Answer to reviewer #1

Dear Reviewers,

You will find here below our replies point by point to each of your comments.

We have prepared a revised version of the manuscript that incorporate all the changes mentioned in our replies. In particular, we have:

1) clarified and reformulated in the text some model formulations (benthic, optical, phytoplankton groups composition).
2) added additional model-data comparison (adding additional validation) Yet this paper is not a validation paper and a companion paper will describe in details the validation of the model over 1950-2025.
3) moved to the appendix some materials to lighten the main text.
4) made the minor corrections suggested.

We thank both reviewers for the time they spend reviewing our work.

Kind regards,

Marilaure Grégoire on behalf of the coauthors.

Comments of the reviewer in normal, our answer in italics.

I think this is a useful and well-described model. I cheer the authors for putting so much effort into the description of the model, although I did not feel like I could meaningfully review the aggregation module. My main concern is that despite the substantial effort to describe the model, the testing of the model is somewhat limited. I think the hypoxia/oxygen comparison is helpful, but I would like to see more justification for the benthic module and more detail presented on the Black Sea ARGO comparison. General and detailed comments are listed below.

General Comments:

Is the benthic module really much better than a reflective model? There is not accumulation of solutes, sorption, etc, so maybe there should be a discussion as to how this module would compare to a simple reflective model, like the one in the Fennel ROMS biogeochemical module? As I read lines 620-625, there seem to be even more assumptions and ‘fixes’ made to permit the incorporation of the benthic module. Part of this discussion or consideration should be whether this benthic module could store materials seasonally or long-term, as otherwise it may not offer much real mechanistic value. I can see in the application that the model produces seasonal changes in sediment-water exchanges, presumably due to the temperature effect on remineralization, but there are not enough observations to indicate if this is correct. The model may get a relative magnitude of the sediment solute fluxes, but the time variation is super important to justify the somewhat complicated approach to the benthic module.

The reviewer is right, the benthic model does not simulate the accumulation of solutes in the sediment. Yet it permits the accumulation of the solid sedimentary materials. Indeed, it simulates the accumulation of two pools of organic carbon and detrital silicate, in both cases for a fast and a slow reacting pool. Thus, a reflective boundary condition that assumes that all the detrital material is instantaneously reinjected in the water column is not equivalent. This is shown in Soetaert et al (2001) that did an extensive review of the modelling of benthic-pelagic coupling and compared 6 formulations *of the benthic-pelagic exchanges going from no-bottom (simplest) until vertically resolved (most sophisticated). This paper concluded that the best choice that offers a good compromise in terms of tractability and reliability is an approach that explicitly simulates the vertically integrated sedimentary particulate matter and where the bottom fluxes of dissolved constituents are parameterized based on mass budget considerations. This is the formulation we have chosen here in the benthic model of BAMHBI. We compare benthic pelagic fluxes of solutes and bottom oxygen with observation to assess the quality of the benthic model. For this comparison we use all the available datasets.*

The seasonal cycle is imparted by a temperature effect but also by the seasonality of the bloom that reaches the bottom, accumulates and is progressively degraded. The benthic model is described in section 2.2.5. All the equations and parameterization are described in details as requested by GMD.

I found the application of the model in the Black Sea to be superficial and unsatisfying. Can the authors offer a little more sensitivity analysis of processes related to these variables and how this changes the comparison?

This paper is meant to highlight this model, and despite an enormous amount of text describing the model, there is very little analysis to show how well it works.

Indeed, the manuscript extensively described the model equations. This was a request from GMD. With case studies, we have demonstrated the performances of the model on the shelf focusing on benthic-pelagic coupling and bottom oxygen while in the deep sea we have shown comparison of the simulated chlorophyll and oxygen with BGC Argo observations. In addition, as also requested by the second reviewer, we have provided comparison of the seasonal and interannual cycle of surface oxygen and nitrates in the deep sea. A paper dedicated to an extensive description of the validation of a hindcast simulation is in preparation. Here we focus on providing the technical details, with selected case studies showing comparison with observations.

Specific edits/comments

Line 45: “sesaonal” is incorrectly spelled.

Corrected.

Line 61: I think the authors should also not that parameter definitions are provided in Table 2

The reviewer is right and we have added a reference to table 2 as well.

Table 1: “Silicilic” should be “Silicic”

Corrected

Benthic Module in table 2: for Panox, the word “Fraction” is missing an F

Corrected

Line 117: I think there needs to be a section describing the chlorophyll module in more detail? In Table 1, the reader is given the impression that chla is simply a fraction of each process, and it is unclear how this differs from assuming a C:CHL ratio. But now I see there is an entire section on the Chla content and that should be cited here.

The citation has been added.

Line 132: Is there any further information to give on how the IOP model was tested? In a supplemental material, perhaps.

We have added information on how the IOP parameters have been estimated (from field data analysis by Dmitriev et al., 2009, referred now in the text) and how the model performs with comparison with observations by referring to a companion's paper by Macé et al., (2025) published in GMD discussion that presents the results of the Fulloptics model.

Line 191: The sentence "In case of carbon limitation, when bacteria do not require to consume NHs for DOC consumption,…" I think should be "In the case of carbon limitation, when bacteria are not required to consume NHs for DOC consumption,…"

We have reformulated the sentence.

Figure 3: Why is the are the gelatinous pools not connected to the other variables in this diagram?

This figure shows the flows associated to the microbial loop and then the production of organic material (dissolved and particulate) and its degradation. It represents the mortality, hydrolysis, excretion, egestion, feeding, and messy feeding. The contributions of gelatinous to these fluxes are through the production of POM via mortality and egestion (arrow in brown and dark green), consumption of POC (light green), excretion (orange).

Line 200: by "variable" do you mean that the C:N ratio varies in time, or that the ratio varies by organic matter type? Please clarify

This is a variation in time since for each POM and DOM (labile and semi-labile) components we have a state variable to represent its carbon and nitrogen composition. This amounts to 6 state variables in total, 3 for the carbon and three for the nitrogen.

Line 232: I think it is confusing to describe the ODU as partitioned between the reduced substances. You don't actually model them and their portion is not represented in the ODU production term in the model. I suggest removing this detail. Further, I think Table 3 is not helpful, because not all of these equations are actually modeled. It therefore confuses the reader who might not realize these processes are not being modeled. Simply describing the simplification to ODU is sufficient.

We move the table describing the equations of anoxic degradation in the appendix. This is a good suggestion as it reduces the number of tables in the main text and since this table does not describe model equations but justifies the approach, to have it in appendix does not comprise clarity.

Line 236: I recommend that the detailed kinetic equations are broken up so that they are in the manuscript where they are described. On this line, I need to scroll down several pages to get to Eqs 83-86.

Since we move to the appendix the complete description of the degradation process, this problem does not appear anymore.

Line 386: Are there no temperature dependencies of gelatinous zooplankton? I suppose this is system specific, but these organisms are highly seasonal in temperate systems.

The temperature effect is taken into account via the function f^T. It was not clear in the previous version of the manuscript since the definition of f^T was included in the phytoplankton section. Now, it is included in a subsection entitled, "temperature effect" and the Q10 values for the different groups have been added in the table listing the parameters values.

Line 528: “produces” should be “produced”

Corrected

Paragraph beginning on line 537: So were these parameters estimated once by this approach and now fixed values are used in the module? If so, please say that. It gives the impression that the monte carlo simulation might be a dynamic part of the model. After seeing the equations, I guess they are just linear regressions that were previously developed? I guess I am just recommending that you describe this more clearly.

We have reformulated the paragraph to clarify that the formulations of parameters pnit, pdenit and panox are estimated once using a vertically resolved model run in 1D and then these formulations are used in BAMHBI.

Line 698: There is an unnecessary space after “Then”

Corrected

Line 713: “Precipitation” should not have an “s” at the end

Corrected