EGUsphere, referee comment RC1
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Comment on egusphere-2022-17

Anonymous Referee #1

Referee comment on "Ecosystem impacts of marine heat waves in the northeast Pacific" by Abigale M. Wyatt et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-17-RC1, 2022

This paper focuses on quantifying and explaining ecosystem impacts of marine heat waves in the NE Pacific, using a modelling approach supported by observational comparisons. The focus is on anomalies in chlorophyll and phytoplankton and their drivers. The topic is of high interest to the community. Several recent papers and theses look at the impacts of the 2014-15 Blob on productivity rates, ecosystem assemblages, and biogeochemical measurements like trace metals. This study broadens that work to consider a wider spatial area, assess drivers quantitatively, and provide some context for the results of previous work like decreasing nitrate observed by BGC-Argo floats. However, in many places in the paper, I was left wondering about whether the small changes detected were significant. Including statistical tests for significance would strengthen the paper, making the overall message more convincing. In general, the paper is well written, but I do have a few additional comments that I think could further improve it.

This paper reports many anomalies for marine heat waves based mostly on model results. Some of the anomalies are very small relative to the absolute concentrations or rates. Which ones are significantly different from zero? The paper would be strengthened by including a statistical test for significance in each case where a change associated with marine heat waves is reported, providing clarity for which changes are significant and which should be reported as no change within error. Examples include (but are not limited to):

- 2% lower large phytoplankton population in the AG
- 2% decrease in large:small phytoplankton in both regions
- 05 mg m-3 decline in chlorophyll in the NPTZ and 0.02 mg m-3 increase in chlorophyll in the AG – The AG value is especially small compared to mean chlorophyll in this location.Is the mean for MHW years significantly different from the mean for other years, given the fairly high variability in this region?
- The location of the 2 uM nitrate contour in Figure 5b.Given the variability in the location of this contour in the 9 MHW events, is the mean significantly different from the all-

year average?

- Mean-MHW values throughout sections 3.3 and 3.4.With 9 MHW years and many non-MHW years in the model, it should be straightforward to calculate whether the MHW years are significantly different (perhaps a Mann-Whitney test or similar) in different months.
- 13% lower large phytoplankton production

Boundaries of nitrate and iron limitation are discussed, but I'm unsure of how these are defined. Nitrate limitation may be defined by the 2 uM contour line, though this should be explicitly stated. I'm not sure what iron value would be considered limiting here. In general, the iron concentrations in the model (Figure S2) seem very high with modelled values around 100-150 nM iron in winter, whereas typical values for dissolved Fe in the region appear to be < 1 nM (see https://doi.org/10.1016/j.marchem.2015.04.004 for example). Is the model iron limiting anywhere? I could not discern the hatching in Fig. 6 discussed in Line 222 or the gray and purple lines discussed in Lines 225-226. How are the limitation factors shown in Figure 7h defined? This section, along with that around Line 58, also caused me to wonder about the role of light limitation. Is the NPTZ actually iron limited before the spring bloom or is it light limited then? Is there grazing limitation that is important to controlling the size of the spring bloom in the NPTZ?

Minor suggestions:

Line 92: I was very surprised to read that chlorophyll data was not available for 2008-2010 from Line P. I contacted chief scientist, Marie Robert, to ask. She looked into it and has found that the data exists but that there is a problem with some of the summary .csv files. Some individual casts seem to contain the data but the whole cruise files do not. She is working on updating the files. I suggest you contact her directly for updates: Marie.Robert@dfo-mpo.gc.ca

Section 2.5: Suggest BGC-Argo rather than bioArgo. Suggest referencing Appendix A here.

Line 175: Fig. X

Lines 203-214: Suggest mentioning in this section that the high nutrient regime near the coast is temporally variable and mainly controlled by the timing of upwelling events.

Figure 1: I'm unclear why the bounding box for the average anomalies shown in panels a and b is different from either box shown on the maps. The targeted area appears to be mainly in the NPTZ. I suggest that it would be more illuminating to show the time series for the black NPTZ box in panels a and b rather than a different region that is not used in further analysis. Panel b: The region for this anomaly is probably the same as for panel a,

but it would be good to state that. I think the location of OSP should be 50oN 145oW, not 50.1oN 149.9oW.

Figure 2: the colours of the float trajectories in the upper panels should match those in the lower time period panel. In particular, the brown colour in the upper panels is orange in the lower panel. Colour bar label is cutoff for panel b.

Figure 3: Suggest adding property labels to the colour bars, i.e. not just units. Also for the y-axis of Figure 10.

Figure 4: Colour bar labels are cut-off. Figure 5: Colour bar labels and legend are cut-off.