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Comment on esd-2021-104

Lennart Bach (Referee)

Referee 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-RC1, 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.

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 modelling 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. Later in the text the authors reference another modelling 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 come 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.

- 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:

A) 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.

B) 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.

- 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.

- 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. MNPP/(MNPP+PNPP)*100).

Other comments:

Line 54: Sherman Ref missing.

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).

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).

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

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.

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

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)?

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?

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?

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.

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

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.

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.

Line 401: Should be B3

Line 435: I wonder if the permanence of CO_2 storage would be even longer if sediment carbonate dissolution due to high respiratory DIC at the bottom was occurring. Have you considered a $CaCO_3$ sediment dissolution? May be worth mentioning.

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

Line 555: What is the difference between MOS and 'ocean afforestation'?

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 than halocarbon production may decline (even though spatial aggregation /re-allocation may occur).

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.