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 **EGUsphere** (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.

Some more detailed comments are provided below.

Recommendation

Minor revision

Detailed Comments

1. Line 53: not sure what is meant by "natural phytoplankton", is there any other type?
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.
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.
4. Table 1: Eucheuma is listed as a 3.0 degree temperature range, which is at odds with line 170.
5. Line 168: I find the use of T^2_{opt} confusing as it seems like a square power application.
6. Line 380: the presented references are not alphabetically or chronologically ordered.
7. Lines 411-414: please rephrase, this is difficult to read.
8. Lines 418-420: please rephrase, this is grammatically incorrect.
9. Line 451: "enhances"
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.
11. Line 468: "these studies highlight … reporting and verification"
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.
13. Line 510: "When macroalgae are harvested"
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.