

To the Editor

We appreciate the constructive comments from the reviewer, and we have completely agreed with and addressed each of these comments, as outlined below. Through these changes, we feel that the revised paper is now substantially clearer and stronger. Detailed replies to each of the reviewer's points are provided below.

Page 1, Line 12: it would be nice to see some example numbers which illustrate what is meant by good

We now include correlation coefficients to illustrate this.

Page 1, Line 14ff: According to Table 3 the judgement of the model performances depends on the considered variable and also all the models are not directly comparable. Also, the considered Fasham-model is now more than 30 years old. I would thus suggest to tone down. I am fine with a statement that the performance of the new model at the test location is well within the range of comparable models. Also, some specific strengths of the model could be listed in addition.

We have toned down this statement and added some strengths.

Page 2, Line 41: I find this difficult to read (unclear what "those" and "they" refer to).

We now clarify what is being referred to.

Page 2, Line 46ff: Split the sentence to make it easier to read?

We have split the sentence into two.

Page 2, Line 50 Better? "..., a second common BGC modelling approach..."

We have made this change.

Page2, Line 59: the disconnection

We have changed this word to "difference" instead of "disconnect."

Page 3, Line 71: I would suggest to start a new sentence after "ratios".

We have made this change.

Page 3, Line 79: obtaining → deriving

We have made this change.

Page3, Line 81: I would rather say that the focus is to introduce the new model and to provide a first rough comparisons to BFM56 and observations, because the model assessment in the presented manuscript is rather limited.

We have made this change.

Page 3, Line 84ff: This sentence is rather long and difficult to follow. What is the key message

here?

We have cleaned up this sentence to help clarify its message.

Page 3, Line 100ff. I would be more careful with this statement (because it is not proven) and use “we anticipate ... that ... might ...” instead of “postulate...”

We have made this change.

Page 4, Line 116: lesser significance → minor importance

We have made this change.

Page 4, Line 117ff: I find “subject to the constraint” difficult to understand and would also suggest to start a new sentence here.

We have removed this wording and started a new sentence here to provide clarity.

Page 4, Line 150ff: I find this a bit confusing because top-down control can act at different levels.

We have now added to this sentence that the top-down control we are referring to is an explicit term for predation of zooplankton.

Page 7, Line 176 The notation of this equation is rather unusual. Also, the variables and abbreviations should be introduced directly (e.g., W appears first in line 187). The same holds for most other equations in this section.

We now clarify that all equations in this section and in Appendix A use this notation because it is the same as it used in all other BFM papers, for consistency. Additionally, we now introduce all variables directly after they are used.

Page 7, Line 194ff: It should be mentioned that only the upper part of the water column is simulated (please provide the respective depth level).

We have now indicated in this sentence that we only simulate the upper 150m of the water column.

Page 7, Line 200: It does not become clear where the prescribed temperatures and salinities originate from observations or the 3D model?

We now mention within this sentence that the temperatures and salinities are obtained from the observations.

Page 7, Line 201: How are the horizontal velocities obtained and how are they used in the coupled setup?

We now mention within this sentence that the horizontal velocities are from the POM 1D physical model.

Page 9-10: I must admit that I am lost.

We now preface this section of text with a sentence highlighting the fact that the ensuing discussion is a description of the equations solved in the POM-1D physical model.

Page 11, Line 276: Its problematic to use the same data for calibration and validation. I would rather suggest to refer to a first model test here.

We have replaced “calibration and validation” with “testing”.

Page 11, Line 285: Please specify how the smoothing was done.

We now specify here that it was done with a locally estimated scatterplot smoothing (LOESS) method.

Page 12, Line 289: I missed what this criterion is used for.

We have added clarification to this sentence to make it clear that this criteria was used to determine the maximum mixed layer depth in the climatology.

Page 12, Line 290: What is meant by “similar processing”?

We now clarify that this is referring to the same averaging, interpolating, and smoothing process described for the physical variables.

Page 12, Fig.2: Could the mixed layer depths be included?

We have now included the mixed layer depths in Fig. 2, panel (a).

Page 13, Line 298: Could it be briefly described what the correction does?

We now specify that this correction specifically reduces the error incurred by linearly interpolating the monthly averages to the model time step.

Page 13, Line 325: How was this splitting done?

We now specify that the the total initial carbon values for phytoplankton and zooplankton used in BFM17 were split into equal amounts for all the phytoplankton and zooplankton groups in BFM56.

Page 14, Line 331: I would not call this validation rather “model assessment at a test location” or similar.

We have changed the section title to “Model Assessment Results”.

Page 14, Line 349ff: I don’t see that the simulated subsurface chlorophyll maximum is particularly pronounced in summer. But maybe this refers to the color scale?

We now specify in parentheses at the end of this sentence that this maximum is evident in Figure 4(g) as a larger chlorophyll concentration at depths close to 100m during the summer months.

Page 14, Line 341ff: I disagree that the oxygen fields look similar. Adding correlations might,

however, convince me. Otherwise, I agree with the authors that the large discrepancy is very likely related to the representation of mixing in 1D and I think there is no drawback just to mention this here (as the authors do anyway later in the text and which could be removed then).

We agree that this statement was too strong and have changed it to "...not completely dissimilar..."

Page 14, Line 342ff: I would not talk about a trend in a seasonal cycle.

We now state that "These results are consistent with those from BFM56..."

Page 14, Line 344: Is there a potential reason why there are discrepancies in chlorophyll and oxygen?

We now offer potential reasons for these discrepancies, namely, the additional phytoplankton and zooplankton groups, nitrification, and explicit representation of remineralization in BFM56.

Page 15, Line 354: "Talk about quantitative evaluation" and "skill assessment" is in my eyes a bit too much - due to the limitation to a single location in an 1d model environment, I would rather call this "a first indication on the model performance".

We have made this change.

Page 17, Line 386ff: A lot of these findings were already described earlier.

We have removed this paragraph.

Page 17, Line 400: I do not agree here because the model performances depend on the considered variables (as also outlined in the manuscript above).

We have now scaled this claim back to not include all of the target fields.

Page 21f, Line 444: Does the BG model feedback on temperature and irradiance? If not this sentence is confusing. Also, I would tend to delete "inputs".

We now clarify that BFM17 is only influenced by the environment through the temperature and irradiance and have deleted "inputs".

Page 21ff, Appendix A2: I find it very difficult to follow the equations. First the notation is very unusual and then generally the abbreviations are not introduced directly. Partly I can kind of guess what it meant.

We now clarify that all equations in Appendix A adopt this notation because it is the same as it used in all other BFM papers, for consistency. We introduce the abbreviations in Table A2, for clarity.

Page 31ff, Appendix B: The idea behind constructing a sinusoidal forcing does not get clear to me and also I missed how winter and summer extremes were defined. Why did the authors not consider measurements here? It might make sense to skip B1, unless there is a clear purpose behind it.

We now clarify that we used an idealized version of the observations for simplicity, as there is no

physical dimension in the 0D framework to properly apply the observational variables to.

Line 682: What is meant by “self-consistent”?

We have removed this term to reduce confusion.

Line 685ff: I did not gain any confidence in the accuracy of the model by Fig. B1. Please rephrase and make the idea behind these experiments more clear.

We have added, at the beginning of Appendix B, that the idea behind this test is to confirm its efficacy as viable BGC model without the – sometimes heavy – influence of any one particular physical model.