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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

This sensitivity study of zooplankton mortality in a regional physical-biogeochemical model of the Eastern Tropical South Pacific provides valuable insights into the responses of ecosystem dynamics and biogeochemistry caused by changes in zooplankton abundance. The relatively simple approach enables disentangling the main drivers

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of change and their relative strength, showing e.g. that large zooplankton assert a top-down control on both small zooplankton and phytoplankton, causing asymmetrical ecosystem responses.

The paper is well-written, fluent and concise, clearly outlining the methods and assumptions, except for some minor issues addressed in the specific comments. It builds on the conceptual framework of Gezlaff and Oschlies (2017), but applies it to a new study system on a different spatial scale using a different model. The comparison of outcomes from the current regional study to the previous global study is one of the main strengths of this paper.

The model used has been fully described and partially validated in previous papers, with the most relevant equations and parameters repeated in this paper. However, I would like clarifications to some parts as written in the specific comments.

I also have two concerns regarding the zooplankton model formulations. It seems to exclude two important functions for zooplankton dynamics: i) excretion of DIN (ammonium) and ii) temperature-dependence of zooplankton growth/metabolism (see e.g.Tian 2006, Ecol. Model.; Richardson 2008, ICES J. Mar. Sci.). The first might be a mistake in the text (lines 141-143), as DIN excretion is included in the model as described by Gutknecht et al. (2013). If these functions are indeed omitted, I would like the authors to motivate why and discuss if they think this could have major implications for the simulated plankton biomasses and biogeochemistry of the system.

The validation of zooplankton biomass over depth shows that the model is not a very accurate representation of the specific system, especially since simulated surface concentrations are one order of magnitude higher than measured ones. However, the authors provide a thorough discussion of this, and I think the results are still sufficient to support the interpretations and conclusions drawn from the study. I believe the results from this study are a valuable contribution both to the general scientific understanding of real-world plankton dynamics and their effects on the ecosystem, as well as to the

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development of commonly used biogeochemical ecosystem models.

In summary, I think this paper lives up to the standards of Biogeochemistry and recommend that it should be published after minor revisions.

Response:

Dear referee,

The authors thank you for your useful comments and support for our paper. Part of the metabolic losses of zooplankton do become DIN, we will correct this. Zooplankton metabolism and mortality are not affected by temperature. We will comment of this in the revised version of the paper.

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 the suggestion by referee three (presented here as Figure 3). These changes do not change any of our conclusions.

Comment:

SPECIFIC COMMENTS AND TECHNICAL CORRECTIONS

Title clearly reflects the contents of the paper.

Abstract provides a concise and complete summary.

Equations 2, 3 and line 131. Please check the symbol for exudation fraction.

Response:

We will fix the symbol in Eq. 3.

Comment:

Lines 134-135 and 244-245. Please write out what P-I curve stands for. You might need to write out the growth function JPi(PAR,T,N) to make this part understandable.

Response:

We will write down that P-I stands for photosynthesis-irradiance and explain the growth function.

Comment:

Lines 141-143. You write that both mortality and metabolic losses become detritus or DON. Is there really no excretion of DIN?

Response:

One fraction of the metabolic losses does become ammonium without further remineralization steps in between. We will correct this in the manuscript.

Comment:

Line 204. You use a spin-up of 30 years. Is this enough to reach steady state? If not please elaborate on why you chose this time period and how the transient state it might affect the results.

Response:

We do not dispose of the high resolution time series of the reference run at the moment. Therefore, we have provided a time series of the final 10 years of the spin-up of simulation A_high (Figure 4). While the deep water might not be in steady state after the 30 years spin up, the upper 100 m are already quite stable after 20 years. The averaged relative changes in concentrations for the same day in different years for the averaged full domain are equal or lower than 0.1 % for the four plankton groups. Nevertheless, concentrations vary more in the smaller regions. For the oligotrophic region, small zooplankton exhibits the minimum averaged relative interannual change (-0.007 %), and large zooplankton the maximum (5.2 %); while in the coastal upwelling

region, the minimum and maximum averaged relative interannual changes are 0.3 % for large zooplankton , and 9.4 % for small zooplankton respectively. In comparison, Le Quere et al. (2016) reported that the 10 plankton functional groups in their model (only four in ours), reached equilibrium after only 3 years.

Comment:

Lines 158-159. Missing explanation for subscript mathrm in PmathrmS and PmathrmL

Response:

The mathrm subscript label was an error in the LaTex code due to the line break. It will be fixed to P_S and P_L.

Comment:

Lines 258-259. “In deep water (between 100 and 1000 m) most of the plankton compartments (Figure 4) present mild to strong relative responses.” I am not sure what you mean here, what could they show other than mild to strong responses? Maybe reformulate to, e.g. ”...relative responses vary from mild to strong: : :”

Response:

We will remove the explanation on plankton groups response in the deep water, and remove this layer from Fig. 4 of the paper as suggested by another referee, because this does not provide anything to the conclusions of the study and the deep layers make Fig. 4 unnecessarily complicated.

Comment:

Line 262 and 268. Please change Appendix “C” to “D”

Line 263. “:” should be “.” or “Concentrations” should be “concentrations”

Lines 272-273. Please remove “top” from “top right/left”, as there is only one row of figures.

Response:

We will make these corrections.

Comment:

Line 276. Please add “zone” to “coastal upwelling zone”

Response:

We will add "region" here because this is how we refer to C in the whole paper. There was a mistake in Sect. 2.5 where we called it "coastal upwelling section". This will also be corrected.

Comment:

Lines 284-285, Fig 6. Units seem to be mixed (m-2 and m-3). Please check.

Response:

We will correct the units referring to Fig. 6 in the text. The correct units are mmol N m^{-2} d^{-1} and mmol N m^{-2}

Comment:

Lines 301-308: Although this paragraph is correct, I had to read it several times before understanding what you mean. Would there be a simpler way of expressing this?

Response:

We will rephrase this paragraph and also refer to the nitrogen loss due to zooplankton mortality as $\mu_{Z_i} \cdot [Z_i]^2$ to simplify the text.

Comment:

Section 4.1 (e.g. lines 337-342). How come you use a range of mortalities that is much lower than the one estimated by Hirst and Kiørboe (2002), when the simulated zooplankton concentrations are an order of magnitude higher than measured ones,

and the sensitivity analysis shows that increasing mortality decreases biomasses? I see your argument that measured data is uncertain and may be lower that actual mass, but I see no reason to assume that it would be an order of magnitude too low? It would be interesting to see if you could improve model-data fit by using the high mortality rate estimated in the field (0.19/day, Hirst and Kiørboe, 2002) or used in other models (0.25 mmol N m-3/day, Lima and Doney, 2004).

Response:

The zooplankton mortality has a rather broad range of values in biogeochemical models, evidencing the little information available to constrain it. The value by Lima and Doney (2004) lies in the upper range of mortality values found in the literature. The estimate by Hirst and Kiørboe (2002) at 25° C is about twice as high as our estimate for the high mortality scenario. On the other hand, the estimate at 5° C lies between our low mortality and reference scenario estimate.

The current set of parameters of the model were adjusted mainly to represent nutrient and oxygen concentrations in this region, and the model at that stage was not tuned to match plankton well (see Jose et al., 2017). Indeed, this is the first time that we evaluate any of the zooplankton compartments for the Easter Tropical South Pacific region. In addition, the ±50 % change in mortality, aside the comparison to observations, was also picked to compare against the study by Getzlaff and Oschlies (2017). The results of this study have indeed drawn a direction for the further improvement of the model. However, a stronger change in zooplankton mortality than the values used here may require further adjustment of other parameters. In such a complex model with considerable uncertainty in its parameters and little observations to compare with, a proper tuning is a complex task (see, e.g., Kriest et al., 2017). Therefore, we have not included it for the present study but aim at improving the model for further work. Nevertheless in the revised version of the paper we will expand the comparison to also mention the mortality estimates by Hirst and Kiørboe (2002) at 5° C and will discuss further avenues of model improvement with regard to zooplankton in the revised manuscript.

Comment:

Line 408. It would help the reader if you would add a short description of the Getzlaff and Oschlies (2017) study the first time it is mentioned in the discussion, e.g. what system(s) did they study, what kind of model (global/regional), same methods of sensitivity analysis? I see now that this comes a few paragraphs down. Maybe you could move part of it up to line 408?

Response:

We will include a brief description of the study by Getzlaff and Oschlies (2017) in line 407 and rephrase the following lines.

Comment:

Line 464. Please change “affects” to “affect”.

Response:

We will correct this.

Comment:

Reference list. References are sufficient and relevant.

Line 631. The doi for Jose et al. 2017 leads to another article.

Response:

We will change to the correct DOI: 10.5194/bg-14-1349-2017

Comment:

Appendices. Contents of Appendices are relevant for the study. Please place text and figures of each Appendix in a logical order.

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We will try to position every figure next to the text of its corresponding appendix section. We will exchange the positions of Appendix A and B to their order of appearance in the text.

References

Getzlaff, J. and Oschlies, A.: Pilot Study on Potential Impacts of Fisheries-Induced Changes in Zooplankton Mortality on Marine Biogeochemistry, Global Biogeochemical Cycles, 31, 1656–1673, https://doi.org/10.1002/2017GB005721, 2017.

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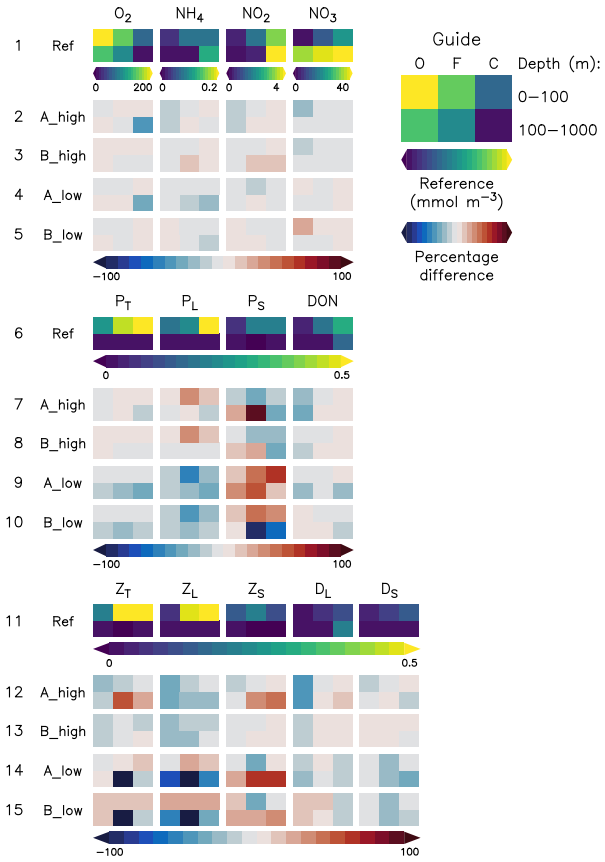

Fig. 1. Same as Fig. 4 of the paper after correcting the averaging weights in experiments A.

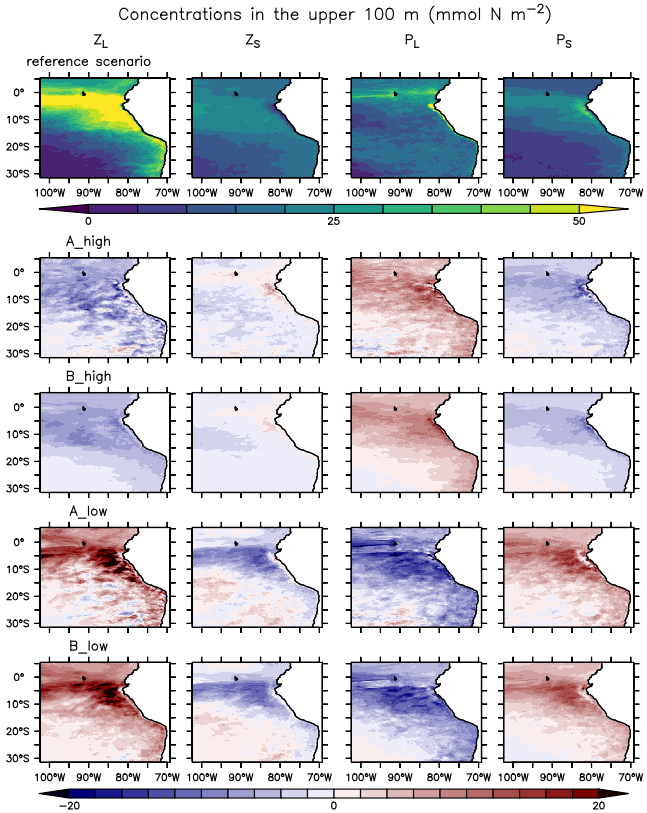

Fig. 2. Same as Fig. D1 of the paper after correcting the averaging weights in experiments A.

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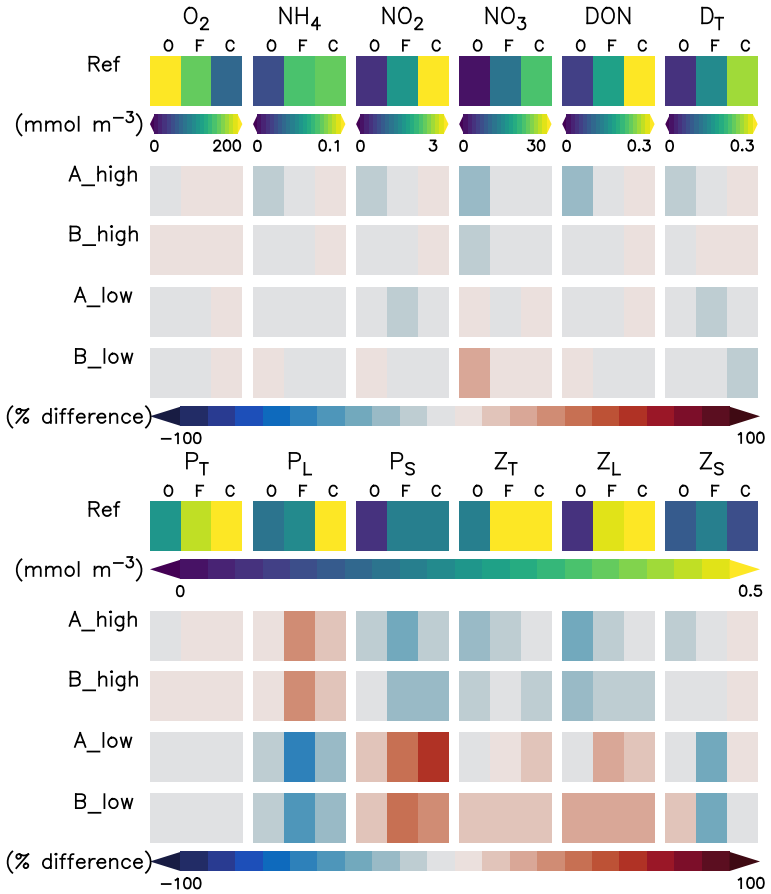

Fig. 3. Same as Fig. 4 of the paper after correcting the averaging weights in experiments A and removing the deep water (100 to 1000 m) layer.

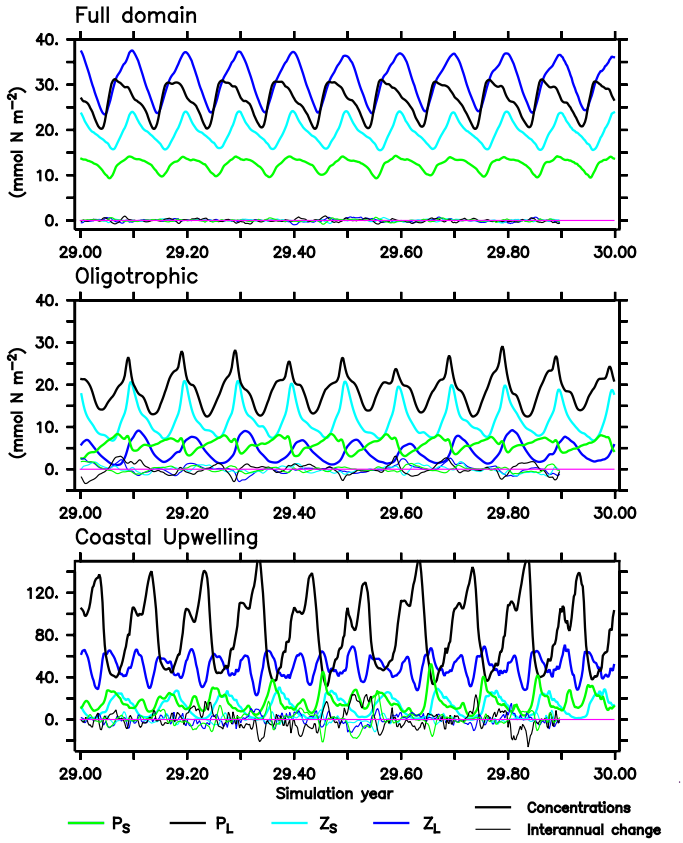

Fig. 4. Time series from year 21 to year 30 of integrated concentrations (upper 100 m) averaged over space (thick lines), and percentage difference between every point in time and the same date one year later

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