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Interactive comment on "Zooplankton mortality effects on the plankton community of the Northern Humboldt Current System: Sensitivity of a regional biogeochemical model" *by* Mariana Hill Cruz et al.

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Comment:

General Comments

The manuscript describes the responses of the plankton ecosystem in the Eastern Tropical South Pacific to different scenarios of small pelagic fish abundance using a coupled physical-biogeochemical model in a regional configuration. Changes in fish predation are simulated by changing mortality rate of zooplankton compartments in

the biogeochemical model. This simple method provides an insight of the ecosystem's response to fluctuations in small pelagic fish biomass.

The manuscript addresses relevant scientific questions within the scope of the journal. The study is based on a previous work (Getzlaff and Oschlies, 2017) carried out on a global scale, using a different model.

The overall presentation is well structured and clear.

The results are discussed in an appropriate and balanced way.

Substantial conclusions are reached but need to be confirmed with an end-to-end model.

The title clearly reflects the contents of the paper.

The abstract provide a concise and complete summary.

The amount and quality of supplementary material is appropriate.

This study has 2 weaknesses:

1/ the evaluation of the plankton compartments is poor. There is little data and the comparison is not convincing. However, the difficulty of comparing model and observations is well discussed. Are there no more in the area?

2/ this study would have deserved some prior improvements: DVM implementation and a tuning of the model. However these two points are mentioned as weaknesses in the discussion.

Response:

Dear referee,

The authors thank you for your valuable comments to improve the analysis and quality of the paper.

We agree of the importance of including more data in the comparison. Fig. 5 shows a comparison between the model (right) and the Eastern Tropical South Pacific region of the global mesozooplankton dataset by Moriarty and O'Brien (2013); O'Brien and Moriarty (2012) (left). We transformed the observation values provided in the dataset to nitrogen units assuming a carbon to nitrogen ratio by weight of 4.9 (Kiørboe, 2013) and a nitrogen molar mass of 14 g/mol. Both model and observations are averaged over the whole year and over the upper 100 m depth. Model values are generally higher than observations. However, please note that the observations are sparse and in many cases there is only one data point available for the whole water column. Therefore, the averages may not be representative of the whole water column. We can include this figure in the appendix of the paper if recommended. Because the available data is from heterogenous sources, and often on a coarse spatial grid, it requires a very careful analysis for comparison with our finely resolved model. A more careful and in depth validation, including tuning of the model will be presented elsewhere.

We note that in the analysis of scenarios A, which serve as complement for experiments B, there was a mistake in the weighting of the time steps when calculating the annual average of the concentrations. This has now been corrected and affects slightly Fig. 4 and Fig. D1 of the paper (see Figures 1 and 2 in this response). For Fig. 4 in the paper we now only present the surface concentrations of organic compartments, to follow your suggestion further down (presented here as Figure 3). These changes do not change any of our conclusions.

Comment:

Specific comments

The reference study of Getzlaff and Oschlies (2017) is based on a simulation that has been running for 300 years. It shows that the Tropics are really long to reach a balance and that the difference between an experiment (high, low scenario) and the reference can changes sign between the first decades of simulation and the rest

(Getzlaff and Oschlies, 2017; see Figures 2, 3). So what is the strategy justifying a 30 year climatological simulation ? What are the reasons for this choice? Does the model reach a state of equilibrium ? Please provide a figure with the time evolution of the main biomasses and fluxes, as in Getzlaff and Oschlies (2017).

Response:

By using a higher resolution model, we can compare the effect of the same strategy at a regional scale with higher physical complexity that is not addressed by the global model. The compromise is that the high resolution model is more computationally demanding and it is technically impossible to run it for 300 years.

Currently the high temporal resolution simulation results are not available for all experiments. Therefore, we cannot provide a figure as in Getzlaff and Oschlies (2017) study. However, we have provided the averaged concentrations over the final 10 years of the simulation, as well as the interannual changes (percentage change between every point in time and one year later, Figure 4). As it can be seen in the figure, there is no major trend in increase or decrease of biomass for any of the plankton groups.

Furthermore, the 300 years time series in Getzlaff and Oschlies (2017) model starts after a spin up time of 10 000 years with the parameters of the reference scenario. Therefore, the 300 years run in their model reflects the re-stabilization of the model from the reference scenario conditions to the ecosystem conditions with a shifted zooplankton mortality. On the other hand, we spun up the model with the already changed mortality.

Comment:

I wonder about the relevance of these results in an end-to-end ecosystem. In the high scenario, the flow of energy and matter return immediately to the detritus pool and feed the microbial loop instead of being transferred to higher trophic levels and take longer time to return to the microbial loop. Won't this difference affect the conclusions of this study? This point deserves to be discussed.

Response:

In an equilibrium state this would not be a problem since there is a constant turnover of nutrients. In a non-equilibrium state, the time that nutrients spend as part of larger animals biomass would further the gap between nutrients consumption by phytoplankton and their replenishment affecting phytoplankton growth rate and potentially the blooming timing. However, this should not be a problem in the coastal upwelling region because nutrients are highly concentrated here. This is not the case for the oligotrophic region although in our study, this region presents the weakest response. Furthermore, small pelagic fish concentrate mainly in the highly productive upwelling region rather than in the oligotrophic waters offshore. On the other hand, fish and larger animals are highly mobile and are not constantly drifted by advection as nutrients and plankton do. Therefore, migrations transport nutrients and organic matter in and out of the region in a horizontal (McInturf et al., 2019; Varpe and Fiksen, 2005; Williams et al., 2018) and vertical fashion (Davison et al., 2013; Lavery et al., 2010). We will point this out in Sect. 4.3.

Furthermore, the inclusion of more higher trophic levels in an ecosystem implies an inherent energy loss per additional trophic step (see Ryther, 1969), hence reducing the total trophic transfer efficiency of the ecosystem. On the other hand, large organisms tend to live longer and they are considered to have a more efficient metabolism (Brown et al., 2004). As a consequence, they store in their bodies more biomass relative to their metabolic losses than smaller organisms over their lifespan. Therefore, the turnover rate of organic matter may ultimately also depend on the number of trophic levels above large zooplankton as well as their size and longevity. These are open questions that might be explored with finely resolved ecosystem models. An end-to-end model for the Northern Humboldt Current System is currently work in progress which may help to elucidate some of these questions. Finally, we have seen in this study with only two size classes of zooplankton that the relative abundances of both

zooplankton and phytoplankton changed in relationship to each other when zooplankton mortality changed. Therefore, in a real ecosystem we might expect shifts in the zooplankton size spectrum with the corresponding changes in the trophic transfer efficiency of the ecosystem. A biogeochemical model with a higher resolution in the plankton groups might then be necessary to further explore such changes.

Comment:

A paragraph is missing in the Introduction to describe plankton groups in the study area : the spatial distribution, the succession from the coast to the open ocean... It is disseminated throughout the paper, but it would be clearer to have it in the Introduction.

Response:

We will include such description in the introduction.

Comment:

This manuscript is based on the Getzlaff and Oschlies (2017) study. This latter should be described in Section 2.4 or the first time you discuss it in the discussion section. I mean: specify the area of the study, a different model, the method, the scenario, a 300 year simulation. We learn the main elements of this study but too late in the text.

Response:

We will add a description of the methods in Getzlaff and Oschlies (2017) study after line 407.

Comment:

L42: please indicate that this calculation is valid excluding any non-linearity.

Response:

We will take care of this.

Comment:

Section 2.3: please modify the title to “Zooplankton comparison” or “zooplankton evaluation”, because we can’t say it is a validation.

Response:

We will change this to "zooplankton evaluation".

Comment:

Section 2.3: The model is compared to data between February 10 to March 3, 2013. Which model data are used for the comparison ? an annual mean for the last year of simulation ? a monthly average ? a daily average ? Please specify.

Response:

We will specify in Sect. 2.3 and in the figure caption that the model data is the average from January to March.

Comment:

Figure 2: the comparison is not really convincing. Why not show the comparison in log transform as in Appendix C ? This would be justified, as biomasses often have a log distribution.

Response:

We wanted to be as fair as possible and not to hide the fact that the model overestimates zooplankton at the surface. However, we do acknowledge that a logarithmic comparison provides valuable information, especially for the deep water where concentrations are low, and this is why we included it in the Appendix.

Comment:

L199: what is the width of this box? Because Figure 2 shows that the zoo maximum is not to the coast but offshore ($\sim$ 50 km offshore). Is this maximum included in the box ?

Response:

The width of the box is about 40 to 50 Km from the coast. We will mention this in the figure caption and in Sect. 2.5. The large phytoplankton peak is included in this region but not the large zooplankton peak. We choose to do it this way because of the high heterogeneity of the upwelling zone and subsequent transition zone were water is transported offshore. We evaluate the development of such spatial succession in Fig. 5 of the paper.

Comment:

Figure 3: please add “in the reference scenario” at the end of the legend.

Response:

We will make the addition.

Comment:

L209: “coastal upwelling” section: do you mean the white line or the coastal blue box in Figure 1 ? if it is the latter, please change to “coastal upwelling region”.

Response:

We will change this to "coastal upwelling region" since it refers to the blue box.

Comment:

L244-250: Is the spatial pattern of modelled plankton realistic? Is this distribution found in observations, described in literature? Is plankton succession from the coast towards the open ocean typical of EBUS ?

Response:

Hutchings (1992) explains spatial successions as a common phenomena in EBUS. We will mention this in the revised manuscript.

Comment:

L255: why are deep large detritus increased in the A_low scenario ?

Response:

This is most likely an artifact from the averaging error described at the beginning of the response. The increase is not present any more in the corrected figure (Figure 1).

Comment:

Section 3 and Figure 4: I wonder what the description of the deep zone (100-1000m) provides because the analysis focuses on the surface layer. I would remove that part. This would simplify Figure 4 and remove the questions about the strong differences in phyto and zoo found at depth, even if this is explained in the text. I think it would simplify the message.

Response:

We intended to provide an overview of the whole model response at the beginning of our results section and that is why Fig. 4 also shows the deep water averages. However, we agree that for the purposes of this paper the deep water boxes are not necessary and they distract the reader. Therefore, we will remove them from Fig. 4 in the paper/Fig. 2 here; and will replace it with Fig. 3 of this response.

Comment:

Figure 5: please specify that 12° S section refers to the white line in Figure 1.

Response:

We will indicate this in the legend of Fig. 5 in the paper.

Comment:

L339-340: The mortality rates estimated for linear assumption are lower than the 0.19 d-1 estimated by Kiørboe (2002) at 25° C, but there are close to the 0.062 d-1 estimated at 5° C in the same study. Why compare to the first estimate and not the second? What

is the temperature in the region ?

Response:

The temperature in the region lies between 16 and 18° C. We will include also the estimate at 5° C in our comparison.

Comment:

L340: “This indicates that the model may not include all potential sources of variability.” This sentence should be changed. Of course the model does not take into account all kinds of variability, this must be mentioned, but variability difference between the model and the data cannot be summarized by this sentence. Several other reasons should be mentioned: 1/ In-situ observations represent a snapshot of the ocean while the model outputs are an average (daily, ... not specified in the text). 2/ there is a crucial lack of data to make a robust assessment. 3/ The sampling methods do not allow for a representative sampling. 4/ The scenario uses a climatological simulation, without taking into account inter-annual variability

Response:

We will change line 348 to "The observations, on the other hand, are susceptible to sampling errors such as net avoidance, and do not cover the whole taxonomic and size spectrum of mesozooplankton.", and line 351 to "Several sources of variability are not accounted for in the model as it only simulates the most relevant processes in the system. We employ a climatological model which aims at simulating an average state over several years, dismissing interannual variability. Furthermore, we here compare a three months average from January to March, while observations provide only a snapshot of a highly dynamical system."

Comment:

L407-408: Are these numbers for the coastal upwelling area or the full domain ?

Response:

For the full domain. We will specify accordingly.

Comment:

L408-409: The sentence should be rephrased. It suggests that there are several compartments of zooplankton in their study, whereas there is only one, so obviously "the response depends only on one zooplankton size class". In fact, since this manuscript is based on the Getzlaff and Oschlies (2017) study, this latter should be described before being discussed.

Response:

We will rephrase that section and include a description of the study by Getzlaff and Oschlies (2017) after line 407.

Comment:

L409-412: are these numbers for the global ocean or for your specific area ?

Response:

For the global ocean. We will specify accordingly.

Comment:

L407-412: model with 1 plankton compartment = mild changes. Model with 2 compartments = more pronounced changes. What would be expected with 3 plankton compartments ? Can we think that the more plankton compartments there are in the biogeochemical model, the greater the change in plankton biomass ?

Response:

We would not draw this conclusion from the experiments because there are more differences between the two models. It would be necessary to compare exactly the same model changing only the number of zooplankton compartments. Furthermore, large

zooplankton acts in our model as the main driver of the process, masking the impact of changing the mortality of small zooplankton. In addition, the grazing pressure by small zooplankton has a zig-zag effect on the trophic chain, resulting in an inverse response between the two size classes of zooplankton and phytoplankton.Therefore, we might expect similar responses, if not weaker, in a model with three zooplankton compartments, depending on the ecological role of the third compartment.

Comment:

L428-429: I do not understand this sentence. Figure 5 shows a maximum at the coast and not at the transition from coast to open ocean.

Response:

In this sentence we do not mean that the bloom occurs in the transition between coastal and open ocean. Instead we mean that the bloom develops at the same time that water is being advected offshore. We will rephrased to: "The phytoplankton bloom, which develops closest to the coast and then is offset while water is transported offshore, can be explained by an imbalance between sources and sinks, triggered by changing environmental conditions."

Comment:

Table 1: explain in the legend why "Global" and "Full" are put together, same for "Tropics" and "Oligotrophic", "Southern ocean" and "Coastal Upwelling"

Response:

We will add to the legend: "We grouped together "Global" and "Full" because both refer to the whole model domain in each of the two studies. Similarly, "Oligotrophic" and "Tropics" refer to regions characterised by low nutrient concentrations, and "Coastal Upwelling" and "Southern Ocean" are both regions with high nutrient concentrations."

Comment:

L445-447: I am sorry, I do not find the same numbers. Could you detail them please ?

Response:

We used the formula: $\frac{\mu_i \cdot (Z_i \cdot F)^2 - \mu_R \cdot (Z_R \cdot F)^2}{\mu_R \cdot (Z_R \cdot F)^2} \cdot 100$, where Z_i is the amount of nitrogen in each experiment, Z_R is the nitrogen in the reference scenario, μ_i and μ_R are the mortality rates and F is a conversion factor to transform from Tg of nitrogen to mmol N m^{-3}. Since F is the same in all cases, this simplifies to: $\frac{\mu_i \cdot Z_i^2 - \mu_R \cdot Z_R^2}{\mu_R \cdot Z_R^2} \cdot 100$.

Comment:

L 494: I do not understand. Figure 4 shows that grazing on Zs is not affected

Response:

We do not show any grazing fluxes in Fig. 4 of the paper. This sentence refers to Fig. 6. We will add a reference to Section 3.3 here.

Comment:

L 497: ENSO seems to be the main factor but is not discussed.

Response:

The investigation of ENSO effects are not the main purpose of the paper. We mentioned it as a possible cause of fluctuations in small pelagic fish. However, this paper does not address the causes of such fluctuations, but rather focuses on the consequences. Therefore, we will remove the sentence mentioning ENSO in the conclusions section to avoid confusions.

Comment:

Technical corrections

L41: the units “Mt” has not been defined above, please define it or use the full name.

L119: correct “dissolved”

L129-130: correct the exudation symbol

L272: please remove “top” when you refer to Figure 5

L273: please remove “top” when you refer to Figure 5

Figure 6: please check units

L337: please change ”ZL” to “ZL”

L339: “estimated ”instead of “estimate”

Appendices: Please place text together with figures for each Appendix.

Response:

We will implement all technical corrections.

References

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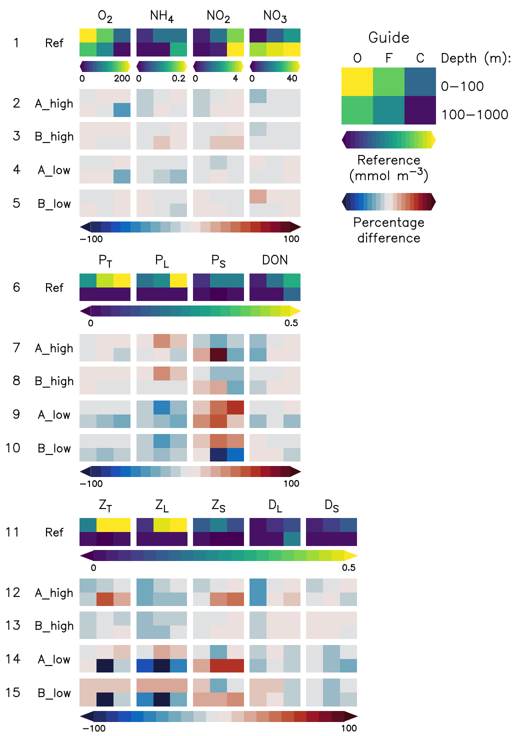

Fig. 1. Same as Fig. 4 of the paper after correcting the averaging weights in experiments A.

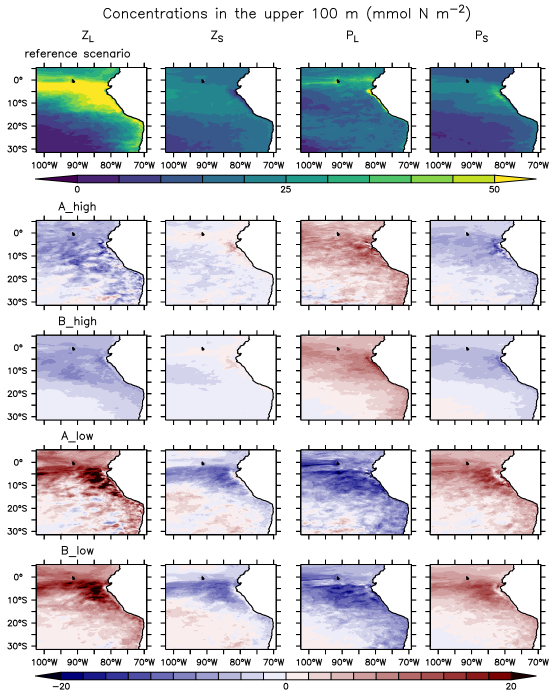

Fig. 2. Same as Fig. D1 of the paper after correcting the averaging weights in experiments A.

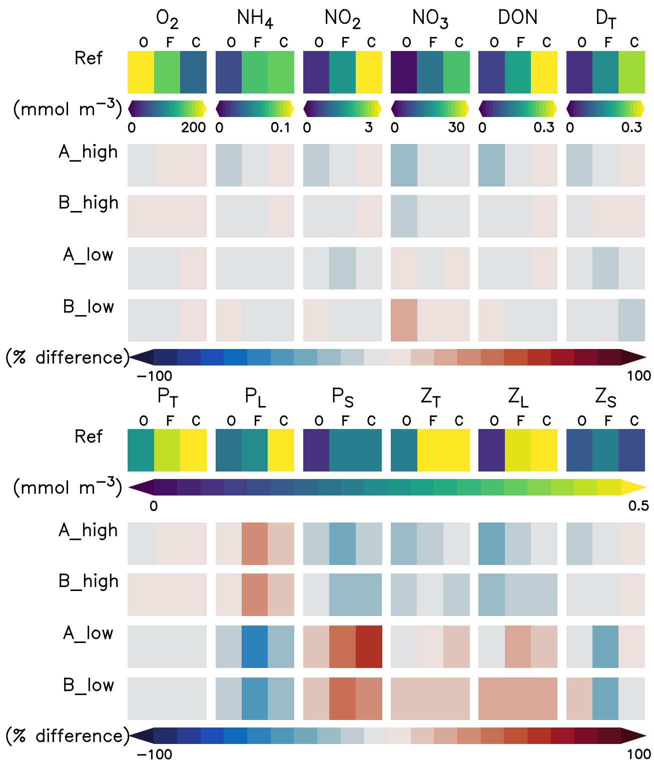

Fig. 3. Same as Fig. 4 of the paper after correcting the averaging weights in experiments A and removing the deep water (100 to 1000 m) layer.

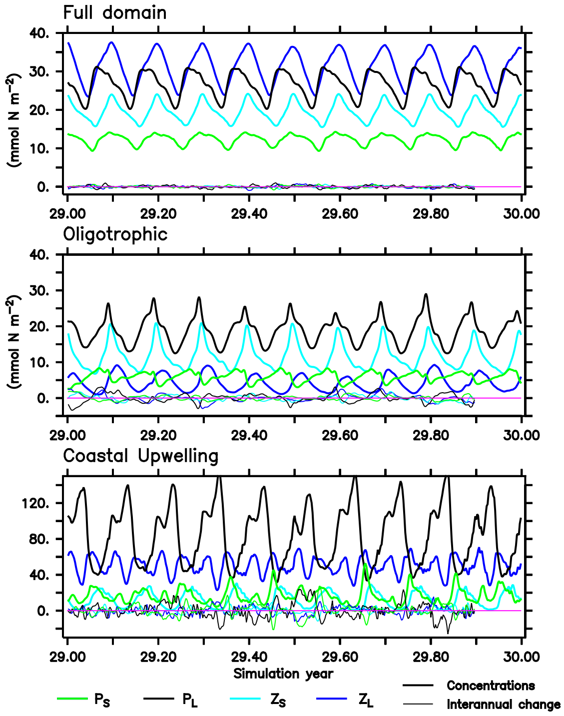

Fig. 4. Time series from year 21 to year 30 of integrated concentrations (upper 100 m) averaged over space (thick lines), and percentage difference between every point in time and the same date one year later

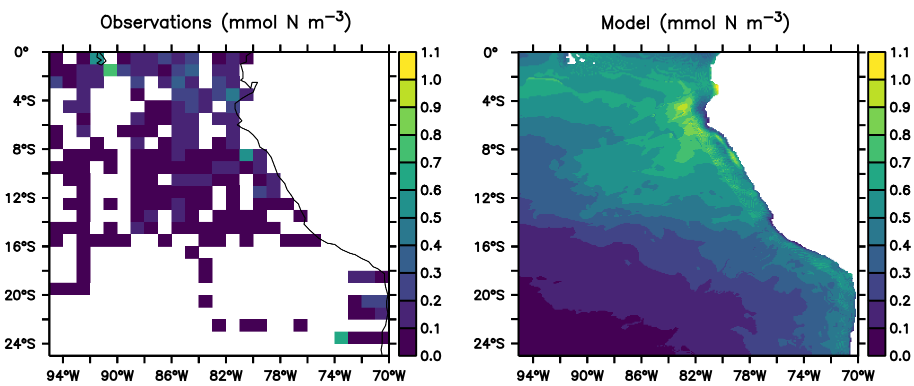

Fig. 5. Comparison of mesozooplankton observations from the global dataset by O'Brien and Moriarty (2012) (see main text for more information), and model large zooplankton averaged over the upper 100 m depth