nature portfolio

Peer Review File

Large global variations in the carbon dioxide removal potential of seaweed farming due to biophysical constraints

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

15th Nov 22

Dear Dr Arzeno-Soltero,

Your manuscript titled "Biophysical potential and uncertainties of global seaweed farming" has now been seen by 4 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

All four reviewers find issues with the model inputs that could be improved or clarified, I also find some issues with it. However, these should all be addressable in review. In particular, please pay attention to the comments from the reviewers regarding clarifying, improving, or justifying the model inputs. I agree with these, and also consider there is some concern around the estimates of DBL thicknesses within densely packed seaweed farms, these may be orders of magnitude larger than those estimated using models of mainstream seawater velocities. However, I understand this is complex, so perhaps a simple caveat is required here for the reader, as this would further support the author's conclusions in reality. Also, please pay attention to Reviewer #3's comments around framing. This is particularly important here.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Christopher Cornwall, PhD
Editorial Board Member
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Clare Davis, PhD
Senior Editor
Communications Earth & Environment

Joe Aslin, PhD
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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at <a

href="http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf">http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
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- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

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If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This manuscript presents data from modelling of carbon dioxide removal (CDR) of seaweed aquaculture, including the potential of offshore as well as coastal systems for CDR. The authors propose that seaweed farming at a scale of 1 Gt C/year would require ~ 1 million km2 of productive waters in the economic exclusion zones of the Pacific Ocean. The authors also suggest that their model indicates major uncertainties in estimates of seaweed aquaculture productivity so that there are significant problems in accurately estimating scaled up harvest yields from seaweed aquaculture for CDR.

There is considerable interest in the literature about using seaweeds as a form of ‘blue carbon’ and as a possible CO2 remediation strategy. However, there are many challenges

facing the potential application of macroalgae in blue carbon solutions, particularly in terms of scaling up these to an extent that might significantly influence CO2 concentrations. The analysis provided by this manuscript is therefore a welcome addition to the debate and provides novel insights into the constraints and limitations in determining the potential contribution of seaweed culture as a CDR mechanism.

As stated above, there is a considerable amount of information on seaweeds as a blue carbon solution and this is recognized reasonably well in the manuscript, though I was a little surprised to find the analysis of Sondak et al (2016) not included (Sondak FA, Ang PO, Beardall J and 17 others (2016) Carbon dioxide mitigation potential of seaweed aquaculture beds (SABs). Journal of Applied Phycology doi:10.1007/s10811-016-1022-1). While I am not a modeler, I found the analytical approach logical and the discussion and conclusions convincing.

The only weaknesses I felt about the manuscript was the comparison between modelled biomass and actual biomass in field studies (supplementary figures 5-8) used to test the validity of the model. Here the data are given as box plots. Differences (or similarities) in means between modelled and field values could mask larger discrepancies between the model and field data (e.g. values for field data could be 1,2,3,4,5 and the modeled values 2,5, 4, 1, 3, with the same mean but very poor correlation). Would it not have been better to calculate biomass standing stock values based on the field parameters in each study and then do regressions of modelled vs field values? Also, I rather thought that productivity (tC/km2/y) would be a better basis for such comparisons than using biomass as g DW/m2.

I was also puzzled by the use of 0.2 d-1 for growth rate of all algae used in the modeling, given that growth rates can vary considerably (see e.g. supplementary info Table 1 in Xiao et al 2019. cited reference # 78, 10.3389/fmars.2019.00618).

These points aside, this manuscript will certainly contribute to the debate about the role of seaweed as a blue carbon CDR strategy.

John Beardall

Reviewer #2 (Remarks to the Author):

Arzeno-Soltero et al. tackle the difficult task of estimating the area of the world's oceans needed to produce and harvest 1 Gt of seaweed carbon per year from open ocean farms. Rather than take a back-of-the-envelope approach, the authors apply a dynamic growth model (informed by spatially resolved environmental data and intrinsic biological parameters) to four major groups of currently farmed seaweeds to estimate harvestable seaweed carbon for the global offshore ocean for limiting and unlimiting nitrogen conditions. The model assumptions were clearly stated, appeared reasonable, and the authors provided spatially explicit estimates of error in harvestable carbon associated with different abiotic and biotic variables. The paper was well written and organized and I really enjoyed reading it as it is the most scholarly and comprehensive article that I have seen to date on this rapidly emerging

topic. The findings are important as they provide realistic and scientifically defensible expectations for open ocean seaweed farming as a significant means of carbon dioxide removal. I applaud the authors for their thorough efforts on what I believe will be a highly valued and widely read paper.

Below I offer a few minor comments for the authors to consider when revising their paper. I did not comment on the Online Methods detailing the specifics of the model as many of the details were outside my scope of knowledge. I defer to others with more expertise in this area to evaluate this portion of the manuscript.

Line 21. Suggest replacing "bottom-up limitation mechanisms" with "growth limiting mechanisms" to avoid unnecessary jargon and improve clarity. Insert "offshore" before “cultivation” to clarify that this study does not consider onshore or nearshore farming of seaweeds, as stated in the last paragraph of this section

Line 27. Avoid using a noun (i.e., macroalgae) to modify another noun (i.e., growth). Suggest replacing “macroalgae” with “macroalgal” (adjective) here and elsewhere where macroalgae is used as an adjective.

Line 30. Table 1. N uptake rates often differ for different forms of nitrogen. Supp Figure 1 suggests nitrate is the form of N considered in G-MACMODS. It might be worth noting that other forms of nitrogen (e.g., ammonium, urea and other forms of DON) are taken up by seaweeds and used to support growth in areas or times of low nitrate. Also from what I could tell the max growth rates in Table 1 (0.2/day) were largely based on lab studies of isolated individuals (often juveniles) or portions of individuals (e.g. individual blades or fronds), which likely over-estimate the growth rates of populations of plants competing for light and other resources on an ocean farm.

Line 34. if “nutrient” specifically refers to nitrate then replace "nutrients" with "nitrate" here and elsewhere in the text and figures to reduce confusion.

Line 39-40. here would be a good place to clarify that G-MACMODS also assumes that nitrate is the primary (only?) form of N used by seaweeds.

Lines 49-50. suggest moving this sentence to the preceding paragraph when first describing the details of G-MACMODS as it provides valuable context to Table 1 (as reference in this paragraph) and to the intended scope of the analyses.

Line 70. – Caption for Figure 1 should clarify that annual harvest yields are based on output from G-MACMODS.

Lines 75-76. Is this a reasonable expectation given the difference between growth in phytoplankton reflects the rapid turnover of single celled individuals while growth in seaweeds results from increases in the size of a comparatively steady state population of multicellular individuals in which competition for light within and among individuals can limit growth before nutrients become limiting? A brief discussion or acknowledgment of the major differences between phytoplankton and seaweed growth seem warranted given the

comparisons between these two groups in the text and Fig.1.

Lines 83-84. I question whether this result amounts to sufficient evidence for confirmation. Might an alternative explanation be that estimates of seaweed growth used in G-MACMODS are unrealistically high? In any case, I suggest downplaying “confirmation” (which implies absolute confidence) unless stronger evidence is provided.

Lines 97-99. I think these results are really interesting as they are similar to the general patterns of abundance observed in natural populations (brown algae tend to dominate in temperate regions and red algae in the tropics). It has been suggested that the prevalence of reds in the tropics is due to them having higher concentrations of halogenated metabolites that deter herbivory and biofouling, but may come at costs to growth. Perhaps this is something to mention?

Line 105. – Caption for Figure 2 should clarify that annual harvest yields are based on output from G-MACMODS.

Lines 111-128. The uncertainty analysis is a really valuable addition to the paper and is very well presented. The maps in Fig 3a and b provide a quick view of the magnitude of error for different regions under the two nutrient scenarios, while Figs 3g and h (which were not labeled in the pdf that I downloaded) nicely summarize the relative importance of the biological parameters in Table 1 for different levels of harvest. That mortality rate was the only variable that was relatively unaffected by annual harvest rate and nutrient scenario might be something that the authors want to highlight. The probability functions in Figs. 3c and d were not mentioned in the text. The results of these figures should either be interpreted or deleted.

Line 161. Is there a reason that DOC export is not included with POC export as a loss term?

Reviewer #3 (Remarks to the Author):

General comments:

The abstract (and the general theme of the introduction/the manuscript) allude to the paper being about CDR, which is not what the study examines. The authors do state this on line 164-165 for example and in other sections of the manuscript but this point is likely lost as most of the paper talks about harvested carbon in the context of CDR. The study estimates the global harvest potential of different seaweed groups based on a dynamic growth model and does not expressly look into CDR. As such, the framing of the manuscript this way can be potentially misleading. I suggest reframing the manuscript as what the title and study examines which is potential global harvest rates of seaweed farming and have a small discussion paragraph about CDR.

I have some reservations about the appropriateness of some of the data sources used for the model that I have mentioned in specific comments – some assumptions seem overly

simplistic and extrapolated from one or two studies in specific species to entire groups (or all groups) which in turn reduces the usefulness of predictions. Moreover, it is often not clear which particular data sources are used when just a reference is given.

Specific comments:
Line 1: would suggest changing “may” to “will” – this is pretty certain.

Line 27: can you put the actual number of simulations or the range if multiple

Line 39-50: I think a little bit more info (references) on the rationale for these choices is needed here. I agree nitrogen is likely to be the limiting nutrient a non-seaweed expert may need to have more information on why this is the case. It could also be useful to expand on how you came to the choice of the exact parameters for ambient and limited scenarios and some justification of these. I think its important to make it really clear as these nutrient conditions underpin the entire study.

52-53: perhaps a reference to why this would be unacceptable to ocean health – see:
Boyd PW, et al. 2022. Potential negative effects of ocean afforestation on offshore ecosystems. Nat Ecol Evol.

Line 75: I consider this a very broad assumption with no references to support it – suggest adding info and references.
Line 85-86: “but it might provide a reasonable estimate of the localized potential harvests of operations small enough in scale so as to not dramatically alter local nutrient budgets“
this point is made a couple of times in the manuscript but no rationale/justification for it or how small a scale operations need to be – suggest some extra info/rationale for this point where it is made throughout the manuscript.

Lines 111-128: I have not focused on the appropriateness of this technique to estimate uncertainty as it is outside my area of expertise.

Discussion and conclusion

My main comment in the discussion is the framing of it around CDR potential which is not what is examined here – only harvest potential. By just doing a back of the envelope conversion of biomass to amount of carbon harvested and then discussing this in the context of CDR it disregards the complexities and uncertainties around how growing seaweed biomass equates to actual removal of CO2 from the atmosphere. The authors definitely understand this as they state it in multiple places in the manuscript and include the caveats that the amount of harvested carbon does not equate to CDR. So I think clarification around why the framing of the discussion/manuscript around CDR is needed.

Online Methods

Here I have not focused my comments on the specifics/appropriateness of the modelling techniques as it is outside of my expertise and do not feel I can contribute useful/insightful comments. I have however focused on the logic/rationale/appropriateness of some of the

data sources and values used in the models.

Line 213:214: the assumption that carbon is 30% for all seaweed groups is overly simplistic and can drastically underestimate the amount of variation in the units of carbon harvested as it is not uncommon for seaweed carbon content to vary between 25-40 % or even greater within a group. I consider this needs to be accounted for if converting your biomass estimates to units of carbon.

Line 265 – 268: It isn't clear here how you chose the values for some of these parameters e.g. Bcap and Ug for each seaweed type as you only refer to table 1 and the references therein (often there are 2-3 for each group and it isn't clear how you chose a specific value). I suggest that this be clarified as presently unclear how growth rate is the same for all seaweed groups. I also find it very difficult to follow what data is used where as often a just citation of a reference with no details of what data was used e.g. line 268: "The power law in equation (8) was derived by re-fitting data from a comprehensive meta analysis78" – there is no information on how data was refit or what data in this reference.

Line 281: the light-limitation being informed by phytoplankton studies does not seem appropriate – seaweed and phytoplankton have drastically different light requirements – I would suggest that the light limitation needs to be informed from seaweed specific studies.

Line 285-290: here I have issue with the mortality from breakage due to waves. It appears due to the one citation that all breakage is based on the parameters from the reference 83 which is about mortality due to breakage in Gelidium sesquipedale (a red algae) in one location – this is then extrapolated to all seaweeds (e.g. large browns such as Sargassum, Saccharina) which are likely to have drastically different tolerances to wave energy. I consider this extrapolation to all groups and all locations around the world to be too simplistic.

Line 343-361: I think the comparison of the models against actual harvest data is very useful but the details surrounding how the comparisons were made need to be clearer. Comparing of medians and ranges seems how it has been done but no values are given, it is just stated that they are similar. I also question the appropriateness of including this information in the methods and not results?

Line 362-384: I really like the clear conditions explained for when seaweeds are harvested in the model.

Reviewer #4 (Remarks to the Author):

Dear authors,
I enjoyed reading your work and going over the online model and mapping tool. Your article is a thorough and important piece of work, promoting the existing set of seaweed cultivation models to a global scale without compromising the level of details.
I would recommend the acceptance of the manuscript, but still wrote a few small comments

that may help improve the paper:

Comments:
Introduction
Rows 3-5: The connection between the ocean as a natural CO2 sink and the interest in enhancing ocean carbon dioxide removal through seaweed farming is not clear. How much of the natural CO2 removal in the ocean is related to seaweed? Please explain.
Rows 6-8: Biofuel production from seaweed is under research for decades and is still not commercial. Better not to mention it as a first potential use and to be more cautious about it's real potential. In addition, I believe ecosystem services may be provided even in a seaweed sinking project.

Global Seaweed Harvested Yields
Rows 79-80: "… which implies that seaweed might fix, at least, 3-6 times more carbon than current NCP" - I don't think this is correct. The different C:N ratios derive mostly from lower N contents in seaweed (mg N per g DW) rather than from higher C content (mg C per g DW). Please explain or revise this sentence.
Rows 96-97: "…perhaps reflecting the advantage of having higher maximum uptake rates in regions with lower nutrient concentrations (Figs. 1f and 1g)." – I think for low nutrient concentrations the real advantage would be a low half saturation rate (i.e., Ks) rather then a higher maximum uptake rate.

Meiron Zollmann

Arzeno-Soltero et al. [COMMSENV-22-0880-T]

Response to Reviewers

The authors would like to thank the reviewers and the editor for their thoughtful feedback; the manuscript is much stronger thanks to their suggestions.

We would like to point out some major changes to the manuscript. While revising the manuscript to meet the reviewers' criteria, we stumbled upon an error in our nitrogen cell quota (Q) calculation; our old expression for dQ/dt was allowing seaweed to accumulate a small percentage of extra nitrogen. We have now fixed this error; our new equation for change in Q is as follows:

$$dQ = Vdt + Q\left(\frac{1}{1+\mu dt} - 1\right) - E(Q - Q_{min})dt$$

where V is the nitrogen uptake rate [μmol-N/(g-DW h)], and E is the fractional exudation rate [1/day] (equation 1 of the manuscript).

To account for this modification, we adjusted some model parameters to achieve better agreement between the model and literature-reported growth. The updates to model parameters include lowering the exudation rate to 1% per day and changing our values of B_{cap}, a parameter in the biomass crowding limitation function (equation 8 of the manuscript).

This does not affect the main message of the manuscript. Although, overall, we grow and harvest less macroalgal biomass than in the previous results, the macroalgal harvest yield in the most productive 1% of the EEZs remains unchanged. On the other hand, since we modified the value of the exudation rate (and its Monte Carlo bounds) as well as added a new parameter to account for uncertainty in the seaweed wave mortality (following feedback from a reviewer), the results from our random forest analysis are notably different (Figures 3g and 3h in the manuscript).

We have updated Figures 1-4 of the main manuscript as well as Figures 5-11 of the supplementary material. We have also added 4 tables to the supplementary material that provide details on the information considered in the model-field data comparisons.

What follows is our response to each of the reviewers' general and detailed comments. Figures that begin with an "R" refer to figures in this response, as opposed to figures in the manuscript. Changes in the manuscript have been written in red. For clarity, the reviewer's comments are written in black italics, while the authors' responses are in blue roman font.

Reviewer #1 (Remarks to the Author):

This manuscript presents data from modelling of carbon dioxide removal (CDR) of seaweed aquaculture, including the potential of offshore as well as coastal systems for CDR. The authors propose that seaweed farming at a scale of 1 Gt C/year would require ~ 1 million km2 of productive waters in the economic exclusion zones of the Pacific Ocean. The authors also suggest that their model indicates major uncertainties in estimates of seaweed aquaculture productivity so that there are significant problems in accurately estimating scaled up harvest yields from seaweed aquaculture for CDR.

There is considerable interest in the literature about using seaweeds as a form of 'blue carbon' and as a possible CO2 remediation strategy. However, there are many challenges facing the potential application of macroalgae in blue carbon solutions, particularly in terms of scaling up these to an extent that might significantly influence CO2 concentrations. The analysis provided by this manuscript is therefore a welcome addition to the debate and provides novel insights into the constraints and limitations in determining the potential contribution of seaweed culture as a CDR mechanism.

We thank Reviewer #1 for recognizing the relevance of our work. Reviewer #1's constructive feedback has significantly improved the quality of our manuscript.

As stated above, there is a considerable amount of information on seaweeds as a blue carbon solution and this is recognized reasonably well in the manuscript, though I was a little surprised to find the analysis of Sondak et al (2016) not included (Sondak FA, Ang PO, Beardall J and 17 others (2016) Carbon dioxide mitigation potential of seaweed aquaculture beds (SABs). Journal of Applied Phycology doi:10.1007/s10811-016-1022-1). While I am not a modeler, I found the analytical approach logical and the discussion and conclusions convincing.

We thank Reviewer #1 for pointing us to this relevant paper. We have included Sondak et al. (2016) as a reference at the end of lines 5-7 of our revised manuscript (ref. 18), which now read

"While concurrently providing ecosystem services[10] or advancing the bioremediation of coastal waters[11–14] seaweed farming could also enhance ocean carbon dioxide removal (CDR) by fixing carbon from the surface ocean into organic biomass, which can subsequently be sunk to the deep ocean, isolated from the atmosphere[15–18]."

The only weaknesses I felt about the manuscript was the comparison between modelled biomass and actual biomass in field studies (supplementary figures 5-8) used to test the validity of the model. Here the data are given as box plots. Differences (or similarities) in means between

modelled and field values could mask larger discrepancies between the model and field data (e.g. values for field data could be 1,2,3,4,5 and the modeled values 2,5, 4, 1, 3, with the same mean but very poor correlation). Would it not have been better to calculate biomass standing stock values based on the field parameters in each study and then do regressions of modelled vs field values? Also, I rather thought that productivity (tC/km2/y) would be a better basis for such comparisons than using biomass as g DW/m2.

We thank Reviewer #1 for their suggestion. We agree that a point-by-point comparison between field observations and modeling is a better measure of model performance than the box-plot range comparisons. Initially, we did not attempt this because we found that there were often insufficient details in the literature-reported yield studies to allow for direct comparisons with the model output (e.g. no nutrient concentrations reported or lack of location coordinates). However, in response to the reviewer's suggestion, we have re-run the model to match reported field conditions (when available from the corresponding literature sources) and compared model-predicted yield directly to each field estimate. A description of the model-field data comparison is now included in lines 360-414. Our validation figures are now presented as scatter plots in Supplementary Figures 5-8 with the corresponding information in Supplementary Tables 1-4.

I was also puzzled by the use of 0.2 d-1 for growth rate of all algae used in the modeling, given that growth rates can vary considerably (see e.g. supplementary info Table 1 in Xiao et al 2019. cited reference # 78, 10.3389/fmars.2019.00618).

We agree with Reviewer #1 that relative growth rates vary considerably. In our manuscript, we use 0.2 day^{-1} as a maximum growth rate, rather than a relative growth rate. Variability in environmental conditions (specifically temperature and irradiance) as well as the internal nitrogen cell quota (Q) and crowding dependence result in variable relative growth rates, as specified in equation (7) of the manuscript.

While Xiao et al. (2019) do include relative growth rates larger than 0.2 day^{-1}, these often occur under nutrient conditions that surpass oceanic nutrient concentrations. We settled on using a maximum growth rate of 0.2 day^{-1} by refitting the data in Xiao et al. (2019) after eliminating data points from publications where nutrient conditions exceeded 20 uM, as explained in lines 281-286 of the revised manuscript. We also include examples of growth rates from field experiments to support our choice of 0.2 day^{-1} (Table 1 of the manuscript), and we vary the maximum growth rate in our Monte Carlo runs between 0.1 day^{-1} and 0.3 day^{-1} to account for uncertainty (Table 1 in the manuscript).

We have rewritten the section that explains the growth equations for more clarity (Lines 273-286).

These points aside, this manuscript will certainly contribute to the debate about the role of seaweed as a blue carbon CDR strategy.

John Beardall

References:

Xiao, X., Agusti, S., Lin, F., Xu, C., Yu, Y., Pan, Y., ... & Duarte, C. M. (2019). Resource (light and nitrogen) and density-dependence of seaweed growth. *Frontiers in Marine Science*, 6, 618.

Reviewer #2 (Remarks to the Author):

Arzeno-Soltero et al. tackle the difficult task of estimating the area of the world's oceans needed to produce and harvest 1 Gt of seaweed carbon per year from open ocean farms. Rather than take a back-of-the-envelope approach, the authors apply a dynamic growth model (informed by spatially resolved environmental data and intrinsic biological parameters) to four major groups of currently farmed seaweeds to estimate harvestable seaweed carbon for the global offshore ocean for limiting and unlimiting nitrogen conditions. The model assumptions were clearly stated, appeared reasonable, and the authors provided spatially explicit estimates of error in harvestable carbon associated with different abiotic and biotic variables. The paper was well written and organized and I really enjoyed reading it as it is the most scholarly and comprehensive article that I have seen to date on this rapidly emerging topic. The findings are important as they provide realistic and scientifically defensible expectations for open ocean seaweed farming as a significant means of carbon dioxide removal. I applaud the authors for their thorough efforts on what I believe will be a highly valued and widely read paper.

We thank Reviewer # 2 for recognizing our effort to provide a meticulous analysis of seaweed farming potential across the world.

Below I offer a few minor comments for the authors to consider when revising their paper. I did not comment on the Online Methods detailing the specifics of the model as many of the details were outside my scope of knowledge. I defer to others with more expertise in this area to evaluate this portion of the manuscript.

Line 21. Suggest replacing "bottom-up limitation mechanisms" with "growth limiting mechanisms" to avoid unnecessary jargon and improve clarity. Insert "offshore" before "cultivation" to clarify that this study does not consider onshore or nearshore farming of seaweeds, as stated in the last paragraph of this section

We thank Reviewer #2 for that suggestion.

Lines 19 to 22 now read "Here we develop and use a global dynamic model of seaweed growth, the Global MacroAlgae Cultivation MODeling System (G-MACMODS), to analyze the potential of seaweed farming to produce Gt-scale biomass carbon under the assumptions of **growth-limiting** mechanisms. We focus on the **offshore** cultivation of four seaweed types and systematically test the sensitivity of seaweed harvested yield to a range of uncertain biophysical parameters.

Line 27. Avoid using a noun (i.e., macroalgae) to modify another noun (i.e., growth). Suggest replacing "macroalgae" with "macroalgal" (adjective) here and elsewhere where macroalgae is used as an adjective.

We thank Reviewer #2 for that suggestion. We have revised the manuscript to avoid using macroalgae as an adjective.

Line 30. Table 1. N uptake rates often differ for different forms of nitrogen. Supp Figure 1 suggests nitrate is the form of N considered in G-MACMODS. It might be worth noting that other forms of nitrogen (e.g., ammonium, urea and other forms of DON) are taken up by seaweeds and used to support growth in areas or times of low nitrate. Also from what I could tell the max growth rates in Table 1 (0.2/day) were largely based on lab studies of isolated individuals (often juveniles) or portions of individuals (e.g. individual blades or fronds), which likely over-estimate the growth rates of populations of plants competing for light and other resources on an ocean farm.

We agree with the reviewer that uptake rates differ for different forms of nitrogen. We have clarified in Table 1 that these uptake rates refer specifically to nitrate. In addition, we have lines 42 to 45 that read,

"G-MACMODS specifically incorporates nitrogen in the form of nitrate, although other forms of nitrogen (e.g. ammonium and urea) can contribute to seaweed growth in low-nitrate environments[37, 38]. Presuming that nitrogen is the only limiting nutrient is a potential shortcoming of the model framework, but we assume that other nutrient constraints (e.g. iron scarcity) can be overcome by farming practices."

We also agree that populations often compete for light and other resources in an ocean farm. The maximum growth rates (μ_{max}, 0.2/day) are a cap on the growth that we would see under ideal conditions. The relative growth rate (μ) is limited by crowding effects, internal nitrogen reserves, water temperature, and light. This comment has helped us realize that we do not adequately explain our growth equations, so we have rewritten that section of the Online Methods for more clarity (Lines 273-286]).

Line 34. if "nutrient" specifically refers to nitrate then replace "nutrients" with "nitrate" here and elsewhere in the text and figures to reduce confusion.

We thank the reviewer for that suggestion. We have replaced "nutrients" with "nitrate" in most places of our text. We have kept the word "nutrient" in sentences that speak about general nutrient availability or scarcity.

Line 39-40. here would be a good place to clarify that G-MACMODS also assumes that nitrate is the primary (only?) form of N used by seaweeds.

We thank the reviewer for the suggestion. As pointed out in a previous comment, we now have lines 42 to 45 that read,

"G-MACMODS specifically incorporates nitrogen in the form of nitrate, although other forms of nitrogen (e.g. ammonium and urea) can contribute to seaweed growth in low-nitrate environments[37, 38]. Presuming that nitrogen is the only limiting nutrient is a potential shortcoming of the model framework, but we assume that other nutrient constraints (e.g. iron scarcity) can be overcome by farming practices."

Lines 49-50. suggest moving this sentence to the preceding paragraph when first describing the details of G-MACMODS as it provides valuable context to Table 1 (as reference in this paragraph) and to the intended scope of the analyses.

We thank the reviewer for the suggestion. We have now moved the sentence "Our analysis focuses on offshore production, under the assumption that competing uses of nearshore environments will preclude dramatic expansion of nearshore cultivation activities." to lines 59-60 of the revised manuscript.

Line 70. – Caption for Figure 1 should clarify that annual harvest yields are based on output from G-MACMODS.

We thank the reviewer for the suggestion. Figure 1 caption now reads, " Maps of G-MACMODS annual potential harvest per unit area"

Lines 75-76. Is this a reasonable expectation given the difference between growth in phytoplankton reflects the rapid turnover of single celled individuals while growth in seaweeds results from increases in the size of a comparatively steady state population of multicellular individuals in which competition for light within and among individuals can limit growth before nutrients become limiting? A brief discussion or acknowledgment of the major differences between phytoplankton and seaweed growth seem warranted given the comparisons between these two groups in the text and Fig.1.

The reviewer makes a good point; in this sentence, we are oversimplifying the different dynamics that limit phytoplankton and macroalgae growth. The main takeaway from the brief comparison between macroalgae and phytoplankton is that the reader should not integrate the results from the ambient nitrate runs to estimate global seaweed productivity potential. The

ambient nitrate runs are only useful to assess geographical variability in seaweed productivity under the assumption that seaweed farms are relatively small and artisanal.

To communicate this better, we have changed Lines 96-98 (previously lines 75-76) to read

"Thus, assuming that nutrient availability is the only constraint to macroalgal growth (neglecting all other growth and nutrient uptake limitations), we expect macroalgal productivity to be proportional to NCP or, as an upper bound on new production, phytoplankton NPP."

We have also added a brief description of the main differences between phytoplankton and macroalgae (lines 88-94):

"Macroalgal growth and carbon fixation generally occur at slower rates than in unicellular phytoplankton because multicellularity and specialized tissues require greater resources[50–52]. However, these specialized structural tissues, including holdfasts and thalli, allow macroalgae to gain access to more persistent light and nutrients at the ocean surface in coastal areas. Moreover, resource storage and mobilization, as well as chemical grazing defenses, permit survival during resource gaps[53, 54]. Combined with higher carbon-to-nitrogen (C:N) ratios than phytoplankton[55], these features allow macroalgae to achieve equivalent or greater productivity[56, 57], and much greater biomass density[58], than phytoplankton for a given amount of nutrients."

Lines 83-84. I question whether this result amounts to sufficient evidence for confirmation. Might an alternative explanation be that estimates of seaweed growth used in G-MACMODS are unrealistically high? In any case, I suggest downplaying "confirmation" (which implies absolute confidence) unless stronger evidence is provided.

We agree with the reviewer. Our new results show lower productivity in some areas, such as the equatorial Pacific. Despite the changes in productivity, we suspect that our results are optimistic because (a) we are not accounting for micronutrient or phosphorus limitations, (b) we optimize the harvest scheme, and (c) we assume that the farmer knows *a priori* which seaweed type grows best at each location. Hence, we might be overestimating seaweed growth, as the reviewer points out. Regardless, we believe that our ambient nitrate results are not representative of large-scale farming productivity. Hence, per the reviewer's request, we have substituted the word "confirms" with "suggests". Lines 102 to 105 now read,

"This suggests that the ambient nitrate case does not provide a sound basis for estimating potential productivity of widespread, intensive farming in the absence of artificial upwelling, but it might provide a reasonable estimate of the localized potential harvests of operations small enough in scale (e.g. < 25 ha[39–41]) so as to not dramatically alter local nitrate budgets."

Lines 97-99. I think these results are really interesting as they are similar to the general patterns of abundance observed in natural populations (brown algae tend to dominate in temperate regions and red algae in the tropics). It has been suggested that the prevalence of reds in the tropics is due to them having higher concentrations of halogenated metabolites that deter herbivory and biofouling, but may come at costs to growth. Perhaps this is something to mention?

We appreciate that the reviewer has contributed this valuable piece of information. However, given that our model does not account for halogenated metabolites, we cannot suggest this as a reason that tropical red macroalgae produce more biomass than tropical brown macroalgae.

Line 105. – Caption for Figure 2 should clarify that annual harvest yields are based on output from G-MACMODS.

We have included that detail in our caption for Figure 2.

Lines 111-128. The uncertainty analysis is a really valuable addition to the paper and is very well presented. The maps in Fig 3a and b provide a quick view of the magnitude of error for different regions under the two nutrient scenarios, while Figs 3g and h (which were not labeled in the pdf that I downloaded) nicely summarize the relative importance of the biological parameters in Table 1 for different levels of harvest. That mortality rate was the only variable that was relatively unaffected by annual harvest rate and nutrient scenario might be something that the authors want to highlight. The probability functions in Figs. 3c and d were not mentioned in the text. The results of these figures should either be interpreted or deleted.

We thank the reviewer for appreciating the value of our uncertainty analysis. As was detailed in the general summary of these responses, the modifications we recently implemented in our model changed the results of our random forest analysis. However, given that mortality rates remain the dominant source of uncertainty, we have added some text to highlight the points mentioned by the reviewer (lines 138-140):

"Based on a random forest analysis of Monte Carlo results, the biological parameters that consistently govern the harvested seaweed yield in both nutrient scenarios are the constant mortality rate (not due to waves) and the wave factor (specifically associated with wave mortality)."

We have also double-checked the labeling of Figure 3g and 3h, and we now mention Figs. 3c and 3d (lines 145-147).

Line 161. Is there a reason that DOC export is not included with POC export as a loss term?

We do not include DOC export in G-MACMODS because we focus on the macroalgal carbon available for harvest and not the carbon lost to the surrounding environment. We understand the need to discuss DOC in terms of carbon dioxide removal (CDR) and sequestration, but, at the request of another reviewer, we have de-emphasized our focus on CDR.

Reviewer #3 (Remarks to the Author):

General comments:

The abstract (and the general theme of the introduction/the manuscript) allude to the paper being about CDR, which is not what the study examines. The authors do state this on line 164-165 for example and in other sections of the manuscript but this point is likely lost as most of the paper talks about harvested carbon in the context of CDR. The study estimates the global harvest potential of different seaweed groups based on a dynamic growth model and does not expressly look into CDR. As such, the framing of the manuscript this way can be potentially misleading. I suggest reframing the manuscript as what the title and study examines which is potential global harvest rates of seaweed farming and have a small discussion paragraph about CDR.

We appreciate the reviewer's comments and have made some adjustments throughout the text to de-emphasize our focus on CDR (for example, we have removed the discussion on the fate of POC dislodged from the macroalgae farms, among other topics). However, we believe it is important to motivate our argument of global seaweed cultivation potential in regards to CDR, since that is a current incentive for expanding the seaweed farming industry (see, for example, the 2021 report *A Research Strategy for Ocean-Based Carbon Dioxide Removal and Sequestration* by the National Academies of Sciences and Engineering).

I have some reservations about the appropriateness of some of the data sources used for the model that I have mentioned in specific comments – some assumptions seem overly simplistic and extrapolated from one or two studies in specific species to entire groups (or all groups) which in turn reduces the usefulness of predictions. Moreover, it is often not clear which particular data sources are used when just a reference is given.

We agree with the reviewer that we should be more transparent in terms of the origin of our biological parameters. To address this, we have included the specific values for each biological parameter and their corresponding reference in Table 1. We also agree that we have only used values from 1 or 2 genera to represent an entire seaweed group, but we expect that the range of values that we test in our Monte Carlo simulations is large enough to account for differences within a seaweed group.

Specific comments:
Line 1: would suggest changing "may" to "will" – this is pretty certain.

We thank the reviewer. Lines 1-2 now read, "Recent analyses of global climate scenarios suggest that limiting warming to < 1.5º above pre-industrial levels will require large reductions in

greenhouse gas emissions as well as the removal of ~ 2.5-13 Gt-CO_2/year (~ 0.7-3.6 GtC/year) by midcentury[1,2].”

Line 27: can you put the actual number of simulations or the range if multiple

We have added the range of simulation numbers, which, incidentally, has changed since our original submission. Lines 27-29 now read, “To test sensitivities and evaluate uncertainties, we performed 1012--1066 simulations of macroalgae growth and harvest for each of four seaweed types (defined using biophysical characteristics from currently-farmed temperate and tropical red and brown genera).”

Line 39-50: I think a little bit more info (references) on the rationale for these choices is needed here. I agree nitrogen is likely to be the limiting nutrient a non-seaweed expert may need to have more information on why this is the case. It could also be useful to expand on how you came to the choice of the exact parameters for ambient and limited scenarios and some justification of these. I think it's important to make it really clear as these nutrient conditions underpin the entire study.

Thank you for the suggestions. We have added a reference for the nutrient limitation and changed the sentence to read, “G-MACMODS assumes that nitrogen is the limiting nutrient because its demand relative to other nutrients, such as phosphorus, is often greater than its supply in the ocean[34]. The average N:P ratio in macroalgae is 38:1[35], yet the median N:P ratio in the ocean is 22:1[36]” (lines 40-42).

We have also expanded our rationale for choosing our different nutrient scenarios (lines 50-57).

52-53: perhaps a reference to why this would be unacceptable to ocean health – see:
Boyd PW, et al. 2022. Potential negative effects of ocean afforestation on offshore ecosystems. Nat Ecol Evol.

We thank the reviewer for the suggestion. We have added the reference (lines 63-64; ref. 45).

Line 75: I consider this a very broad assumption with no references to support it – suggest adding info and references.

We thank the reviewer for the feedback. Given that another reviewer also commented on that sentence, we have removed it from the manuscript.

Line 85-86: "but it might provide a reasonable estimate of the localized potential harvests of operations small enough in scale so as to not dramatically alter local nutrient budgets"
this point is made a couple of times in the manuscript but no rationale/justification for it or how small a scale operations need to be – suggest some extra info/rationale for this point where it is made throughout the manuscript.

We thank the reviewer for the feedback. We have given some examples of small-scale versus large-scale operations in lines 52–55 and again in line 105.

Lines 111-128: I have not focused on the appropriateness of this technique to estimate uncertainty as it is outside my area of expertise.

We appreciate all the feedback that the reviewer has offered in other areas of the manuscript.

Discussion and conclusion

My main comment in the discussion is the framing of it around CDR potential which is not what is examined here – only harvest potential. By just doing a back of the envelope conversion of biomass to amount of carbon harvested and then discussing this in the context of CDR it disregards the complexities and uncertainties around how growing seaweed biomass equates to actual removal of CO2 from the atmosphere. The authors definitely understand this as they state it in multiple places in the manuscript and include the caveats that the amount of harvested carbon does not equate to CDR. So I think clarification around why the framing of the discussion/manuscript around CDR is needed.

We thank the reviewer for their comment. Our intention is not to detract from the complexities of CO_2 removal from the atmosphere. We framed a discussion around CDR because much of the motivation behind expanding seaweed cultivation revolves around CDR (see, for example, Duarte et al., 2021), and we want to explore how much seaweed one would have to grow (taking into consideration the deep uncertainty surrounding this topic) to meet those carbon targets. We have reworked some of our discussion to decrease the emphasis on CDR.

Online Methods

Here I have not focused my comments on the specifics/appropriateness of the modelling techniques as it is outside of my expertise and do not feel I can contribute useful/insightful comments. I have however focused on the logic/rationale/appropriateness of some of the data sources and values used in the models.

Line 213:214: the assumption that carbon is 30% for all seaweed groups is overly simplistic and can drastically underestimate the amount of variation in the units of carbon harvested as it is not uncommon for seaweed carbon content to vary between 25-40 % or even greater within a group. I consider this needs to be accounted for if converting your biomass estimates to units of carbon.

We agree with the reviewer that carbon content varies among seaweed groups. Unfortunately, G-MACMODS estimates biomass, not carbon content. To vary carbon content dynamically, we would have to rewrite our model in units of carbon, rather than biomass, as well as consider the different dynamics that control carbon content variability. We assume that carbon content is 30% of seaweed dry biomass, since this is the average carbon content found in Atkinson and Smith (1983). As an alternative, we have added text on cumulative harvest values assuming that carbon content is 20% or 40% in lines 159-161. We have also made our 30% assumption explicit in every figure caption.

Line 265 – 268: It isn't clear here how you chose the values for some of these parameters e.g. Bcap and Ug for each seaweed type as you only refer to table 1 and the references therein (often there are 2-3 for each group and it isn't clear how you chose a specific value). I suggest that this be clarified as presently unclear how growth rate is the same for all seaweed groups. I also find it very difficult to follow what data is used where as often a just citation of a reference with no details of what data was used e.g. line 268: "The power law in equation (8) was derived by re-fitting data from a comprehensive meta analysis78" – there is no information on how data was refit or what data in this reference.

We agree with the reviewer that it was previously unclear where some of the parameters stemmed from, and we have remedied this by including the specific parameter value for each reference (Table 1). The way in which we incorporate these parameters is described in our manuscript as

"Values of parameters required by G-MACMODS were gathered from available literature for a few representative seaweed genera (Table 1); 'standard runs'' were defined using average (when multiple parameter estimates were available) or speculated values (based on information from other genera when there were few or no published values)..." (previously lines 329–332, now lines 344-348).

The methods for our fit were also described in the original manuscript immediately following the sentence that the reviewer cites here (old lines 268-273, now lines 281-285):

"Our fit was applied over the data in ref. 11 and binned to 0.01-width bins from 0-1 g/L and 0.1-width bins for 1-60 g/L seaweed density, weighted by the number of observations in each bin (with a minimum weight of 8 observations). Our fit excluded data corresponding to

total-nitrogen (NO_3+NH_4) conditions not likely to be found in the surface ocean (values above 20 µM)".

Finally, we assume a maximum theoretical growth rates for all seaweed based on the upper bounds of data in the Xiao et al (2019) metadata study, but the genus-specific relative growth rates are a function of the parameters that define a response to temperature, nutrient availability, irradiance, and crowding. We have re-written some of the methods to improve our explanation of growth rate estimates (lines 273-286).

Line 281: the light-limitation being informed by phytoplankton studies does not seem appropriate – seaweed and phytoplankton have drastically different light requirements – I would suggest that the light limitation needs to be informed from seaweed specific studies.

While we agree with the reviewer, we could not find a seaweed-specific light limitation equation based on experimental evidence. Phytoplankton-based light limitation equations have historically been used to describe the light-limitation in seaweed (e.g. in Chapter 5 of Hurd et al., 2014). For example, we include here a figure depicting some of the light limitation functions previously used in other seaweed studies (Figure R1). Although the lines in Figure R1 are all different, they share a similar structure. For this reason, we have decided to keep the light limitation equation from phytoplankton studies, parameterized with seaweed relevant values.

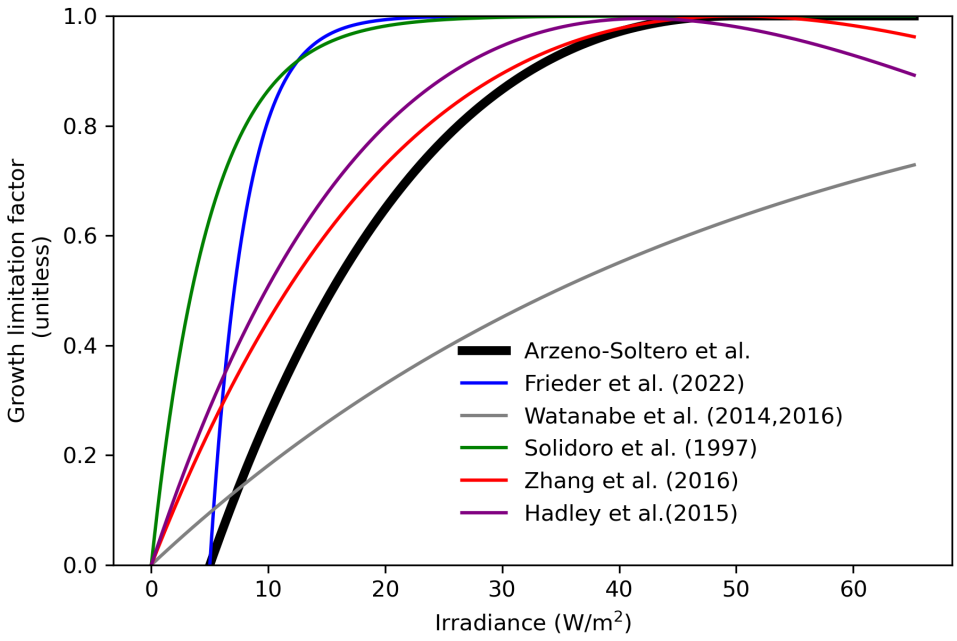

Figure R1. Light limitation equations from a variety of seaweed studies, assuming a compensation irradiance of 5 W/m^2 and a saturating irradiance of 50 W/m^2.

Line 285-290: here I have issue with the mortality from breakage due to waves. It appears due to the one citation that all breakage is based on the parameters from the reference 83 which is about mortality due to breakage in Gelidium sesquipedale (a red algae) in one location – this is then extrapolated to all seaweeds (e.g. large browns such as Sargassum, Saccharina) which are likely to have drastically different tolerances to wave energy. I consider this extrapolation to all groups and all locations around the world to be too simplistic.

So far, we have only found two equations for seaweed wave mortality. As is shown in Figure R2, the wave mortality equation taken from Duarte and Ferreira (1993), the one we implement in G-MACMODS, is less restrictive than the wave mortality equation derived for *Macrocystis pyrifera* in Rodríguez et al. (2013). Since we are applying one wave equation to four different groups, we originally implemented the more optimistic equation. However, to address the uncertainty surrounding wave morality, we have added a wave mortality factor (Π) that can increase or decrease the impact of our wave mortality equation. We also re-ran our Monte Carlo analysis to include this variable (lines 299-306 and Table 1). The wave mortality factor is now among the parameters that most influences the uncertainty in our results (see Figure 3 of the revised manuscript).

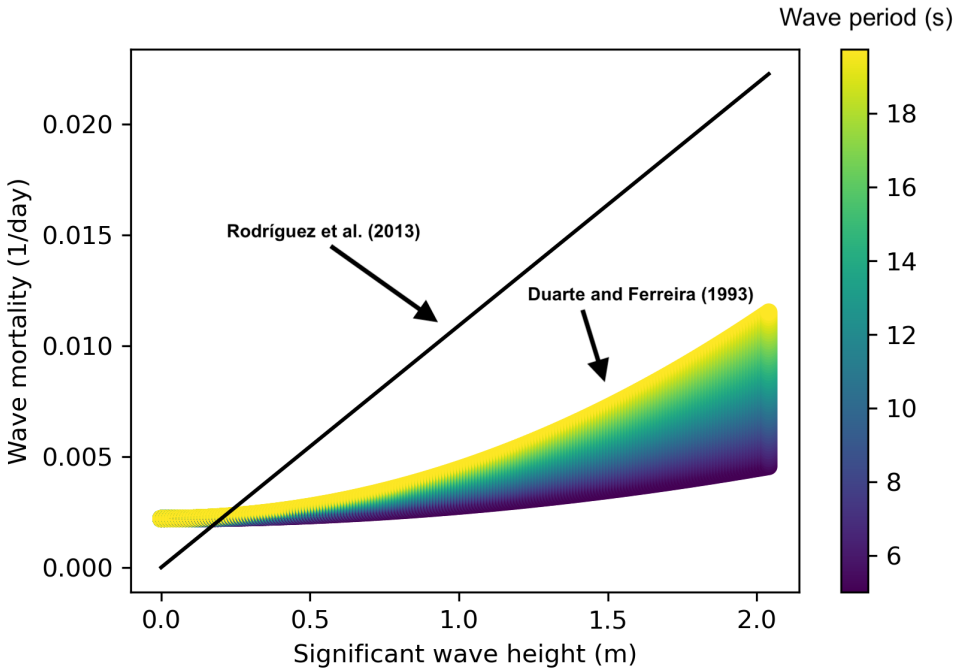

Figure 2. Seaweed wave mortality equations as a function of significant wave height and wave period.

Line 343-361: I think the comparison of the models against actual harvest data is very useful but the details surrounding how the comparisons were made need to be clearer. Comparing of medians and ranges seems how it has been done but no values are given, it is just stated that they are similar. I also question the appropriateness of including this information in the methods and not results?

We thank the reviewer for the feedback. We have re-done our model-field data comparisons to show G-MACMODS performance on a point-by-point basis (Supplementary Information Figures 5-8) . We have described our methods for these comparisons in lines 360-414. The field data used for the comparisons and details about each publication can be found in the Supplementary Information (Tables 1-4). We do not include these comparisons in the results, since they helped determine certain model parameters such as B_{cap} (lines 278-281) and, hence, were integral to the development of G-MACMODS.

Line 362-384: I really like the clear conditions explained for when seaweeds are harvested in the model.

We appreciate the reviewer's feedback.

References:

Duarte, C. M., Bruhn, A., & Krause-Jensen, D. (2022). A seaweed aquaculture imperative to meet global sustainability targets. Nature Sustainability, 5(3), 185-193.

Duarte, P., & Ferreira, J. G. (1993). A methodology for parameter estimation in seaweed productivity modelling. In Fourteenth International Seaweed Symposium: Proceedings of the Fourteenth International Seaweed Symposium held in Brest, France, August 16–21, 1992 (pp. 183-189). Springer Netherlands.

Frieder, C., Yan, C., Chamecki, M., Dauhajre, D., McWilliams, J. C., Infante, J., ... & Davis, K. (2022). A macroalgal cultivation modeling system (MACMODS): Evaluating the role of physical-biological coupling on nutrients and farm yield. Frontiers in Marine Science, 214.

Hadley, S., Wild-Allen, K., Johnson, C., & Macleod, C. (2015). Modeling macroalgae growth and nutrient dynamics for integrated multi-trophic aquaculture. Journal of Applied Phycology, 27, 901-916.

Hurd, C. L., Harrison, P. J., Bischof, K., & Lobban, C. S. (2014). Seaweed ecology and physiology. Cambridge University Press

National Academies of Sciences, Engineering, and Medicine. (2021). A research strategy for ocean-based carbon dioxide removal and sequestration.

Rodriguez, G. E., Rassweiler, A., Reed, D. C., & Holbrook, S. J. (2013). The importance of progressive senescence in the biomass dynamics of giant kelp (Macrocystis pyrifera). Ecology, 94(8), 1848-1858.

Solidoro, C., Pecenik, G., Pastres, R., Franco, D., & Dejak, C. (1997). Modelling macroalgae (Ulva rigida) in the Venice lagoon: Model structure identification and first parameters estimation. Ecological Modelling, 94(2-3), 191-206.

Watanabe, Y., Yamada, H., Mine, T., Kawamura, Y., Nishihara, G. N., & Terada, R. (2016). Photosynthetic responses of Pyropia yezoensis f. narawaensis (Bangiales, Rhodophyta) to a thermal and PAR gradient vary with the life-history stage. Phycologia, 55(6), 665-672.

Watanabe, Y., Nishihara, G. N., Tokunaga, S., & Terada, R. (2014). Effect of irradiance and temperature on the photosynthesis of a cultivated red alga, P yropia tenera (= P orphyra tenera), at the southern limit of distribution in J apan. Phycological research, 62(3), 187-196.

Xiao, X., Agusti, S., Lin, F., Xu, C., Yu, Y., Pan, Y., ... & Duarte, C. M. (2019). Resource (light and nitrogen) and density-dependence of seaweed growth. *Frontiers in Marine Science*, 6, 618.

Zhang, J., Wu, W., Ren, J. S., & Lin, F. (2016). A model for the growth of mariculture kelp Saccharina japonica in Sanggou Bay, China. Aquaculture Environment Interactions, 8, 273-283

Reviewer #4 (Remarks to the Author):

Dear authors,
I enjoyed reading your work and going over the online model and mapping tool. Your article is a thorough and important piece of work, promoting the existing set of seaweed cultivation models to a global scale without compromising the level of details.

We thank the reviewer for their generous words and useful feedback.

I would recommend the acceptance of the manuscript, but still wrote a few small comments that may help improve the paper:

Comments:
Introduction
Rows 3-5: The connection between the ocean as a natural CO2 sink and the interest in enhancing ocean carbon dioxide removal through seaweed farming is not clear. How much of the natural CO2 removal in the ocean is related to seaweed? Please explain.

We thank the reviewer for that feedback. At the request of another reviewer, we have de-emphasized the focus on carbon-dioxide removal, which has led us to remove that particular sentence.

Rows 6-8: Biofuel production from seaweed is under research for decades and is still not commercial. Better not to mention it as a first potential use and to be more cautious about it's real potential. In addition, I believe ecosystem services may be provided even in a seaweed sinking project.

We appreciate highlighting that the seaweed biofuel industry is still under development. While we should be cautious about its real potential, we would still like to leave that option in this sentence. We have rewritten the sentence to now say,

"Some strategies for reducing greenhouse gas emissions include incorporating farmed seaweed into human and animal diets[3–5] or generating biofuels[6–8], though some challenges still prevent commercial-scale use of seaweed-based biofuel[9] (lines 3-5)."

Global Seaweed Harvested Yields
Rows 79-80: "… which implies that seaweed might fix, at least, 3-6 times more carbon than current NCP" - I don't think this is correct. The different C:N ratios derive mostly from lower N contents in seaweed (mg N per g DW) rather than from higher C content (mg C per g DW). Please explain or revise this sentence.

We thank the reviewer for pointing out that high C:N ratios could be due to low nitrogen content, rather than high carbon content. We have modified the sentence with a caveat on nitrogen consumption. Lines 99-100 now read,

"If seaweed consumed all the nitrogen available to phytoplankton, then we would expect to see, at most, 6 times as much seaweed POC as phytoplankton NPP."

Rows 96-97: "...perhaps reflecting the advantage of having higher maximum uptake rates in regions with lower nutrient concentrations (Figs. 1f and 1g)." – I think for low nutrient concentrations the real advantage would be a low half saturation rate (i.e., Ks) rather then a higher maximum uptake rate.

We appreciate the feedback and agree that lower Ks is a real advantage in low-nitrate environments. The sentence now reads,

"Temperate red seaweed appear at the equatorward edges of regions dominated by temperate brown seaweed, perhaps reflecting the advantage of having higher maximum uptake rates and lower half-saturation constants in regions with reduced nitrate availability (Figs. 1f and 1g)" (lines 114-116).

Meiron Zollmann

Editor comments

"[I] consider there is some concern around the estimates of DBL thicknesses within densely packed seaweed farms, these may be orders of magnitude larger than those estimated using models of mainstream seawater velocities. However, I understand this is complex, so perhaps a simple caveat is required here for the reader, as this would further support the author's conclusions"

We greatly appreciate the feedback from the editor. We agree that diffusive boundary layers are likely to be thicker within a densely packed seaweed farm. We have added this caveat within the section Model Limitation. Lines 453-454 now read,

"We expect that densely-packed farms would impede thinning of the diffusive boundary layer, thus reducing macroalgae's ability to take up nitrate."

8th Mar 23

Dear Dr Arzeno-Soltero,

Your manuscript titled "Biophysical potential and uncertainties of global seaweed farming" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Christopher Cornwall, PhD
Editorial Board Member
Communications Earth & Environment

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

This is a revised version of the manuscript I reviewed previously. The authors have made substantial efforts to address the points raised by all reviewers. For myself, I am satisfied that the authors have fully addressed the criticisms I raised earlier and I have no further comments to make, other than to confirm that I feel this will make a nice, substantial contribution to the debate on uses of offshore cultivation in producing seaweed biomass for use, inter alia, in carbon dioxide removal.

John Beardall

Reviewer #2 (Remarks to the Author):

The authors adequately addressed all the concerns that I raised in my previous review. I congratulate them on producing a really nice paper that should set the standard for future work on this topic. I really appreciated their scholarly efforts to discover, scrutinize and incorporate the results of previously published works in all aspects of their study.

Below I offer a few minor editorial suggestions for the authors to consider

Abstract –It would be helpful to provide an example (or two) of the biophysical constraints referred to at the end of the Abstract to give some context for the understanding that specifically needs to be improved.

Lines 30-31. The attribute of nitrate that was evaluated needs to be clarified. Suggest rephrasing sentence as follows “Environmental forcing included data for water temperature, solar irradiance, current velocities, wave height, wave period, and nitrate concentration…”

Line 85. “particular” or “particulate”? Particular makes it seem like a select type of organic matter as opposed to particulate which implies organic matter composed of separate particles.

Line 103. Suggest being more direct with the conclusion. Consider replacing “does not provide a sound basis for estimating” with "greatly overestimates the"

Lines 115-116. Are there any studies that show that red seaweeds have higher maximum uptake rates and lower half-saturation constants? If not, then consider deleting this sentence as it seems highly speculative.

Line 184. There are no reference numbers 266 and 268. I suspect these are typos and refer to reference numbers 66 and 68.

Legend for Figure 4. Suggest defining the white boxes in 4a (e.g. White boxes in 4a correspond to the locations depicted in 4b, c, and d)

Reviewer #3 (Remarks to the Author):

I am satisfied with the authors responses to my comments and look forward to seeing the published version soon.

Reviewer #4 (Remarks to the Author):

Dear authors,
Thanks for responding to my comments and revising the manuscript.
Now I believe it is ready for acceptance from my side.

Best regards,
Meiron

We express our sincere appreciation to the editor and reviewers for their valuable feedback during the revision process. We have implemented the editor's suggestion to update the title and abstract to better reflect the changes made in the revised manuscript. Furthermore, we have followed the journal's guidelines by removing in-text citations and adding appropriate citations for the environmental data. We have also recalculated the area from which we harvested the macroalgae, and made corresponding changes to the text (lines 74-75). We would like to emphasize that this modification does not affect the results of our study.

We are pleased to announce that our model and data are now publicly available, as indicated in the Data Availability and Code Availability statements. We are confident that the final version of the manuscript is a significant improvement over the original submission, thanks to the constructive comments and guidance provided by the editor and reviewers.

Once again, we express our gratitude to the editor and reviewers for their valuable feedback and support.

What follows is our response to the reviewers' comments. Changes in the manuscript have been written in red. The reviewers' statements are in black italics while the authors' responses are in blue roman font.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

This is a revised version of the manuscript I reviewed previously. The authors have made substantial efforts to address the points raised by all reviewers. For myself, I am satisfied that the authors have fully addressed the criticisms I raised earlier and I have no further comments to make, other than to confirm that I feel this will make a nice, substantial contribution to the debate on uses of offshore cultivation in producing seaweed biomass for use, inter alia, in carbon dioxide removal.

John Beardall

The authors thank Dr. Beardall for highlighting the relevance of the manuscript for the ongoing debates on offshore seaweed cultivation. Dr. Beardall's contributions have been greatly appreciated throughout this review process.

Reviewer #2 (Remarks to the Author):

The authors adequately addressed all the concerns that I raised in my previous review. I congratulate them on producing a really nice paper that should set the standard for future work on this topic. I really appreciated their scholarly efforts to discover, scrutinize and incorporate the results of previously published works in all aspects of their study.

The authors extend their sincere appreciation to Reviewer #2 for their invaluable insights and comments during the revision process.

Below I offer a few minor editorial suggestions for the authors to consider

Abstract –It would be helpful to provide an example (or two) of the biophysical constraints referred to at the end of the Abstract to give some context for the understanding that specifically needs to be improved.

We thank the reviewer for the suggestion. We have replaced our abstract with the one suggested to us by the editor.

Lines 30-31. The attribute of nitrate that was evaluated needs to be clarified. Suggest rephrasing sentence as follows "Environmental forcing included data for water temperature, solar irradiance, current velocities, wave height, wave period, and nitrate concentration…"

The authors thank the reviewer for their suggestion and have modified lines 30-32 to say, "Environmental forcing included data for water temperature, solar irradiance, current velocities, wave height, wave period, and nitrate concentrations, sourced from a combination of satellite measurements (MODIS) and global ocean model simulations (HYCOM and CESM)."

Line 85. "particular" or "particulate"? Particular makes it seem like a select type of organic matter as opposed to particulate which implies organic matter composed of separate particles.

The authors thank the reviewer for pointing out this typo. Line 85-87 now read, "In steady-state, the upper ocean nutrient inventory is set by a balance between biologically-mediated export (e.g., sinking particulate organic matter), equivalent to NCP, and physically-mediated supply; nutrient supply thus comprises an ultimate constraint on the magnitude of NCP."

Line 103. Suggest being more direct with the conclusion. Consider replacing "does not provide a sound basis for estimating" with "greatly overestimates the"

The authors thank the reviewer for the suggestion. Lines 103-106 now read, "This suggests that the ambient nitrate case greatly overestimates the potential productivity of widespread, intensive farming in the absence of artificial upwelling…"

Lines 115-116. Are there any studies that show that red seaweeds have higher maximum uptake rates and lower half-saturation constants? If not, then consider deleting this sentence as it seems highly speculative.

The authors have deleted this sentence.

Line 184. There are no reference numbers 266 and 268. I suspect these are typos and refer to reference numbers 66 and 68.

Thank you for that observation. The numbers are indeed supposed to read 66 and 68, but, given that these references show up as superscripts next to "km^2", the "2" in "km^2" and the reference numbers meld together. We have added space between "km^2" and references 66 and 68 to avoid the confusion (line 183).

Legend for Figure 4. Suggest defining the white boxes in 4a (e.g. White boxes in 4a correspond to the locations depicted in 4b, c, and d)

The authors thank the reviewer for the suggestion. The white boxes in Figure 4a are now defined in the figure legend.

Reviewer #3 (Remarks to the Author):

I am satisfied with the authors responses to my comments and look forward to seeing the published version soon.

The authors are grateful for the constructive comments provided by Reviewer #3 throughout the review process.

Reviewer #4 (Remarks to the Author):

Dear authors,
Thanks for responding to my comments and revising the manuscript.
Now I believe it is ready for acceptance from my side.

Best regards,
Meiron

The authors appreciate Dr. Zollmann's thoughtful feedback throughout the review process.