text.

Are these really all of the Argo floats available in this region? Or is there some other selection criterion being applied that is not spelled out here (e.g., availability of nitrate data or data within a certain area)? I find it hard to believe that these are the only Argo floats deployed in this region over an 11 year period.

2) The Abstract ends by saying that "primary production anomalies modify the allometric phytoplankton distribution, resulting in a 2 % decrease in the ratio of large to small phytoplankton in both regions". Firstly, this seems like a very small change to emphasize as a key point in the Abstract: I am wondering if it is a mistake and it should be 2X or 20%. Secondly, it isn't easy to tell whether this passage is talking about production or biomass, and seems to shift arbitrarily between the two. Finally, where exactly in the main text is this assertion substantiated? Figures 7 and 8 illustrate the seasonal decoupling of large and small phytoplankton production, but can not be used to directly infer the Large/Small ratio of either biomass or production. Figure 9 shows only summer data. (BTW "allometric phytoplankton distribution" here is a good example of unnecessary jargon: "phytoplankton size distribution" would suffice. And if one wishes to get dogmatic, the anomalies do not "modify" the size distribution. This sort of quasi-teleological confusion of subject and object is characteristic of inexperienced authors receiving inadequate guidance (see also 208, "Salinity maintains ...").

3) The interaction of the N and Fe cycles is sometime characterized in superficial terms, although I think the overall conclusions are mostly sound. It might help to spend a few sentences in the Introduction sketching out a conceptual model of how the authors think the overall system works.

On 164-165, would not a prolonged period of stratification also result in depletion of surface iron concentrations? In the absence of significant aeolian sources I think it would. However, it would also tend to drive the system towards N limitation even in the absence of new aeolian Fe. It also seems to be implied that only large phytoplankton are subject to iron limitation (130-135), which I think is questionable. Iron is potentially limiting for nanophytoplankton even if iron limitation is the main driver of the dominance of diatoms or nanophytoplankton. On 268 it is stated that "small phytoplankton are not simulated with iron limitation" so possibly the lack of Fe limitation is by construction in this model. If this is the case it should be stated up front in the Methods.

The limitation factors are never really explained. I assume this means a number between 0 and 1 where 1 means N or Fe replete and 0 means no growth, but this should be clearly stated in the Methods. (On a terminological note, I think "nitrate limited" and "nitrate limitation" should be changed to "nitrogen" across the board.)

In the last paragraph of section 3.2, the terminology is sometimes vague or confusing, wrt what is meant by a "boundary". On 223, the "2 uM nitrate boundary" could be "2 uM nitrate contour". In the next sentence, "nitrate boundary" occurs without any context. I assume this means the boundary between regions of N and Fe limitation, but it could be

spelled out more clearly. This is an example of a place where adding a few more words could increase clarity substantially. The last few sentences (226-229) read like a description of the model solution, and this seems like a missed opportunity to state what the authors think is happening in terms of physical processes (see also 339-343).

Some details:

10 and elsewhere I would change "Alaskan gyre" to "Alaska gyre" across the board

15 change "limitations" to "limitation"

17 delete "climatologically" or change it to e.g., "usually" or "chronically"

18 "Contrastingly, we find that ..." conversely? in contrast? by contrast?

19 maybe change "lower light limitation" to "higher mean irradiance"

20 change "allometric phytoplankton distribution" to "phytoplankton size distribution"

26 not sure "recorded" is the appropriate term here; how many of these were recognized as such when they occurred?

31 " a redistribution of marine biogeography " ???

32 delete "geographical"

35, 37 "Chlrophyll"

36 change "demarks" to "demarcates"

37 delete "Pacific"

38 change "nitrate surface concentrations" to "surface nitrate concentrations"

48-50 this sentence is very awkwardly worded

57 change "Ekman-driven transport" to "Ekman transport" or "Ekman flow driven transport"

60-61 I would consider also citing Glover et al 1994 (10.1029/93JC02144) here (Bograd et al appears to be missing from the ref list)

67, 387 change "contrasted" to "contrasting"

127 delete "re-"

132-133 delete "and are efficient ... Geider et al., 1997)"

158, 160 mmol kg⁻¹ should be umol

159 add a ' on "floats"

171-172 "nitrate concentrations are near-zero for most stations (P4-P20)" Is this unusual? Don't some of these stations always see drawdown in summer? (e.g., Pena and Varela 2007).

186 add "North" before "American"

191 change "biophysical" to "biogeochemical"

205 change "values" to "concentrations"

207 "> 5 mg m⁻³" Is this a mistake? This is an extremely high concentration for an open-ocean environment.

211-214 this assertion seems disconnected from the preceding text; not clear what its relevance is

217 add a "~" before "130 W"?

219 "nitrate becomes more depleted" more than what? (unclear antecedent)

250 not clear what is meant by "in this region of the model"

267 "the limitation factor is significantly lower (-0.06)" significant by what criterion?
P<what?

285-287 "Lg Chl" and "Sm Chl" appear only in this one place, as does "chl" (elsewhere Chl)

286-296 "southern-like" and "northern-like" appear only in this paragraph and are not defined or explained

306 specify mmol of C or N

311-313 another very awkwardly worded sentence

333-336 Does this sentence make sense? It reads like it is sort of arbitrarily combining different levels of causation. If there is a clear hypothesis as to "A leads to B leads to C", it would be better to express it that way.

350 add "concentration" after "nitrate"

360 "changes sampled along the floats" along the floats' trajectories?

364 change "this data" to "these data"

398 comma in wrong place

416-417 I'm not sure this sort of editorializing is necessary, and I doubt that it is discussed by Frölicher and Laufkötter. As for the following sentence (418-420), the intended meaning is fairly clear but the wording could be improved.

Figure 7d, 8d unit should be nM?

Figure 9 unit needs a space between mg and m⁻³

Figure 9 caption: There are a bunch of details about this Figure that are not really explained in the caption: the meaning of the vertical bars (probably mean, but needs to be stated, and panel (b) is different from the other 3), the vertical position of the symbols (arbitrary, but again should be stated), and the meaning of the symbol colours (obvious from the positions, but in this case is having two colours even necessary?) And there appear to be more years than there are symbols.