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 intial 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 comments 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_{max} (maximum absolute anomaly relative to climatology)
- Δn_i refers to the integrated biomass change between t_0 and t_{max}

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

By definition,

$dni = (dni(t_0) + dni(t_1) + dni(t_2) + \dots + dni(t_{max}))/N$, N being the number of 5-days output timesteps between t_0 and t_{max}

Thus:

$dn_i = (\partial_t n_i(t_0)* \Delta t+(\partial_t n_i(t_0)+\partial_t n_i(t_1))*\Delta t+(\partial_t n_i(t_0)+\partial_t n_i(t_1)+\partial_t n_i(t_2))*\Delta t+ \dots+(\Sigma\partial_t n_i(t_0->t_{max})* \Delta t))/N$

$dn_i =((N*\partial_t n_i(t_0)+(N-1)* \partial_t n_i(t_1)+(N-2)* \partial_t n_i(t_2)+\dots+ 1*\partial_t n_i(t_{max}))* \Delta t)/N$

As you can see, this calculation is clearly different from $\Sigma\partial_t n_i(t_0->t_{max})* \Delta t$, which corresponds to your definition of dn_i

I would either recommand calculating:

$\Delta n_i = (\partial_t n_i(t_0)* \Delta t+(\partial_t n_i(t_0)+\partial_t n_i(t_1))*\Delta t+(\partial_t n_i(t_0)+\partial_t n_i(t_1)+\partial_t n_i(t_2))*\Delta t+ \dots+(\Sigma\partial_t n_i(t_0->t_{max})* \Delta t))/N$ (and the corresponding contributions) and compare it to the dn_i as defined in the manuscript

Or redefine dn_i as $n_i(t_{max}) - n_i(t_0)$ and compare it to Δn_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 Δn_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"