Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/esd-2021-104-RC2, 2022

Comment on esd-2021-104

Anonymous Referee #2

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-RC2, 2022

Authors present results of assessing the efficiency and impacts of macroalgal ocean sequestration (MOS) within an Earth System Model. The model assumes that MOS infrastructures appear throughout deep ocean sites where long-term sequestration is possible and macroalgal biomass will grow where there are adequate nutrients to sustain an annual crop. They then look at several MOS scenarios to address the long-term impacts on ocean ecosystems and the carbon cycle.

The paper is interesting and potentially important as many are looking to seaweeds as a CO2 removal (CDR) strategy. However, there are several issues that make it not quite ready for publication at this time in my opinion. I have issues with the overall premise of how the authors envision MOS, there are missing and odd elements of the farmed macroalgae model and the presentation is not adequate and the manuscript needs both a reorganization and some editing to make it more easily readable. I will detail these overall concerns and follow with specific comments by line number.

The basic MOS scenario created by the authors have created is in my opinion an unrealistic possibility which degrades the relevance of the model results presented. As I understand it, they are trying to assess is whether MOS (farmed everywhere it can) can by itself keep global temperatures within the Paris accord targets while still allowing moderate emission scenario (following RCP4.5). This seems to me to be an odd thing to test as I cannot imagine that actually happening. The recent NASEM report on ocean CDR suggests that portfolio of several CDR approaches is more likely solution to the negative emissions quandary. Further the presumption that MOS infrastructure can be deployed everywhere that macroalgae can grow requires a number of logistical hurdles to be overcome. Together, I am having a hard time understanding the actual relevance of these scenarios understanding the efficacy and impacts of MOS as a CDR strategy. Within that basic frame, the individual cases for stopping MOS, no decay of biomass and adding artificial upwelling cases, all make sense. But the overall premise does not, at least in my opinion.

I have some quibbles with the modeling that I think requires some discussion. Carlos Duarte and his colleagues have focused on the importance of recalcitrant dissolved organic carbon (DOC) that is released as the farmed macrophytes grow to long-term carbon sequestration. This mechanism is not included in the model nor is it discussed why it is not included. Given the long lifetimes (1000's years) of the recalcitrant DOC pool in the ocean, even a small fraction of recalcitrant DOC released during growth could be important. On another issue, I do not see the rationale to have zooplankton graze on farmed macroalgal biomass directly (the Trancoso et al. paper provides no observational evidence supporting this). It makes no sense to me that the same modeled organisms that would ingest phytoplankton would also affect the farmed biomass. Last, I am not convinced that biomass once harvested would be transported to depth without any losses. This assumption needs to stated or losses along the sinking path accounted for.

My last major issue is the writing – both organization and in its execution. There is no single statement of the high-level modeling goals, assumptions and scenarios to be used and the rationale supporting their validity. That information is spread out from pages 4 to 14 (and beyond), making the paper very hard to read and review. This information clearly needs to be in one place – right after the introduction. I am sure that a serious relook at the organization of the paper would really help its overall presentation. With regard to execution, you spend too much time referencing what you are doing that is similar to other works, but do not say in the text what you're actually doing. This is especially annoying in the model introduction. For example, in lines 96-97 you refer to models that yours is based on and then say how yours differs from these, but don't say upfront what your generic adaptation of their will actually do. Also, lines 131-134 were particularly obtuse, but there are many other examples throughout. The model introduction section needs to be understandable without making the reader refer to a stack of other papers. This is a correctable writing issue that made it hard to review the text but requires serious attention.

Detailed comments follow.

Lines 131-146 were hard to follow without having the macroalgae cultivation assumptions stated earlier and a separate section outlining modeling goals and assumptions is needed.

Lines 146-155 – Please tell the reader why is R_erosion a constant without having to refer to the reference.

Lines 204-214 – Definition of FR is out of order and needs to be moved down after the sunk biomass is defined.

Table 1 – What is the column denoted "Reference" referring to? The references in this paper are not numbered.

Line 236 – The units for Seed and KN do not match in Table 1. ???

Lines 244 and Fig 3 – It took me a long time to figure out what belts were referring to.

Line 256 – This topic sentence is not useful. Please state what are in the references.

Table 4 – Do not understand the difference between "selected area" and "belt" and why there are units there in km^2.

Lines 306-318 – The "validation" should be in a table comparing the NPP, yields, etc. from farms with the model results.

Lines 357 – Define PNPP before you use it.

Lines 375-376 – Not sure if that obvious statement is needed.

Line 377 – Is denitrification in the model? It is not stated in the model description.