Reply to reviewer 3:

General comments

de Froe et al., present a very interesting and novel study using mechanistic modelling to predict coral biomass, and respiration. The work is highly interdisciplinary, effectively integrating physical oceanography, ecophysiology, and ecology. To my knowledge, this is one of the first mechanistic models applied to a deep-sea benthic species, and among the first to incorporate feedback loops between environmental and biological processes in deep-sea ecological modelling. The study is therefore both original and of high scientific quality. The materials and methods are clearly described and concise, and the authors demonstrate a strong understanding of coral feeding biology and ecophysiology, which are well integrated into the model. Model limitations are also well explained.

I suggest that the authors emphasize the innovative aspects of their work more strongly. A brief paragraph in the introduction about the use of mechanistic models in deep-sea benthic ecology would help set the context for non deep-sea specialists. Additionally, some key advantages of the approach could be highlighted further. For example, this mechanistic model can estimate parameters that are difficult to obtain with statistical modelling such as species distribution models (e.g., biomass, respiration), especially in data-limited environments such as the deep sea. Statistical models often require extensive datasets and in the case of deep-sea benthic species they are typically limited to presence/absence or abundance data, with very few exceptions, such as the models used for validating the present study. Emphasizing that this model integrates physiological and environmental information without requiring large-scale sampling, except for validation, would strengthen the paper's contribution and practical significance.

Thank you for your kind words and your review. Following your suggestions, we adjusted the introduction by adding a paragraph in the introduction that explains the main difference between mechanistic modelling and statistical modelling approaches, and we also include a few sentences about how mechanistic modelling can be beneficial for deep-sea ecology in general.

In the discussion, section 3.4, we added a few sentences that highlighted the key advantages of mechanistic models for deep-sear research and cold-water coral research. Thereby mentioning that mechanistic models integrate physiological and environmental data. See also the reply on the general comments of reviewer 2. ##

Specific comments

Line 22: A comma after species would help the flow of this sentence.

Done, thank you.

Lines 32-33: Consider moving this sentence up, as it presents the main finding. The authors can then note that it aligns with previous model predictions.

Agree, moved the sentence before the comparison.

Lines 86-92: These lines focus mostly on POC concentration, but the connection to stressors such as acidification or temperature is not straightforward. I suggest adding 1–2 sentences emphasizing the importance of feeding in CWC ecology (e.g., ability to rapidly exploit food pulses whenever they occur, occurrence of seasonal cycles of growth and reproduction that correlate with food availability), and conclude by noting that feeding has been shown to influence responses to climate stressors (e.g., Büscher et al., 2017).

J. V. Büscher, A. U. Form, U. Riebesell, Frontiers in Marine Science. 4, 101 (2017).

Thank you for this remark. In this paragraph we tried to explain that the interaction between a CWC reef and the surrounding environment (food uptake, altering currents) is vital in predicting CWC distribution and we tried to introduce the topic of scale-dependent feedback. Indeed, other environmental conditions also play a role in determining the spatial distribution of CWCs, and there are mixed effect present. In the following paragraph we give an overview of all the different effect studies, and physiological experiments that were conducted with CWC reefs, and of which we can use the data for our model.

We added a sentence that food flux and availability is one of the most important predictors of coral growth, with the suggested reference ##

Lines 113-115: I assume that the strong regional contrasts refer to heterogeneity in geomorphology/terrain. Please rephrase.

Thank you, adapted accordingly. These sentences now read as follows:

The CWC mounds and ridges on the south-eastern (SE) slope of Rockall Bank (northeast Atlantic Ocean) provide an excellent study site to develop a mechanistic model of CWC biomass and respiration in relation to local hydrodynamics and food supply. This area shows strong geomorphic heterogeneity with numerous CWC mounds between 500-1000 m depth which are surrounded by sediments. The CWC mounds in this region have been studied extensively for several decades (e.g., de Froe et al., 2022; van Haren et al., 2014; Kenyon et al., 2003; Mienis et al., 2006). These mounds are formed by the framework building CWC species *Desmophyllum pertusum* (previously known as *Lophelia pertusa*, Addamo et al., 2016) and *Madrepora oculata*, for which a relatively large amount of physiological data is available.

Lines 129: I suggest highlighting that these variables are rarely predicted by statistical models, in the discussion this is an important contribution.

Thank you, we adapted the sentence to the following:

We specifically aim to predict CWC biomass and quantify CWC and benthic respiration, parameters which are difficult to predict with statistical spatial distribution models.

We emphasized this also in the discussion, see reply on general remarks.

Lines 193-195: I think that an "a" should be added to the reference (Soetaert et al. 2016a). In the work by Soetaert et al, it is mentioned that POC is influenced by passive sinking, hydrodynamic transport, and biological degradation. The results of this study highlight downwelling events often transferring POC to mounds. In these lines, the authors mention advective and passive transport,

which may lead non-technical readers to think that upwelling/downwelling were not considered. Please clarify this for non-specialists.

The Soetaert et al., 2016a was a reference error. There is only one Soetaert et al. 2016 cited in the paper. We have corrected this now. We changed the text here now to:

A detailed description of the organic matter transport model can be found in Soetaert et al. (2016). In this model, POC transport is simulated in the model domain by a combination of advective transport, passive sinking, and biological degradation. As in Soetaert et al. (2016), advective transport is driven by horizontal currents (*u*,*v*) and vertical or upward/downward currents (*w*).

Line 243: The parameter mCWC does not seem to be standardized to coral size/weight or area. Confirm whether it should be expressed per m^2.

Indeed, respiration is here expressed in % biomass respired per day, which is sometimes used in mechanistic models. The CWCs have a base respiration rate of 0,0035 d^{-1}, so every day 0,35% of its biomass is respired.

Lines 329-332: Can the authors provide the rationale for these decisions (e.g. dividing initial biomass by 3, coral growth/decline enhancement factor of 12)?

The specific values are chosen iteratively throughout the modelling process. The value of dividing the initial biomass by three was chosen due to:

- The initial biomass of CWC biomass was too overestimated in each grid cell of the model, and dividing by three would still overestimate the CWC biomass in the model domain, but speed up computations considerably.
- If we divided by two, this would also be the case, but computations would just take longer to end up with the same results.
- If we dived by ten, some areas would be underestimated, and so in those areas CWCs would start to grow.
- To synchronize the effect of overestimation throughout the model domain we decided to divide the initial biomass by three.

The coral growth/decline enhancement factor of 12 was chosen due to:

- Corals would grow/decline considerably slower than the changes in POC transport/hydrodynamics.
- We chose 12 for two reasons: 1. As we have one month of hydrodynamic output, a factor 12 would represent one year of growth/decline, which makes it a bit more intuitive to compare speeds. 2. Enhancing the coral growth/decline also affects daily dynamics of corals in the model. Some areas would, for instance, show CWC growth over the whole month period, but would CWCs would decrease for some days/weeks due to lack of POC influxs. Then at spring tide, when POC flux would increase (see video supplement), the CWCs would grow again. By using an enhancement factor, this growth/decline is amplified and if much larger factor is taken (say factor 100), then fluctuations of CWCs would be very high over daily time scales. This also affects then the POC dynamics in the bottom layer, and therefore the

mechanics of the model. A factor of twelve proved to speed up computations, but would not affect the dynamics of the model. This is done iteratively, so other factors were used in earlier versions/in the development of the model.

We have adapted the text to the following to accommodate this comment:

First, we divided the initial CWC biomass from step 2 by three and used that as initial benthic biomass. The specific value of dividing by three was chosen iteratively during the model development process. Dividing by three would speed up computations considerably but would not alter the initial conditions retrieved from step 1 and step 2. The new initial CWC biomass would still be higher than the CWC equilibrium biomass throughout the model domain, as CWC biomass was still declining in consecutive model runs. Second, we ran the coupled model for a total of five consecutive months with a coral growth/decline enhancement factor of twelve. This is a method that is also used in morphological and sediment transport modelling approaches (Ranasinghe et al., 2011). The enhancement factor of twelve was chosen iteratively during the modelling development process and was chosen due to the following reasons: 1) as we used one month of hydrodynamic output, an enhancement factor of twelve would compare with one year of running the model, which makes comparing modelling output with/without enhancement factor more intuitive, and 2) using a much larger factor (i.e., enhancement factor of 100), would change the short-timescale dynamics of CWC growth and thereby alter the mechanics of the model. For example, using an enhancement factor of 100 would in some areas of the model domain cause strong daily CWC biomass fluctuations, which would affect the bottom water POC concentrations as well. A factor of twelve proved to speed up the computations but would not affect the mechanics of the model. After running the model for five consecutive months with the enhancement factor, two months were run without the growth- enhancement to arrive at the final output, in which $dCWC_b / dt$ was close to steady-state.

Line 396: Should this be “local” rather than “regional”? How easily could the model be scaled up to a larger area?

Thank you, I guess it depends on your definition of local or regional, if you have sufficient computer power, you could scale up this model to larger areas. Changed the word to local.

Line 403: “shows” instead of “show.”

Thank you, corrected.

Line 412-413: This sentence is a bit confusing. It seems to refer to environmental factors that were included in the model, but that long-term variations in these factors where not considered, potentially leading to discrepancies between observed and actual CWC cover. The reference to respiration may confuse the reader, please rephrase.

Thank you, we see what you mean.

This section discusses the fact that our modelled CWC biomass agrees well with dead coral framework cover, and the causes behind this. We adjusted the text to avoid confusion, and removed the reference to respiration. The new text now reads as follows:

The good agreement between our modelled CWC biomass and observed combined cover of living CWCs and dead coral framework could have several causes: 1) the different timescales between our model and CWC reef dynamics. Our model is based on one-month of hydrodynamic model output, organic matter transport and CWC physiology. Although CWC biomass declines in the model if insufficient food is provided, mortality or longevity of CWCs is not included. This means that, in our model, if (food) conditions remain favorable, CWCs can exist indefinitely. Although generally little is known on the temporal and spatial dynamics between living CWCs and dead coral framework on a reef, CWCs would die-off at one point in time and become dead coral framework. 2) It could be that CWCs have grown in the past in the areas where we predict high CWC biomass, but which have died-off due to conditions that were not included in our model (i.e., infection, predation, temperature, ocean acidification). Therefore, it is reasonable to find dead coral framework where high CWC biomass is predicted in our model. Previous work on the same video transects also shows that cover of live corals and dead coral framework are highly correlated (van der Kaaden et al., 2023). The presence of dead coral framework on the mounds indicates areas that were favorable for CWC growth in the past. It would be interesting to expand our CWC biomass model with dead coral framework as a state variable, where dead coral framework is built up with a mortality rule (as in Hennige et al. 2021). This would especially be interesting as dead coral framework affects bottom hydrodynamics (Bartzke et al., 2021; Corbera et al., 2022) and baffles sediment (Wang et al., 2021), and alters nutrient cycling (Maier et al., 2021).

Line 237-239: Considering that the simulation was run for a month, how was benthic respiration extrapolated to the whole year? Did the authors assume that POC is steady throughout the year? I also suggest dividing this sentence in two, to improve flow.

We assume that the reviewer refers to lines 437 – 439 instead of 237 – 239.

Yes, in this case we assumed benthic respiration in the model is representative for the whole year. So it would be the integrated respiration of the whole month times twelve. There is no data available on the seasonal fluctuations of CWC benthic respiration. The numbers of de Clippele et al., 2021 are also based on benthic respirtation values measured in spring, so with the same kind of conditions, and extrapolated to the year. So extrapolation was done in a similar matter.

We changed the text to the following:

Extrapolating our modelled benthic respiration to a whole year, the seafloor of the model domain would respire in total 104,845 tonnes C per year. CWCs alone were responsible for 11,260 tonnes C yr^{-1} of benthic respiration, or 10.7% of the total benthic respiration in the model domain.

Line 239: The reference to 5,763-9,260 tones is not standardized to area, was the referenced study at exactly the same domain?

We assume that the reviewer refers to line 439.

Yes indeed, we added this info by changing the sentence to the following:

Our predicted CWC-based respiration was comparable to, but at the upper end of the carbon turnover estimate of 5,763 to 9,260 tonnes C yr^{-1} which was predicted from a CWC suitable habitat model of exact the same area (De Clippele et al., 2021; Rengstorf et al., 2014).

Line 440: Specify whether this reference refers to “a” or “b.”

Thank you, done.

Lines 572–575: On a first read, I found the first sentence slightly contradictory to Fig. A and the fact that this study used a POC model which has highlighted the interaction between tidal currents and CWC-formed mounds in the past (Soetaert et al. 2016a). In the second sentence I realized that this part refers to bottom currents and processes that likely occur in smaller scales. I suggest authors to rephrase these two sentences to avoid confusion.

Thank you, indeed confusing. We rephrased the sentence to the following:

The interaction between bottom hydrodynamics and CWC framework was not included in the model as it would require resolving spatial scales much smaller than 250 m (horizontal resolution of one grid cell in this model = 250 m^2).

Figures

Figure 1: A: Adding labels on land shapes will help readers that are not familiar with the area. A short note on the relationship between parent grid, child grid and model domain would also help. These are mentioned in the text, but their relationship is a bit unclear.

Thank you, will be adapted accordingly.

Figure 3: Please add latitude and longitude labels.

These will be added##

Figure 7B: Consider comparing modelled versus observed depths directly in panel B for easier interpretation.

This will be added##

Figure 9: For panel A, I suggest using unfilled rectangles to keep the underlying data visible. For panel C, I believe that a simple correlation plot between measured and modelled values (keeping the colour palette for sand and coral areas) would be more informative.

For A): this will be adjusted. ##

For C) we plotted this correlation, but due to the differences in the spatial resolution, the relationship is not 1:1 for each side. See plot below. We think it is more useful to keep comparison on the scale of the whole model domain, rather than a 1:1 regression plot.

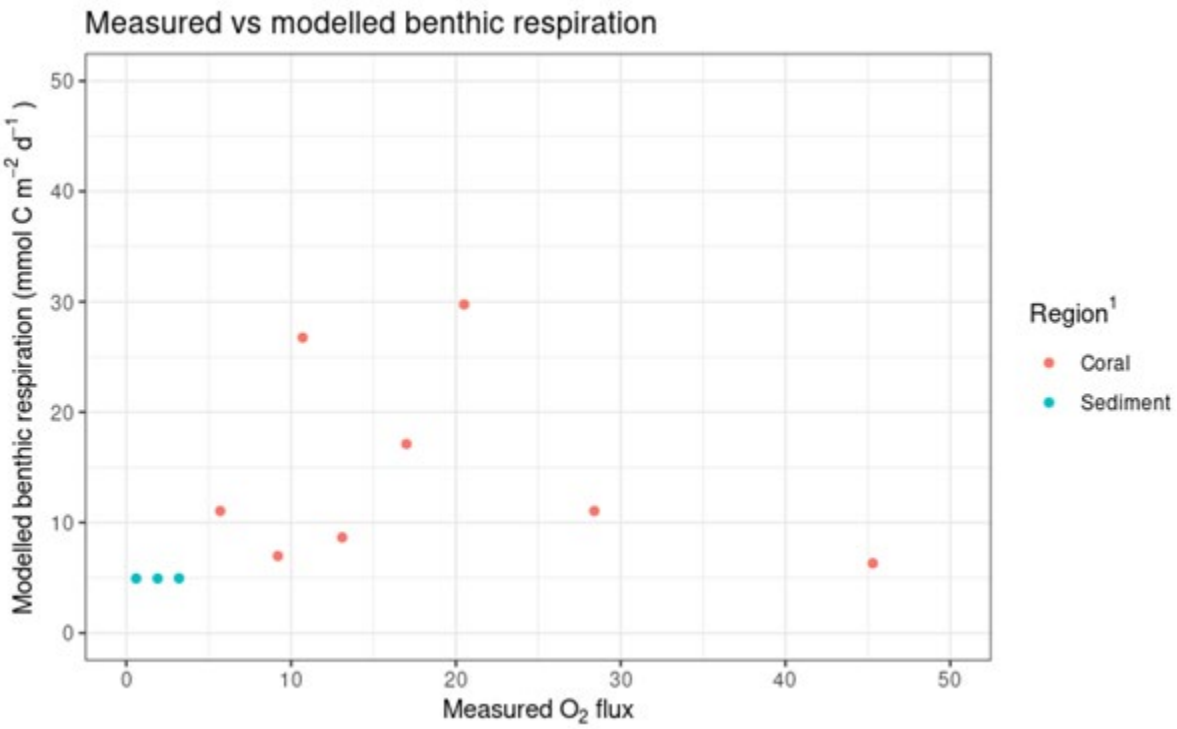

Figure 10: Consider overlaying panel E with panels A and C as an inset, rather than showing it separately. Similarly, clarify the locations of panels A and B. Standardizing the format for all figures that refer to the model area (e.g. Figure 1C, 3, 4A,B), with similar contour line style, colour palettes etc. might help the reader follow the figures easier.

Will be adjusted accordingly, thank you for the suggestions. ##