Response to editor's comments on Czajka et al., 2025, Version 2:

We thank the editor and reviewer for their second reading of the manuscript, and their additional comments that will further improve the manuscript. Once again, reviewer comments are in black, and our author responses are in blue.

Thank you for your thorough responses and revision. The manuscript has been re-evaluated by one of our previous reviewers, who was very positive about the revision and requested clarification on two minor points related to the model settings. I have also reviewed the revision and provide the following technical comments for your consideration:

Thank you for your additional efforts on this manuscript!

L46: Suggest replacing "such as the input of acidic freshwater and nutrient runoff from precipitation" with "such as the inflow of acidic freshwater and the runoff of nutrients from precipitation" to better distinguish these as separate processes.

Done.

L176: "as model equations were developed from a study on diploid oysters " - please cite the study from which these model equations were derived.

Unfortunately, this study is not quite yet out in print but we reference our Ocean Sciences abstract: Rivest et al., 2020.

Table 3. May consider replacing identical values in simulations that match either the reference run or combined future run with the labels 'Reference' or 'Future.' This would reduce redundancy and help readers quickly identify which variables differ across simulations. For example, since simulation AtmCO2 mirrors the reference run in all variables except Ω_Ca, only Ω_Ca would display a numerical value, while other cells could simply state 'Reference.'

This is a fabulous idea in principle, but unfortunately is difficult to implement. The idea is clear for the example given (AtmCO2) but in most cases for the other simulations, we cannot simply use "future" or "reference". This is because we are referring to model results in this table, not model forcings. For example, the TMDL simulation certainly alters oxygen, but the effect is so small, it looks like the oxygen is the same as in the "reference" case. Although we like the idea in principle, we feel the best option is to retain the original version of Table 3.

Figure 5. Suggest increasing the thickness of the dashed line for better visibility.

Done.

In the caption, correct "ΩCa" to Ω_Ca (subscript formatting).

Done.

Figure 8. Suggest capping the y-axis maximum at 0.

Done.

Figure 9 caption: “change in bottom POC”

Done.

Figures 10 & 11 caption: “(a,d) AtmCO2, (b,e) Temp, and (c,f) TMDL”

Done.

Upon addressing these minor points, we will be pleased to accept your manuscript for publication. Thank you for choosing Biogeosciences to share your valuable work.

Thank you!

Response to reviewer’s comments on Czajka et al., 2025, Version 2:

The authors have thoughtfully addressed my previous comments, and I recommend acceptance after clarifying the following minor points:

Thank you for your additional time reviewing the manuscript.

1. In response to my earlier question about the C:N ratios for semi-labile and refractory DON, the authors state that these ratios "are allowed to freely evolve with time" (Lines 130–131). While I agree that these ratios should vary, I’m still confused about how this temporal variability is implemented in the model.

Thank you for this comment! We now realize that the confusion lies in the fact that we didn’t mention that we had DOC state variables as well as DON state variables. Because they are separate state variables, they evolve separately in time and their ratio varies. We have fixed this in the text by adding the bolded text below:

“small and large detrital nitrogen and carbon, semi-labile and refractory dissolved organic nitrogen **and carbon**, DIC, TA, and dissolved oxygen (O_2).”

2. The authors’ explanation of the one-way coupling between EcoOyster and ROMS-ECB is helpful. However, I’m still confused about the model being described as a "bioenergetics model" when its governing equations (provided in the Supplement) appear to be formulated in terms of mass balances rather than energy flows. Could you help me understand where the energy flow calculations come into play? Is that handled in a different part of the model that I'm missing?

This is a very good point. Because this is technically not a bioenergetics model, we have removed “bioenergetics” from the text and simply call this an “oyster growth model”.