

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.

Anonymous Referee #2

Received and published: 11 January 2021

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 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 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.

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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.

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

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

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.

Lines 158-159. Missing explanation for subscript m in $P_{\mathit{m}}S$ and $P_{\mathit{m}}L$

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. . ."

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.

Line 276. Please add "zone" to "coastal upwelling zone"

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

Lines 301-308: Although this paragraph is correct, I had to read it several times before

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understanding what you mean. Would there be a simpler way of expressing this?

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 than 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).

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?

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

Reference list. References are sufficient and relevant.

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

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

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-417>, 2020.

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