

Review of the manuscript Towards “The impact of large-scale macroalgae cultivation and harvesting strategies on the marine carbon dioxide removal efficacy and marine biogeochemistry”, by Anugerahanti et al, submitted to EGU sphere (2025-5360).

Manuscript overview

The manuscript details results from a modelling study whereby a marine hydro-biogeochemical model (simulation biology up to zooplankton) is used to test different global strategies of macroalgae farming with the aim to sink off the harvest and so increase carbon sequestration. The study shows that carbon removal up to the required level in order to keep global temperature increase within the 2 degree Celsius limit is possible this way as a first rough estimate. They conclude that it is however not worth the effort and risk to the biological carbon pump already operating in the oceans. Model deficiencies are well discussed in the manuscript.

Review overview

The manuscript is generally well written (some issues remaining with singular/plural and a too enthusiastic use of comma's), and presents a good overview of the performed global modelling scenarios. In general, model response is what is expected given basic ecosystem functioning (i.e. competition for nutrients and thus primary production impact, standard remineralisation of seabed material). As such, the presented scenarios do not really represent additional carbon removal as part of the carbon now counted as removed would otherwise be stored elsewhere through the ecosystem, possibly long-term. Without all required nutrient addition (only Fe was added) an impact on primary production was to be expected. Deep sea oxygen is also studied but without extensive bacterial or benthic representation this remains a first estimate of impact again along the line of what was to be expected. The authors make this very clear in the text though.

Overall, I had the impression that the authors were mainly trying to show just how large the required scale of seaweed cultivation needs to be if it aims to be the sole effective mechanism to reduce carbon from the system in order to mitigate climate change. Their answer is that it is simply not do-able on this scale (14% of the ocean surface) as the food web impacts would be severe. But of course it is not the only mechanism and application on this scale is not viable, even in this first estimate with a simple model approach. As such, the presented work has some merit.

The authors show that macroalgae farming should be concentrated in nutrient rich regions (on which global fisheries depend) and that, without considering realistic no-harvest loss, the southern ocean is the best place for it (some of the roughest ocean conditions on the planet and very expensive to get to). Long term carbon removal is only considered from the macroalgae sinking point of view without taking into account the carbon emissions needed to operate such a scheme or the impact on biodiversity and

natural marine food web carbon storage. And still their conclusion is that this is not a good idea.

I do miss observations on other methods of marine carbon removal (though alkalinity enhancement is mentioned), like soil storage in coastal areas which generally performs a double benefit function (e.g. coastal protection) which I do not see here. Indeed, oceanic cultivation impacts the global ecosystem though its use of necessary but limited nutrients, whereas coastal cultivation could help mitigate eutrophication issues. The included comparison to coastal cultivation provides a link to current efforts on a much smaller and manageable scale, and invites for a discussion on alternatives. It would therefore be good if the authors use this work to emphasize that a diverse array of options is needed if mCDR is to be effective in combatting climate change, and that much better options exist than using 14% of the global ocean to farm macroalgae.

We thank the Reviewer for their careful constructive comments and their recommendation for minor revision.

We agree with the reviewer's broader point to add coastal macroalgae cultivation, which can also mitigate eutrophication and coastal protection. The absence of shelf seas features such as riverine nutrient inputs, shallow water physical processes (e.g. tides) and a well-developed seafloor ecosystem submodel preclude MEDUSA from successfully representing coastal cultivation of macroalgae.

However, we have added some remarks on the into the tracked changed manuscript in Section 4.3 line 480-485:

Natural climate solutions, such as saltmarshes, mangroves, and seagrass ecosystems or coastal macroalgal cultivation, can deliver both carbon sequestration and a range of co-benefits, including coastal protection, nutrient retention, and biodiversity support (Mecreadie et al., 2021, Pessarodona et al., 2023). These systems provide relatively direct and more verifiable carbon storage through sediment burial, while also helping mitigate eutrophication. Such coastal approaches may offer more tractable mCDR deployment, even though at smaller scale. These findings reinforce that no single approach is likely to deliver the required scale of carbon removal.

Detailed Comments

1. Line 53: not sure what is meant by "natural phytoplankton", is there any other type?

We have changed 'natural phytoplankton' to 'phytoplankton' to avoid a redundant qualifier throughout.

2. Line 75: I do feel that question 2 has not been answered in the manuscript as basically the model lacks the complexity to do so. The same holds for question 3. In both cases, only a very limited first estimate answer is provided.

We agree that we only provide first-order estimates, which is why we revised hypotheses 2 and 3 to address Reviewer 1's comments in the previous review iteration. Hypotheses 2 and 3 are answered in Results section 3.3, and we have clarified that our results are indicative estimates of the potential biogeochemical response in both the Results section (lines 328-330 in the revised manuscript first review iteration) ...

...leading to pronounced changes in the phytoplankton NPP. Note that these changes reflect competition for nutrients and light as represented in the model and provide an indicative estimate of the potential biogeochemical response, rather than a detailed prediction of realised ecosystem behaviour...

... And in the Discussion section, especially regarding the oxygen loss (lines 399-401 in the revised manuscript first review iteration):

We note that these results are not a detailed characterisation of benthic ecosystem impacts. MEDUSA's representation is intentionally simple, and a full assessment of seafloor community responses, including benthic respiration dynamics and faunal impacts, would require dedicated benthic ecological models. However, these results may be used to identify large-scale signals and patterns that would motivate future benthic studies.

We have also added a few sentences with regards to the lack of phytoplankton functional types in our model in the new tracked changed manuscript in the Discussion section 4.1.:

... zooplankton grazing is explicitly represented). Additionally, MEDUSA only represents two phytoplankton functional types, which do not fully capture the diversity of nutrient acquisition strategies and ecological responses present in natural systems... (lines 385-386)

...as illustrated in the High Loss experiment (see Fig 3). A more diverse representation of phytoplankton functional types could also alter the magnitude and spatial pattern of NPP responses. (lines 388-389)

3. Line 124: please define "modest". I would not expect the temperature rise over the 1976-2024 period to be modest, certainly not in coastal areas or the arctic.

The word 'modest' in this context was intended to refer to the temperature change experienced in the simulation period is relatively small compared to some climate scenarios used (e.g. SSP4 and SSP5). We have revised the text to make this distinction clear (lines 125-126 in the tracked changed manuscript):

The temperature increase within this simulated period is relatively small compared to late 21st-century warming used in other CDR studies (e.g. SSP585 in Palmieri & Yool, 2024) and therefore shows limited effects

4. Table 1: Eucheuma is listed as a 3.0 degree temperature range, which is at odds with line 170.

Thank you for noticing this is a mistake, the Eucheuma temperature range is supposed to be between 22.5-27.5, we have corrected this in the newly revised manuscript.

5. Line 168: I find the use of T_{2,opt} confusing as it seems like a square power application.

We have changed them to T_{1,opt} and T_{2,opt} to avoid confusion (lines 169-170).

6. Line 380: the presented references are not alphabetically or chronologically ordered.

We have changed citations to chronological order throughout (line 382).

7. Lines 411-414: please rephrase, this is difficult to read.

We have revised sentence to (lines 417-423 in tracked changed manuscript):

Higher thresholds allow more macroalgae biomass to accumulate ahead of harvesting, leading to an increase in macroalgal NPP. However, as more of this NPP is lost to non-harvesting losses in the model, the associated CDR efficiency is actually lower compared to other harvest experiments (Fig. 3). Further, by increasing the quantity of harvested material transferred to depth, higher thresholds additionally cause greater seafloor oxygen depletion, as well as a larger reduction in the NPP of phytoplankton (Table 3, Fig. 5k).

8. Lines 418-420: please rephrase, this is grammatically incorrect.

We have changed this to (lines 427-429 in the tracked changed manuscript):

In farmed macroalgae, non-harvesting loss due to falloff and frond erosion can exceed 10% of the growth rate (Zhang et al., 2012). This simulation explores how CO₂ flux and ocean biogeochemistry are affected when non-harvesting losses consume more of the macroalgal biomass.

9. Line 451: “enhances”

Changed to “enhances” (line 460).

10. Lines 455-458: needs rewriting, “whereby the Equatorial Pacific ... shows ... , while the Southern Ocean (with a deeper mixed layer depth) keeps ... ”. Just how many comma’s can one sentence have? There are 5 here, which makes for difficult reading.

We have changed this to (lines 464-468 in the tracked changed manuscript):

This explains the regional variation in CDR efficiency shown in Figure 4c. In the Equatorial Pacific, relatively high macroalgal NPP is associated with lower CDR efficiency. In contrast, the deeper mixed layer in the Southern Ocean keeps low-DIC water in contact with the atmosphere for longer, resulting in higher efficiency.

11. Line 468: “these studies highlight ... reporting and verification”

We have revised this sentence as suggested (line 478).

12. Line 478: this is a first mention that riverine nutrient inputs are absent. Do the authors mean that these nutrient sources are not included in the model at all? If so I would like to see this mentioned earlier in the text.

Yes, there are no riverine nutrient inputs in the model. We mentioned this to Section 2.1 in the tracked changed manuscript lines 118-119:

... For other plankton tracers we use uniform nominal values. Riverine nutrient inputs are not included in this configuration, so nutrient supply is limited to oceanic initial conditions and internal cycling.

13. Line 510: “When macroalgae are harvested”

We have revised this sentence as suggested (line 532).

14. Conclusions: given the model deficiencies I find these a bit strong. And I would prefer to see a more general conclusion that even in this first approach, the results do not show a viable option on the necessary scale and that efforts must be spread over different initiatives.

On reflection, we would agree that these conclusions can appear a bit strong. We have added a short framing paragraph to better contextualise them (lines 515-522):

This study provides a global-scale assessment of macroalgae cultivation within a coupled ocean biogeochemical model. Our simulations highlight the potential for carbon sequestration, while also revealing significant trade-offs, as well as sensitivities to model assumptions and different cultivation strategies. Given the uncertainties and model limitations, the results presented here should be interpreted as a first-order approximation of the biogeochemical consequences of large-scale macroalgae cultivation. Overall, our findings suggest that solely relying on large-scale macroalgae cultivation is unlikely to provide a viable mCDR solution, and that mitigation efforts will likely need to be distributed across different mCDR strategies, such as OAE and coastal ecosystem restoration. The key findings of this study are summarised as follows: ...