

Interactive comment on “A new marine ecosystem model for the University of Victoria Earth system climate model” by D. P. Keller et al.

Anonymous Referee #2

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This is a good manuscript and is appropriate for being published from GMD. However, the reviewer would ask the authors to make some revisions of the manuscript before publication. Here are some specific comments and concerns on the manuscript.

In Section 4: When describing their ecosystem model configuration, the authors seem to expect readers too much to refer to previous papers like Schmittner et al. (2005, 2008). Of course, they do not need to repeat description of previous papers here in this manuscript. However, the reviewer would strongly recommend the authors to clarify all the parameters' name and the abbreviations at least. Some names and the abbreviations (such as in Eqs. (2), (3), (8), (11), (12), (13), (15) and in page 1148 line 1) show up without definitions/notations (the reviewer is sorry if s/he misses the description in the text). This revision would help readers read this manuscript with less

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necessary to refer to the previous papers cited in this manuscript and make easy to understand the content.

Equation (8): More description is necessary for modeling oxygen cycling in the text.

Page 1148 lines 13-17: This sentence is too long to be without any comma(s). Please delimit this properly, with comma(s).

Page 1150 line 13: Here the authors call “(1 – gamma)” as “assimilation efficiency”. However, it seems to make readers confusing because gamma is already defined as “assimilation efficiency” (in Table 1). Is there any possibility for the authors to change this?

Right side of Equation (29): A parenthesis (“(”) is necessary before “(1 - gamma)”. “RCaCO₃/POC” → “RCaCO₃:POC” (refer to Table 1) No definition of “RC:P” was found (the author is sorry if s/he misses). If this is identical to “RCaCO₃:POC”, the authors do not need to put both “RCaCO₃:POC” and “RC:P” (same as for “Iz=0” and “PAR” in Equation (12)).

Page 1153 lines 9-12: This sentence looks awkward and a little hard to understand. Please rephrase.

Page 1154, lines 8-9: Is it possible for the authors to show which locations are limited by iron, nitrogen and phosphorus, respectively, with figure(s)? This information should be very informative and can be only provided by modeling studies.

Page 1155 lines 16-18: Is this statement for Figure 16? Denitrification seems prominent in Bay of Bengal, only for a new model, not both models. None of the models seem to simulate denitrification strong enough to be stressed on in this text, for the coasts of Namibia and Aden.

Page 1156 lines 6-8: The reviewer would recommend similar statement for PP (Fig. 14) and N₂ fixation (Fig. 15) in the text, and discuss briefly the relationship.

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Page 1156 lines 21-22: Conversely, the reviewer is wondering if diazotrophs was simulated very uniformly in space with the old model. Could the authors comment on this?

Page 1157 lines 10-12 (“To evaluate how well . . .”): This sentence looks awkward and a little hard to understand. Please rephrase.

Page 1158 lines 24-25: The reason why the rain ratio is extremely high in polar regions is because PP is low there. Therefore, the rain ratio itself does not have specific meaning for the polar regions, and seems better not to stress on this. Accordingly, the reviewer recommends the authors to replace Fig. 23(b) with POC flux when comparing to CaCO₃ flux (Fig. 23(a)), which seems to be more straightforward for the comparison.

Page 1159 line 7: The old model seems to do a good job when compared to satellite-based estimates (Fig. 18).

Page 1161 line 11: Could the authors put the ratios in number (heterotrophic to autotrophic biomass ratios in the open ocean) here?

In Summary and conclusions: The reviewer considers that one of the most important improvements by the new model as a part of the earth system climate model is the seasonality. Therefore, s/he would recommend the authors to describe more which procedures especially lead to the improvement in this section (and in abstract if possible).

Table 1: “CaCO₃ over nonphotosynthetical POC production ratio): other terms like “nonalgal” sounds more appropriate than “nonphotosynthetical” because here one discuss the material rather than the process.

Fig.1: There is an arrow from Z to D that is called “grazing”. The term sounds very unnatural. Is it possible for the authors to replace this term with alternative one?

Fig.2 (a): Which kind of phytoplankton, PO or PD?

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Fig.3. It seems better to show the result of two extreme months, like January vs. July or December vs. June, not January vs. June.

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