Second round of review of “Hotspots and drivers of compound marine heatwave and low net primary production extremes” by N. Le Grix et al.

The authors did a nice work to account for most of my major concerns in initial evaluation of the paper. There are however two changes that I could like the authors to consider (and some very minor comments) before the paper can be definitely suitable for publication.

My first major comment relates again to their decomposition procedure. The authors indeed writes L295: $dn_i \sim \Delta n_i$. If I understand it well:

- $dn_i$ refers to mean biomass anomaly between $t_0$ (climatological value) and $t_{\text{max}}$ (maximum absolute anomaly relative to climatology)
- $\Delta n_i$ refers to the integrated biomass change between $t_0$ and $t_{\text{max}}$

Given the formula, $\Delta n_i$ is thus simply the difference in biomass between $t_0$ and $t_{\text{max}}$ (i.e. $n_i(t_{\text{max}}) - n_i(t_0)$), while $dn_i$ is the biomass anomaly averaged between $t_0$ and $t_{\text{max}}$ (i.e. $\Sigma dn_i$). By construction, $\Delta n_i$ should therefore be systematically larger than $dn_i$ (about twice larger), which indeed appears to be the case on Figure 5.

By definition,

$$dn_i = (dn_i(t_0) + dn_i(t_1) + dn_i(t_2) + \ldots + dn_i(t_{\text{max}}))/N,$$  

$N$ being the number of 5-days output timesteps between $t_0$ and $t_{\text{max}}$

Thus:

$dn_i = (\partial n_i(t_0) \ast \Delta t + (\partial n_i(t_0) + \partial n_i(t_1)) \ast \Delta t + (\partial n_i(t_0) + \partial n_i(t_1) + \partial n_i(t_2)) \ast \Delta t + \ldots + (\Sigma \partial n_i(t_0 \rightarrow t_{\text{max}}) \ast \Delta t))/N$

$dn_i = ((N \ast \partial n_i(t_0) + (N - 1) \ast \partial n_i(t_1) + (N - 2) \ast \partial n_i(t_2) + \ldots + 1 \ast \partial n_i(t_{\text{max}})) \ast \Delta t)/N$

As you can see, this calculation is clearly different from $\Sigma \partial n_i(t_0 \rightarrow t_{\text{max}}) \ast \Delta t$, which corresponds to your definition of $dn_i$.

I would either recommend calculating:

$$\Delta n_i = (\partial n_i(t_0) \ast \Delta t + (\partial n_i(t_0) + \partial n_i(t_1)) \ast \Delta t + (\partial n_i(t_0) + \partial n_i(t_1) + \partial n_i(t_2)) \ast \Delta t + \ldots + (\Sigma \partial n_i(t_0 \rightarrow t_{\text{max}}) \ast \Delta t))/N \text{ (and the corresponding contributions)}$$

Or redefine $dn_i$ as $n_i(t_{\text{max}}) - n_i(t_0)$ and compare it to $\Delta n_i$ as defined in the manuscript.

There will otherwise by a mathematical inconsistency and I suspect that proceeding either ways will end up in a closer match between $\Delta n_i$ and $dn_i$ in the paper.

My second comment relates to the fact that the authors do not discuss anywhere the fact that their biological contribution systematically exceeds the integrated biomass changes, i.e. that the residual (that the authors previously attributed to ocean dynamics)
systemically opposes the biological contribution. I would recommend the authors adding a small paragraph in the discussion section where they could provide hypothesis to explain this behaviour (that I still don’t really understand).

Minor comments:

L553: “(see section 2.5,” : a closing parathesis is missing.

L553: “resulting in in”: remove one “in”