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Reply on RC1

Jiajun Wu et al.

Author comment on "Carbon dioxide removal via macroalgae open-ocean mariculture and sinking: an Earth system modeling study" by Jiajun Wu et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-104-AC4>, 2022

Wu et al. investigate seaweed farming for atmospheric CO₂ removal (abbreviated as 'MOS' in their study) in an Earth System model. They do so by plugging in a macroalgae model into UVic, making the biological carbon pump ultra-efficient, mainly by no remineralization losses during sinking of seaweed carbon and C/N ratios of 20 for macroalgae primary production (instead of presumably 6.6 phytoplankton primary production). Their study is timely, important, and interesting. I have, however, several major comments, which I think need consideration and hopefully improve the manuscript.

Dear Dr. Bach,

We appreciate your time and efforts to provide feedback on our manuscript and are grateful for the insightful comments on and valuable improvements to our manuscript "Carbon Dioxide Removal via Macroalgae Open-ocean Mariculture and Sinking: An Earth System Modeling Study". We will incorporate your suggestions in the revised manuscript. Please see below for a point-by-point response to your comments and concerns.

Major comments:

- **Zooplankton grazing on macroalgae:**

The authors implement grazing on macroalgae by the generic zooplankton group in the model. I was curious because I have never heard of zooplankton grazing on macroalgae. The authors cite a modeling study as evidence (Baird et al. 2003) but this study says: "In the model, large zooplankton graze on large phytoplankton and microphytobenthos, while small zooplankton graze on small phytoplankton." This study does not simulate macroalgae grazing by zooplankton according to this sentence and therefore it cannot be taken as evidence for this mechanism.

Response: Thank you for pointing out this. In the revised MS, we have removed the reference of Baird et al., 2003. See the response below for further explanation of our parameterization.

Later in the text the authors reference another modeling study (Trancoso et al., 2005) but this study does not provide evidence either but instead refers to a 700+ page textbook to

justify their zooplankton grazing preference for macroalgae of 0.0008. Based on this, it is quite nebulous where the justification for zooplankton grazing on macroalgae comes from. Introducing this trophic link and the associated matter flux may not be appropriate. But I have too limited understanding of the model to understand if this parameterization has noticeable influence on any relevant outcome. The authors should justify/clarify this issue.

Thank you for the opportunity to clarify the parameterization used in the model. The confusion about having pelagic zooplankton graze on macroalgae likely comes about because we used the original terminology of such models, which designates all higher trophic levels as "zooplankton". We admit that there is little evidence that pelagic zooplankton graze on macroalgae. However, in our model "zooplankton" represent all higher trophic levels, including known macroalgae grazers such as amphipods (Jacobucci et al., 2008), gastropods (Chikaraishi et al., 2007; Krumhansl et al., 2011), sea urchins (e.g. Yatsuya et al., 2020) and fishes (e.g. Peteiro et al., 2012). Thus, we included this food web pathway of the model to assess the sensitivities of macroalgae to potential grazers in the ocean, assuming that with large macroalgae farms the pelagic larva of some grazing organisms like fish or urchins, would settle within the farms. Text will be added to the manuscript to clarify this point and better explain what "zooplankton" actually represents in the model.

However, it is worth noting that even with the assigned grazing preference ($\psi = 1 \times 10^{-4}$), the "zooplankton" communities do not have large grazing impacts on macroalgae biomass. In the revised manuscript we will illustrate this via a sensitivity experiment where zooplankton does not graze on macroalgae but follows the original grazing preferences in UVic ESCM..

- **The importance of CN ratios:**

From my understanding, the key parameter determining the CDR potential of MOS in their model is the CN ratio of macroalgae (assumed to be 20), or more precisely the delta_CN relative to phytoplankton. I assume the UVic phytoplankton CN is 6.6 (this should be mentioned in the text) so the delta_CN is 13.4. Surprisingly, there is no sensitivity analysis on this important parameter which should largely determine the CDR (e.g. 270 PgC until 2100) as given in the abstract. There are two issues with this:

When I utilize our own calculation from Bach et al. (2021), which calculated the CDR dependency of MOS as a function of phytoplankton and macroalgae CN, then I calculate with your numbers (20 and 6.6) a reduction of the CDR potential of 33%. This is very similar to your 37%, which is encouraging. However, the important point is that our calculation underlined the massive dependency of the MOS CDR potential on the assumed phytoplankton/macroalgae CN combinations (I attached the figure below). For example, just mildly increasing phytoplankton CN from 6.6 to 8 (8 being more realistic for (sub)tropics; Martiny et al., 2013), then CDR reduction increases from 33% to 40%. I would assume that similar sensitivities occur in their model, so that it is at least important to emphasize how influential the CN assumptions are.

Response: Thank you for pointing out this important point concerning the potential influences of the stoichiometric CN ratio of macroalgae on the CDR capability of MOS. According to your suggestions, we have performed additional sensitivity simulations (MOS_Conti \pm 120% of the original macroalgae molar C:N ratio) to address this issue. The result indicates that the CDR capacity of MOS is sensitive to the molar C:N ratio of macroalgal biomass. Compared to MOS_Conti, the carbon captured by MOS (MOS-C) in MOS_Conti_CN_High increases by 22% (by the year 2100) and 19% (by the year 3000) when the macroalgae C:N ratio is increases by 20% to 240:10. When the macroalgae C:N

is 20% lower than the original value, MOS-C decreases by 13% (by the year 2100) and 18% (by the year 3000). Our results agree with the range of CDR potential reduction by nutrient reallocation (7-50%) reported in Bach et.al., 2021.

We have added these results and analysis in the revised manuscript.

- **The authors provide the CDR potential and provide e.g. 270 PgC until 2100 for the scenario without artificial upwelling. I believe this number is based on macroalgae-specific carbon sequestration. If this is the case then this number is misleading because the growth of macroalgae resulted in a substantial decline in phytoplankton carbon sequestration. The MOS CDR potential should be provided as CDR_MOS – CDR_phytoplankton to account for the substantial reduction of an already existing carbon sink.**

Response: Sorry if there is any confusion over how much CDR is done and how we define it. In table 5 we actually give several numbers including how much C is sequestered in the sunk macroalgae biomass (the 270 Pg C you mention above) and how much atmospheric CO₂ has been reduced, which for the above example is only a reduction of 142.6 Pg C. The difference between what is sequestered by the macroalgae and how much atmospheric CO₂ decreases is due to several C cycle responses and the direct effect of MOS on the ocean. These effects include the decline in phytoplankton CDR as a result of shading and nutrient removal by MOS, and also the C-cycle responses of the land and ocean to lower atmospheric pCO₂ when MOS is deployed (see Keller, D. P., Lenton, A., Littleton, E. W., Oschlies, A., Scott, V., & Vaughan, N. E. (2018). The Effects of Carbon Dioxide Removal on the Carbon Cycle. *Current Climate Change Reports*, 4(3), 250–265. <https://doi.org/10.1007/s40641-018-0104-3>). We will explain the differences between MOS-induced carbon export, air-sea carbon fluxes and reduction of atmospheric CO₂ in more detail in Section 4.3.2 of the revised version of the manuscript. In response to the comments by Ken Caldeira, we also added runs without MOS but with CO₂ emissions reductions and found that one ton of CO₂ sequestered in the ocean by MOS is, in our model and on a 100 year timescale, equivalent to an emission cut of about 0.8 tons of CO₂. This will now also be included in the revised manuscript.

- **Artificial upwelling:**

The authors test if nutrients acquired from artificial upwelling could boost MOS. Artificial upwelling is implemented by upwards movement of nutrients but the upwards movement of cold temperature is avoided. I understand the rationale for this (I guess you don't want to make this an artificial upwelling paper and therefore 'engineer' a perhaps rather unrealistic counter-current heat exchange into the pipes). However, for the not so familiar reader this paints a very rosy picture. As you have shown in the 'sorcerer's apprentice paper' (Oschlies et al., 2010), upward movement of temperature enhances heat absorption by the oceans so that Earth absorbs more heat than without artificial upwelling (and will be warmer than without artificial upwelling once it is stopped). For me this is such an extreme side-effect that I would think artificial upwelling has very little chance for large-scale implementation. The problem I am having is that the neglect of AU side-effects in the paper could make this addition to MOS sound more attractive than it may be. I think this needs to be thoroughly explained.

Response: Thank you for noting this important discrepancy. In this study, we avoided the pumping of cold water by artificial upwelling (AU) by using a hypothetical AU system that keeps temperatures at ambient levels (e.g. via heat exchangers). As you mentioned above, it has been revealed that the upwelled cold waters by ordinary AU may cause significant side effects, such as the quick rebound, or even surpassing, of CO₂ concentrations and surface temperatures after the termination of AU (Oschlies et al.,

2010). Meanwhile, if the cold water upwelling was included in this study, there would be extra analysis required on the associated impacts on planetary radiation budget imbalance, marine biogeochemistry and global carbon pool (e.g., the enhanced terrestrial carbon sequestration due to the cooling effects by AU). All these points are interesting to discuss, however, they are beyond the scope of this study. Our main object in this study is to analyze the maximum CDR potential of MOS, and the AU systems are aimed at providing extra nutrients that supply the macroalgae of MOS. Therefore, we decided to avoid the simulations of MOS with ordinary AU in the manuscript. This reasoning will now be stated with more detail and also a more cautionary note regarding the highly idealized scenario.

Here we show a comparison of MOS with ordinary AU (MOS_AU_Conti_with_heat) with the MOS_AU_Conti and RCP 4.5 in the following **Figure 1**. As shown in the left panel, the decline of CO₂ concentration in MOS_AU_Conti_with_heat is slightly slower than MOS_AU_Conti. This can be explained by the slackened macroalgae NPP due to the ambient upwelled cold water. As expected, the upwelling of cold water by ordinary AU leads to a surface temperature drop during the first decades after deployment. However, the drop in surface averaged temperature (SAT) cannot be maintained due to higher atmospheric CO₂ and slowly warming subsurface waters that become source waters of the artificial upwelling. After a few hundred years, SAT is even higher in MOS_AU_Conti_with_heat than the MOS_AU_Conti with elimination of heat exchange.

These results indicate that the ordinary AU systems will limit the potential of MOS in CDR and global warming mitigation, due to the upwelling of cold water to the surface. However, it is quite difficult to isolate the contribution of upwelled cold water and the CDR by MOS to the decline of SAT.

Although the MOS experiments with ordinary AU will not be included in the manuscript, we have added some discussion in the chapter 5 **Concluding discussions** to address the discrepancy, which states:

“Here we only discuss the CDR potential of MOS_AU combination under an idealized situation, where the artificial upwelling (AU) upwells nutrients without transporting heat and thereby changing the ambient water temperature. The aim of deploying this AU with MOS is to assess the maximum potential of CDR under AU-induced fertilization. However, it can be expected that the NPP of MOS will be slower if the ambient water temperature is reduced by the upwelled cold water upwelled, which will further limit the CDR potential of MOS_AU. Moreover, several dominant side effects due to the upwelled cold water in ordinary AU have been revealed, such as the quick rebound or even surpassing of CO₂ concentrations and surface temperatures after the termination of AU (Oschlies et.al., 2010b). A more detailed analysis of these physical effects would be required when ordinary AU were to be deployed in association with macroalgae farming.”

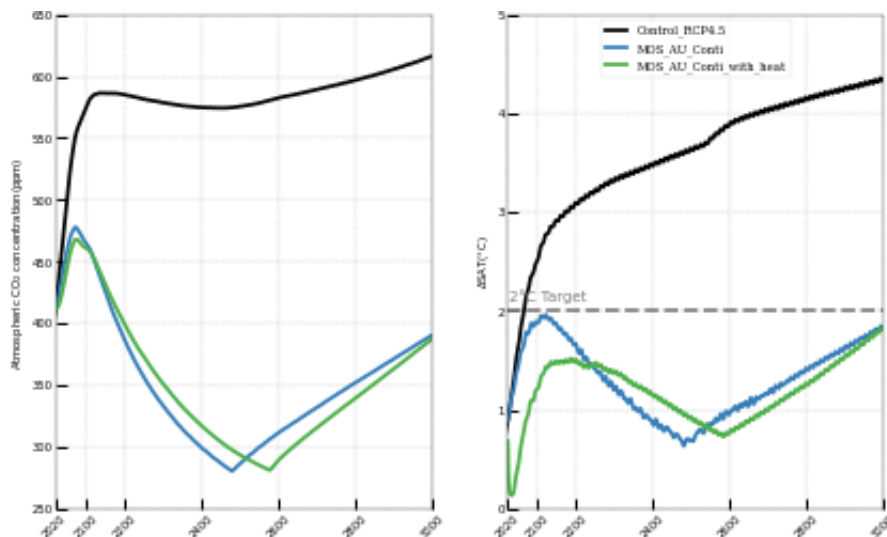


Figure 1. Simulations of annual global mean atmospheric CO₂ concentrations (left panel) and surface averaged temperature relative to the pre-industrial (right panel). The green lines (MOS_AU_Conti_with_heat) represent the continuous MOS_AU with ordinary AU systems which upwell cold water.

▪ **Environmental context:**

The authors ‘bombarded’ the oceans with MOS. Accordingly, they yield very high amounts of CDR. This is an important finding and the necessary first step but I think the discussion on side-effects does not reflect the massive change superimposed by modelled MOS appropriately (with the exception of de-oxygenation, which is covered very well). Looking at Fig. 7, it looks as if phytoplankton net primary production (PNPP) is largely replaced by macroalgae NPP (MNPP) in places where MOS was implemented (e.g. Eq. Pacific or Southern Ocean). The authors provide a global mean for the reduction of PNPP (37%) but this distracts from the problem that this reduction may be locally much higher. It seems like the CDR potential calculated here comes at the cost of an almost complete replacement of phytoplankton food webs in some regions (e.g. Eq. Pacific or Southern Ocean). For the productive Eq. Pacific this sounds like replacing the rain forest with palm oil plantations. Such a requirement for a deep transformation of entire marine ecosystems needs to be mentioned alongside the numbers that suggest a high CDR potential, just to illustrate what it really takes to achieve those numbers. (I am mainly stumbling over the second last sentence of the abstract which sounds almost encouraging but is it really encouraging considering the environmental transformation?). One suggestion to illustrate that would be a world map that shows %NPP of macroalgae in 2100 (i.e. $\text{MNPP}/(\text{MNPP}+\text{PNPP})\times 100$).

Response: Thank you for pointing out these very valid concerns, which we share. We appreciate your suggestion of presenting regional shifts rather than only global relative changes of phytoplankton NPP and have produced the map of %NPP of macroalgae as suggested by the reviewer (see below). In the revised version we will add more text in Section 4.3.4 to better illustrate the impacts on NPP and the implications that this would have for marine food webs, which read:

“Fig.8 illustrates the shift of oceanic NPP from PNPP to MOS_NPP. In regions where MOS is deployed, 70% of the total NPP is macroalgae NPP. The macroalgal NPP is thus nearly twice as high as PNPP. This may lead to additional ecological and biogeochemical issues. One of them is the decline of zooplankton in this study. We performed an additional

simulation, in which the zooplankton grazing on MOS is turned off, and the grazing preferences follow the original settings in Keller et al., 2012. As shown in Fig.B12, the grazing by zooplankton on MOS has no significant effect on neither the zooplankton biomass nor the MOS_NPP. As the zooplankton grazing preference for macroalgae is lower than for phytoplankton, the zooplankton community is still mainly fed by phytoplankton. Therefore, the decline in zooplankton biomass (Fig.B4) follows the declining phytoplankton biomass trend (Fig.B6)."

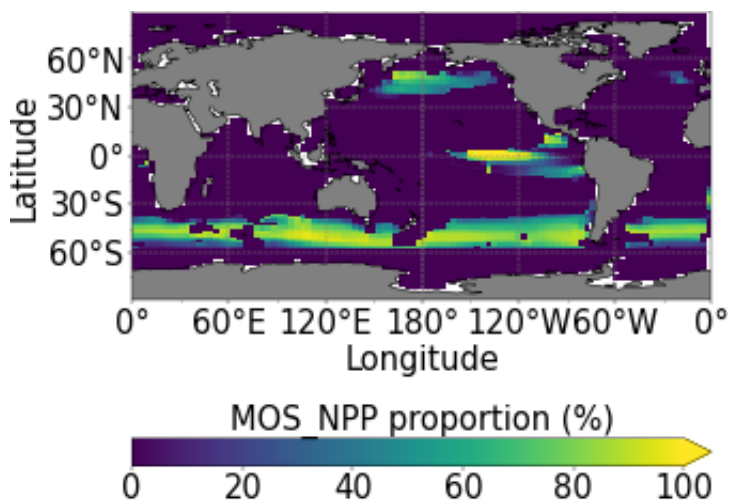


Figure 8. Proportion of MOS NPP (MOS_Stop/Conti) in the global oceanic NPP by year 2100 ($\text{MOS_NPP}/(\text{MOS_NPP} + \text{PNPP}) \times 100$). Note that the NPP values are converted to carbon using each own C:N ratio. The MOS NPP generally amounts to more than 70% of total oceanic NPP where MOS is deployed, indicating an obvious NPP shift from phytoplankton PNPP to MOS NPP.

Other comments:

- **Line 54: Sherman Ref missing.**

Response: Actually this is included in the reference list, which reads:

"Sherman, M. T., Blaylock, R., Lucas, K., Capron, M. E., Stewart, J. R., DiMarco, S. F., Thyng, K., Hetland, R., Kim, M., Sullivan, C., Moscicki, Z., Tsukrov, I., Swift, M. R., Chambers, M. D., James, S. C., Brooks, M., Von Herzen, B., Jones, A., and Piper, D.: SeaweedPaddock: Initial Modeling and Design for a Sargassum Ranch, in: Ocean. 2018 MTS/IEEE Charleston, Ocean 2018, pp. 1–6, IEEE, <https://doi.org/10.1109/OCEANS.2018.8604848>, 2019".

Or did you mean some other paper by Sherman that we are not aware of?

- **Line 60: The authors stress that there are now well-funded research efforts going into the development (engineering) of seaweed platforms making MOS 'no longer fictional'. This text suggests, that there haven't been such efforts before, which is not the case. The US DOE (I think) has put significant money into this idea already in the 80ies (see review by Ritschard, 1992). So it wasn't fictional back then but because the ocean destroyed the infra-structure it seemed to have returned to fictional after this early reality-check revealed how hard it really is. This text should be amended to include these early trials (also indicating how old the idea really is).**

Response: Thank you for noting this and sorry for missing these earlier references. Text

will be added to the 4th paragraph of the Introduction pointing out this early work and citing Ritschard, 1992, which reads:

"In the 1970's, the concept of ocean farms using macroalgae as a marine carbon sink and for bioenergy production was studied with an actual small test farm established off the coast of southern California. These research activities were abandoned due to the damage of the test farm by winter storms and several technical and economic reasons (Ritschard, 1992)."

- **Line 77: I stumbled over the word 'comprehensively'. While I totally see the massive value of testing MOS in an ESM, I would say an assessment is only comprehensive when it includes 1) Models 2) experiments 3) assessments using natural analogues. Maybe the word is just a bit too much as ESMs still miss much of the real-world complexity and many relevant processes (e.g. DOC cycling).**

Response: Yes, we fully agree with the reviewer that this phrasing was not correct, even though we meant it in the narrow sense of evaluating atmospheric CO₂ drawdown by accounting for global C cycle responses. We will replace 'comprehensively' by 'in a global carbon cycle context'.

- **Line 99: This is understandable, but has major implications for the overall conclusion concerning CDR potential (see major comment).**

Response: As also stated in our response to the major comment above, we have performed a sensitivity analysis using different C:N ratios of macroalgae. Results of these experiments will be presented in the revised version to better illustrate the sensitivity of our results to this assumption. We will also expand the discussion of how the next steps in modeling of MOS need to account for variations in essential parameters of macroalgae dynamics.

- **Line 107: Including iron? Much of the area considered for MOS in this study would be Fe-limited. Can we simply assume that coastal seaweeds with very low surface/volume ratio would have any chance in Fe-competition against mostly small (i.e. high S/V ratio) and specialized open ocean phytoplankton? Or do the authors assume Fe comes from the platform? I get that iron may perhaps be too premature to be included in this study (as there seems to be little data) but iron is not mentioned once in the manuscript. It may be worthwhile to at least mention this limitation somewhere.**

Response: Thank you for pointing out this issue. Indeed, iron limitation of macroalgae is not considered in the current study. We agree that most of the ocean regions occupied by MOS are iron-limited for phytoplankton, but we do not know the limitation effect on macroalgae. While we can expect some iron release from the platform, we will acknowledge the lack of information and model parameterisations of iron limitation in macroalgae (both in the methods section and discussion).

- **Line 122: Is the species-specific optimum as in equation 7 for a specific species (e.g. Laminaria) or is that a generic equation?**

Response: The species-specific optimum temperature is an optimum temperature for the specific macroalgae species. Considering both the reported optimum temperature of genus *Saccharina* (reported range 13-30) as well as the observed global distribution of macroalgae, we chose the value of 20°C for a generic optimum temperature. This will be explained in more detail in the revised manuscript. We will also update the reference for optimum temperature in Tab. 1.

- **Line 125: I had problems understanding eq. 9, perhaps because Tmin and Tmax were not explicitly introduced. Assuming they mean min and max T at which a species can grow (?) that would mean that there are only three possible cases for temperature (Topt, Tmin, Tmax)?**

Response: Sorry for not including definitions of what Tmin and max are. Tmin/Tmax is the lower/upper temperature limit above/below which growth of macroalgae ceases. We will add a sentence before this set of equations that explicitly define them.

- **Line 153: The immediate remineralization may be a critical assumption, if the fraction of eroded biomass is high. Can you indicate how high the erosion typically is?**

Response: The parameterization for immediate remineralization was implemented to minimize the computational expense of the model, as explicitly including particulate detritus from eroded biomass would require an additional state variable. Some phytoplankton biomass is treated in a similar manner in a "fast recycling" loop designed to simulate some DOM and microbial loop dynamics (see Keller et al., 2012). We agree that if eroded biomass were high and the material were to rapidly sink or be transported out of the grid cell, then there would be biogeochemical dynamics that the model does not adequately capture. However, the rate of erosion is 0.01% per day, i.e. a few percent per year (see Table 1) and its detailed parameterization is therefore expected to have only a small impact on the dynamics. To help the reader understand why we parameterized the model this way we will include some text stating that "this parameterization of erosion, a small biomass loss of 0.01% per day, pragmatically set as instantaneous remineralization rather than introducing another finite remineralization and finite sinking parameterization with difficult-to-constrain parameters."

- **Line 212: I did not understand this sentence. How was pCO₂ calculated separately for MOS_DIC? Is it added to the DIC pool and then the delta to the DIC pool?**

Response: Sorry for the confusing expression. The air exchange of MOS_DIC shares the identical physical properties as the original DIC. We have deleted this sentence in the revised manuscript.

- **Table 1: Reference numbers? Where can these be checked??? Hard to review because it would be great to know where all these numbers come from.**

Response: Sorry for the confusion. These were in an early version, but were inadvertently left out of the final draft. The references have now been added directly to the table.

Table 1. Model parameters

Symbol	Parameter	Unit	Value	Reference
a_{mac}	Macroalgae carbon specific shading area	m^2/kgC^{-1}	11.1	Trancoso et al. (2005)
d	Distance between the cultivating ropes	m	10	Van Der Molen et al. (2017)
$R_{erosion}$	Individual erosion rate	$\% d^{-1}$	0.01	Zhang et al. (2016)
I_{opt}	Optimum light intensity for macroalgae growth	$W m^{-2}$	180	Zhang et al. (2016)
NO_3	Nitrate Concentration	$\mu mol l^{-1}$	model calculation	Keller et al. (2012)
PO_4	Phosphate Concentration	$\mu mol l^{-1}$	model calculation	Keller et al. (2012)
K_N	Half-saturation constant for nitrogen uptake	$\mu mol l^{-1}$	2	Zhang et al. (2016)
K_P	Half-saturation constant for phosphorus uptake	$\mu mol l^{-1}$	0.1	Zhang et al. (2016)
k_w	Coefficient of light attenuation through water	m^{-1}	0.04	Keller et al. (2012)
k_p	Coefficient of light attenuation through phytoplankton	$m^{-1}(mmol m^{-2})$	0.047	Keller et al. (2012)
M_{mac}	Thickness of MOS macroalgae canopy	m	10	Trevathan-Tackett et al. (2015)
$MR_{C:N}$	Molar C:N ratio of macroalgal biomass	-	20	Atkinson and Smith (1983)
$MR_{P:N}$	Molar P:N ratio of macroalgal biomass	-	0.05	Atkinson and Smith (1983)
$MR_{C:P}$	Molar C:P ratio of macroalgal biomass	-	200	calculated
$MR_{DW:WW}$	Ratio of DW to WW of macroalgal biomass	-	0.1 (values reported:0.05-0.2)	Aldridge and Trimmer (2009); Conover et al. (2016); Van Der Molen et al. (2017)
$MR_{C:DW}$	Carbon content of dried macroalgal biomass	%	30	Chung et al. (2011)
$MR_{N:DW}$	Nitrogen content of dried macroalgal biomass	-	0.16	Duarte et al. (2003)
R_{max20}	Maximum respiration rate at 20°C	$\% d^{-1}$	1.5	Martins and Marques (2002); Zhang et al. (2016)
r	Empirical coefficient for macroalgae respiration	d^{-1}	1.047	Martins and Marques (2002)
$Seed$	Initial macroalgal biomass per kilometer cultivating line concentration of N	$kgC km^{-1}$ $mmol N m^{-3}$	2.5 0.02	Van Der Molen et al. (2017) calculated
T_1	E-folding temperature of biological rates	°C	15.56	Schmittner et al. (2008)
T_{opt}	Optimum temperature for growth	°C	20(values reported: 13-30)	Zhang et al. (2016); Martins and Marques (2002)
T_{max}	Upper temperature limit above which growth ceases	°C	35	Breeman (1988)
T_{min}	Bottom temperature limit below which growth ceases	°C	0	Martins and Marques (2002)
u_{max}	Maximum growth rate	d^{-1}	0.2	Zhang et al. (2016)
w	Areal mean artificial upwelling rate	$cm d^{-1}$	1,10	Ochlies et al. (2010b)
Y_{max}	Maximum yield of macroalgal biomass on MOS	$t DW km^{-2} yr^{-1}$	3300	
ψ_{mac}	Zooplankton grazing preference on macroalgae	-	1×10^{-4}	Trancoso et al. (2005)
ρ_{mac0}	Remineralization rate of sunk macroalgal biomass at 0 °C	$\% d^{-1}$	7	Partanen et al. (2016)

- **Line 285: May be nice to have the temperature optimum curve for your seaweed in a figure.**

Response: Thank you for the suggestion. We have added the following temperature optimum curve to the macroalgae model as Figure A3.

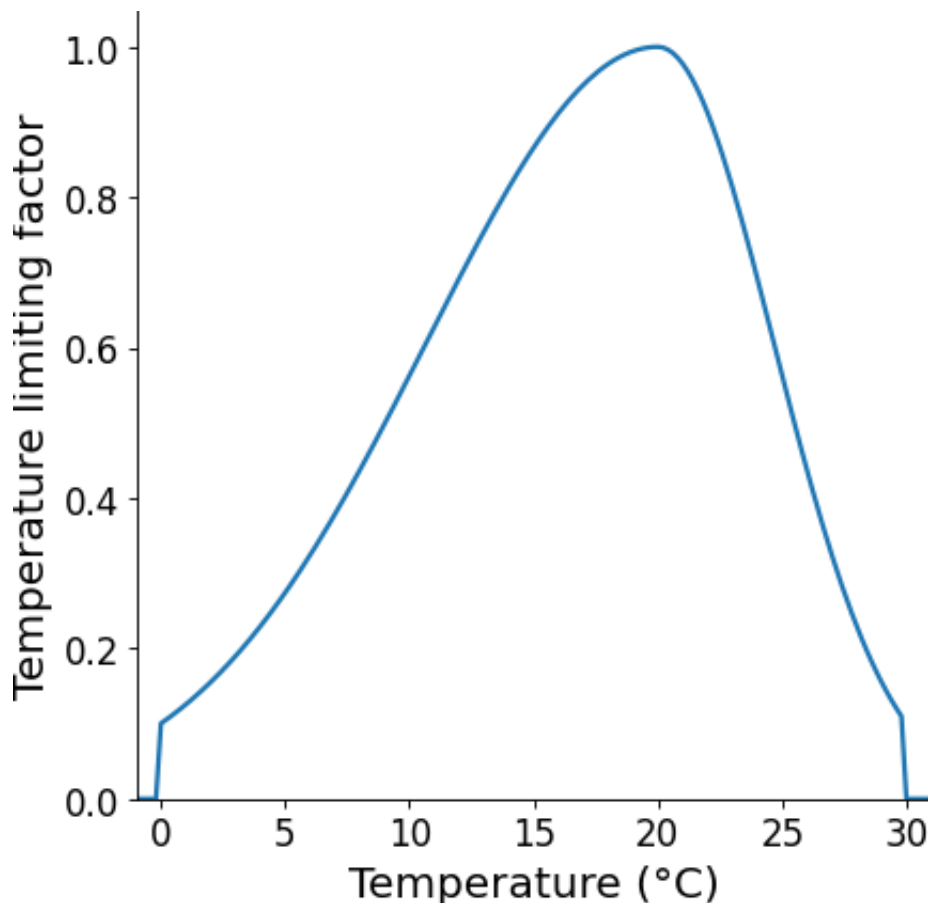


Figure A3. Temperature optimum curve of the macroalgae in MOS

- **Line 312: These numbers are highly dependent on the CN ratio according to the equations provided above. This links back to the issue raised in the major comment above.**

Response: Yes, we agree, see our response to your major comment. We will also add a sentence here, referring to the new sensitivity experiments with different C:N ratios and noting how our results depend on the fixed C:N:P ratio and that modeling macroalgae in this way leads to some uncertainty that may have been compensated for by tuning other parameters.

- **Section 4.3.3. In addition to vertical changes in nutrient distribution, it would have possibly been equally interesting to talk about horizontal changes in nutrient distribution and associated changes in productivity in the surface ocean. This nutrient robbing issue is in my opinion one of the biggest problems in for MOS but not investigated here.**
- **It seems like there is some information on this matter provided in B8 but the figure shows local changes, not quite what is needed for this I think.**

Response: We agree that horizontal changes in the nutrient distribution and associated productivity changes are important. We will add a few additional sentences on this to section 4.3.3 and include supplemental zonally averaged figures of nutrient changes with depth for the Atlantic and Pacific basins to illustrate these dynamics. The new text will read, "In addition to the localized depletion of nutrients by MOS, the MOS-induced Southern Ocean uptake and transport of N and P to the deep ocean acts as a type of "nutrient trapping". These dynamics thereby reduce nutrients and productivity in mid to low latitudes because less N and P are available to be transported northward out of the

Southern Ocean. A similar dynamic has been seen in modeling studies of ocean iron fertilization (Oschlies et al., 2010; Keller et al., 2014)."

- **Line 401: Should be B3**

In the manuscript the sentence "Therefore, the declines in zooplankton biomass (Fig.B3) agree with the declining phytoplankton biomass trend (Fig.B5)." appears to be correct as the figures in the Appendix are:

Figure B3. Plot of global averaged biomass of zooplankton.

Figure B5. Plot of global averaged phytoplankton biomass.

Thus we think the references to appendix figures are correct here.

- **Line 435: I wonder if the permanence of CO2 storage would be even longer if sediment carbonate dissolution due to high respiratory DIC at the bottom was occurring. Have you considered a CaCO3 sediment dissolution? May be worth mentioning.**

Response: Thank you for pointing this out. We agree that this is something to be explored in a further study that includes a CaCO₃ sediment model. We will add a line to the discussion section on this topic stating that, "Processes such as CaCO₃ dissolution at the seafloor, which could be affected by MOS and may impact its overall efficiency (in particular increasing durability of the carbon sequestration), also need to be included in future studies."

- **Line 540: Throughout the manuscript I was wondering why the sensitivity study without remineralization of biomass at the bottom was made? Is there any reason to believe this could be the case?**

Response: While we agree that it is unlikely that there would be no remineralization at all, having a zero remineralization case study allows us to do two things: First, we can use this sensitivity study as an extreme case of infinitely slow remineralization to help estimate the range of possible fates of remineralized organic matter. Second, this sensitivity study could represent a different macroalgae farming approach - that of harvesting the biomass to create bioenergy (on land, with carbon capture or storage) or biochar, with the assumption that all harvested biomass was permanently removed from the ocean. While this is a very idealized case, it serves the useful purpose of providing information on how marine biogeochemistry is impacted by the permanent removal of fixed C, N, and P. We have added the following sentences to the revised manuscript to better describe the idea behind this sensitivity study.

In the 5th paragraph of Sect. 3.2:

".... This sensitivity study can also simulate an extreme case of infinitely slow remineralization, which can help with estimating the range of possible fates of remineralized organic matter. Meanwhile, this sensitivity study could represent a different macroalgae farming approach - that of harvesting the biomass to create bioenergy with carbon capture or storage (BECCS) or biochar (e.g., Kerrison et al., 2015, Laurens et al., 2020, Roberts et al., 2015), with the assumption that all harvested biomass was permanently removed from the ocean. While this is a very idealized case, it serves the useful purpose of providing information on how marine biogeochemistry is impacted by the permanent removal of fixed C, N, and P."

In the ending of 6th paragraph of Sect. 5:

“These potential side effects are also noteworthy for another macroalgae farming approach, i.e. harvesting the macroalgae biomass for bioenergy with carbon capture or storage (BECCS) or biochar (Sect. 3.2).”

- **Line 555: What is the difference between MOS and ‘ocean afforestation’?**

Response: Ocean afforestation is a term coined by N’Yeurt et al., 2012 and referred to growing macroalgae which could then be harvested to produce biofuels, with the carbon captured and stored upon their combustion to achieve CDR. As this term added some confusion we will remove it from the sentence as it is not necessary.

- **Line 562: I find this argument not well thought through. We have argued in Bach et al., 2021 that one needs to consider the production of halocarbons (or DMSP or any other climate relevant substance) relative to phytoplankton and the re-allocated nutrient consumption. If seaweeds produce less halocarbons per nutrient resource than phytoplankton then halocarbon production may decline (even though spatial aggregation /re-allocation may occur).**

Response: Our argument was based on the assumption that per unit area, farmed macroalgae production of halocarbons is likely much higher than that of the pelagic phytoplankton communities that they would replace. Although of course, as we noted, macroalgae halocarbon production varies by species. However, from a global context you are correct, that if phytoplankton production of halocarbons decreased outside of the farmed area due to fewer nutrients being available, then what really matters is halocarbon production by either phytoplankton or macroalgae per nutrient resource. We will add text to this section to correct this oversight. The new text will read, “Large-scale MOS cultivation might release a significant quantity of halocarbons. However, as MOS also reduces global phytoplankton NPP, it is likely that the production of halocarbons by phytoplankton decreases. Further studies are needed to investigate possible net effects of halocarbon emissions from large-scale macroalgae cultivation and how this is offset by a potential decrease in phytoplankton halocarbon production.”

- **Appendix C: The RCP8.5 run is not really considered in the main text discussion. I see the value of it but may not be needed to have it in there if not discussed appropriately.**

Response: Thanks for the comment. We agree and have removed this from the appendix.