

Response to Anonymous Referee #2

Comments from Anonymous Referee # 2:

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

Response: We have added a new table (Table 2) that defines parameters and variables that are not specifically mentioned in the text. This new table also refers to where the original variable equations or parameter values were published and, when possible, we have included the table or equation numbers from the original publications.

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

Response: We agree that we had not provided enough information on oxygen cycling. We have therefore now defined the terms in this equation (8) in the new Table 2 and provided a brief description 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).

Response: We have corrected this sentence.

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?

Response: We agree that this is confusing and have changed our wording to indicate that gamma is the assimilation efficiency coefficient and that the assimilation efficiency is (1-gamma).

Right side of Equation (29): A parenthesis ("(") is necessary before "(1 - gamma)".

Response: Done.

"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 "I_{z=0}" and "PAR" in Equation (12)).

Response: The "r" has been changed to a "." and we have added $R_{C:P}$ to Table 1 since it is not identical to the other parameter. As for $I_{z=0}$ and PAR in equation 12, these are defined in the text immediately after the equation.

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

Response: We have rephrased this sentence.

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.

Response: While we agree that this is useful information we did not include such a figure for two reasons. First, iron limitation is determined before the model selects between light, nitrogen, or phosphorus limitation (i.e., temperature and iron limitation are determined first to calculate a maximum potential growth rate (Fig. 2 a) before a "min" function selects the other limiting factors that determine

the realized growth rate). Thus, it is not possible to easily compare iron limitation with that of nitrogen or phosphorus. The model was configured in this manner because iron is necessary for photosynthesis, the reduction of nitrate to ammonium, and a number of other key cellular processes that must occur for the uptake and utilization of nitrogen and phosphate to be possible. Since this was not clear to the reviewer we have revised Section 4.2.1 to better describe our model and the factors limiting the growth of phytoplankton. Second, between nitrogen and phosphorus, nitrogen was almost always the limiting factor in the model. Therefore, we felt that it would not be very interesting to show a map of this and have instead just stated this at the end of the first paragraph in Section 5.2.1. However, if, after reading our response, the reviewer still desires a figure that indicates whether an area is nitrogen or phosphorus limited, we will be happy to provide one.

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.

Response: Yes, this statement is for Fig. 16 and we can see why this was not clear to the reviewer due to the poor figure quality and the color scheme that we used. At higher magnification we could see denitrification in the Bay of Bengal in our original figures for the old model runs. Additionally, when viewed at higher magnification there is enough denitrification off of the coasts of Namibia and the Gulf of Aden with the new model for it to be acknowledged as important. Furthermore, with the old model the highest rate of denitrification was off the coast of Namibia and thus we feel that it should be mentioned. We apologize for the size and clarity of the figures and if the discussion paper is accepted for publication in GMD we will work closely with the journal during typesetting to insure that the figures are clear and these areas of denitrification can be seen. We have also redone the figure with a different color scheme so that these areas of denitrification are more clearly defined.

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.

Response: We agree. Thank you for pointing this out. A similar statement for PP is presented in section 6.2 and we have added a statement concerning N₂ fixation to the appropriate section.

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?

Response: Thank you for asking this question. Diazotroph biomass does vary seasonally with the old model and is not uniformly distributed in space. However, when their biomass is averaged zonally to produce a Hovmöller figure the seasonal variations are averaged out making it appear as if they are uniformly distributed in space. We have added a few sentences to this section explaining this.

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

Response: This sentence has been rephrased.

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.

Response: While we agree that the rain ratio is not particularly meaningful at high latitudes in the winter due to low or nonexistent PP, we have decided to keep Fig. 23 b because we feel that it is important to show the seasonal rain ratio at mid to low latitudes and the rain ratio during, and following, the spring bloom at high latitudes. However, to address the reviewer's concern we have added a sentence to this section that discusses the high rain ratio in polar regions during the winter (i.e., stressing that it does not have a large biogeochemical impact due to low PP).

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

Response: While we agree that there are some aspects of the old model that compare more favorably than the new one, such as the areas between 20 and 40 deg. N and S. There are a number of aspects that do not compare well. The old model has too little seasonal productivity a high latitudes and too

much in the equatorial region (especially when compared to the VGPM model). We apologize if the figure is somewhat small making it difficult to see these differences and plan to work with the typesetters to improve the figure size if the manuscript is accepted for publication.

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

Response: Done.

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).

Response: Thank you for this suggestion. The improved ecosystem seasonality is mostly the result of making zooplankton growth temperature dependent and we have added a sentence indicating this. However, since we did not specifically focus on the how the zooplankton growth formulation effects seasonality, and are preparing another manuscript on this subject, we are hesitant to say more here without presenting a more thorough analysis.

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.

Response: We agree and have replaced the term “nonphotosynthetical” with “nonalgal”.

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?

Response: Yes. The term has been replaced with “egestion and sloppy feeding”.

Fig.2 (a): Which kind of phytoplankton, P_O or P_D?

Response: We meant P_O and have added this to the caption.

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.

Response: Thank you for the suggestion. We have changed the figure so that it is now January vs. July instead of January vs. June.