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Comment on egusphere-2022-451

Anonymous Referee #2

Referee comment on "Hotspots and drivers of compound marine heatwaves and low net primary production extremes" by Natacha Le Grix et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-451-RC2, 2022

Review of "Hotspots and drivers of compound marine heatwave and low net primary production extremes" by N. Le Grix et al.

This study aims at (1) evaluating the incidence of compound marine extreme events (high SST, low NPP) in two climate models and (2) identifying the drivers of these in these models, an impact task to be able to confidently project their changes in the future with these models. This study is interesting, very well written and address a very relevant scientific question within the journal's scope. I have a couple of major concerns that need to be addressed before being able to recommend its publication.

My major concerns are the following:

- The role of ocean dynamics: if I understand it well, the authors derive the role of advective process from the difference between phytoplankton change from the beginning to the maximum of an NPPX event and the integral of (NPP – Loss) over the same period. While it appears sounded, I hardly believe the results displayed on Figure 5 and 6, where ocean dynamical processes almost systematically counteracts the impact of (NPP – Loss) for both models and in most regions. I suspect that there is a mistake in the conception or implementation of the method here. The only explanation the authors provide to explain this systematic behaviour is that the increased stratification reduces downward mixing and prevent the export of phytoplankton out of

the top 100m. While this may be true in a limited number of cases, it is known that chlorophyll survives only in the euphotic layers and that a very large majority of the Chl concentration lies within the first 100m. Below these depths, Chl dies and this effect would be accounted in the Loss term rather in the ocean dynamics. Looking at Figure 5, it is obvious that the effect of (NPP-Loss) and dynamics clearly mirror each other, which I find very doubtful. This is particularly the case in the central/eastern equatorial Pacific, where dynamics act to systematically oppose the effect of (NPP-Loss). These events generally develop during El Nino events, during which equatorial divergence weakens and hence dynamics is supposed to systemically contribute to increase Chl biomass. It is clearly not the case for none of the models and Phytoplankton group as shown on Figure 6e,f. I also have a hard time to understand why the impact of circulation can be that different between small and large phytoplankton group, especially for ESM2M-LE model, where circulation changes are the same for both groups and Chl climatological distribution share the same patterns for these group (Figure B5i-l). These simple considerations led me to suspect some caveats in the computation in the dynamical contribution. I strongly recommend the authors to double check there method and, if they are convinced there is no mistake, discuss in more details the reasons that could lead to a systematic offset of (NPP-Loss) effect by the dynamics in the core of the result section but also in the conclusion, where this fact is never discussed.

- The role of light limitation: From Figure 4, the authors argue that light limitation is a major factor contributing to the growth rate anomalies in a lot of places, but they never provide a convincing explanation on how MHW could drive a change in light limitation. It is very surprizing to see that the effect of light limitation are almost opposite to each other between the two models (Fig. 4ij compared to Figure 4kl). The authors hypothesize that this divergence may arise from different nutrient limitations, chlorophyll to carbon ratios and light harvest coefficients but it is very difficult to believe given the very similar formulation of L(lim) in the two models. I would have intuitively argued that changes in Irradiance would have played a major role. I would like the authors to further explore and discuss the mechanisms behind this very surprising and inconsistent role of light limitation in the revised manuscript.

Aside these two major points, find below a couple of minor comments that could improve the readability of the paper:

L161: These models not only differ in their ocean biogeochemical compartment but also in the physical ocean and atmosphere used, which could also explain some of the differences between the two models.

Figure 3: Different colorbars could be used for the two models to avoid saturation for CESM2-LE model (panels c,d,k,l)

L385-389: Why not relating it to Irradiance changes?

L366: Provide more physical interpretation behind the increased light limitation.

Figure 5: Same as Figure 3. Use a different colorbar for the two models to avoid saturating colors for CESM2

L411-418: This is the only place where the authors discuss the counteracting effect of dynamics over (NPP-Loss) and I don't find their explanation very convincing. If there is no mistake in the calculation, the authors definitely need to explain this systematic behaviour in a more convincing way…