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Interactive comment on "Modelling potential production and environmental effects of macroalgae farms in UK and Dutch coastal waters" *by* Johan van der Molen et al.

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General comments

The manuscript by van der Molen et al. deals with the representation of macroalgae – more specifically kelp – farms in the physical-biogeochemical model system GETM/ERSEM-BFM. Their study aims for the model-based analysis of their impact on the ecosystem as well as the productivity of such farms. A number of near-shore (four existing and one non-existing, but potentially suitable) farm sites off the British and Dutch coasts are used for this analysis.

The increasing interest in macroalgae farms, which is well described in Sect. 1.1,

provides a good background for this objective. Due to model limitations, e.g., with respect to spatial resolution or agreement with SPM or chlorophyll a observations, the authors clearly state that their study rather constitutes a "proof of concept" than a detailed analysis of the likely environmental impact and productivity of these farms (at end of Sect. 1.1).

Despite this clearly stated limitation, I consider the objective of the study as relevant and feasible for publication. However, I have a few major and some minor points that should be addressed by the authors.

My first major point of criticism is that the "environmental effects" included in the title are barely addressed in the manuscript (except for the information, that there are basically none in Sect. 3.2) and, therefore, either the title should be adapted or the manuscript should be extended with respect to that. For the relevance of the manuscript, I would propose to do the latter and will provide some suggestions in my specific comments below.

Second, a partial re-ordering of the Sects. 1 and 2 (and subsections), from my perspective, would clearly improve the readability of these parts of the manuscript.

Third, I would request a more detailed discussion of the model setup and the implementation of the kelp farms (and a more detailed description of the latter). For the latter, this could be especially relevant in relation to the potential environmental effects and may provide the reader with a better understanding of why the effects appear to be negligible in the present study.

Provided this more detailed description and discussion of the limitations and constraints, this study may serve as a first guideline for investigating environmental effects and performance of macroalgea farms using 3D physical-biogeochemical models. Therefore, I recommend reconsidering the manuscript for publication after major revision.

Specific comments

1. Most parts of the last paragraph of Sect. 1.1 (page 3, end of line 16 to line 21)

rather sound like a discussion/conclusion to me and I would suggest moving these parts correspondingly. Instead the authors could complete the short outline of the manuscript in this paragraph.
In this paragraph the authors also mention that the large-scale model allowed for the inclusion of all farms in one model (page 3, line 16). The phrasing makes it sound beneficial – which is indeed the case from a technical point of view (setting up one model instead of one for each study site). However, considering the related limitations, this should be discussed critically in the discussion section.

2. Organisation of Sects. 1.2 to 2.2.2: I had difficulties going through this part of the manuscript as it partly caused a back and forth reading. This mainly relates to the fact that the characteristics of the study sites (bathymetry, hydrography etc.) are provided in Sect. 1.2, followed by the description of the considered kelp species (*S. latissima*) in Sect. 1.3, again followed by a now more technical description of the setup of and – if existent – the sampling at the different farm sites in Sects. 2.1.
I see the difficulties with the separation/combination of Sects. 1.2 and 2.1 as the former is a more general description, while the latter is more of a methodological nature. However, considering the analysis of selected study sites as part of the methodological approach, I would propose to combine the currently two subsections on the individual study/farm sites into one subsection for each farm. These subsections should then also include the representation of the individual farms in the applied model setup (e.g., position in the vertical – are all farms in the surface layer of the model?). These (general and technical) descriptions of the study sites would then be part of the methodology section (Sect. 2), preferably after the description of the implementation of the macroalgae farms to ERSEM (Sect. 2.2.2). Current Sect. 2.2.3 would then become Sect. 2.2.4.
In relation to this suggestion, current Sect. 1.3 (description of *S. latissima*) would become Sect. 1.2. From my perspective, this is feasible as Sect. 1.1 already indicates that this study focuses on UK and Dutch sites, where *S. latissima* is a species of

interest.

3. Sect. 1.2.1 (Southern North Sea): As the North Sea study sites are both located in the south-western North Sea and kelp farms will most likely be placed in the shallower southern North Sea and coastal areas (I assume, correct me if I am wrong), I would shorten the North Sea description to the parts relevant for this region, and rather extend those parts slightly.
For instance, the Norwegian Trench and Skagerrak (page 3, lines 26-27) and the stratification characteristics in the central and northern North Sea (page 4, line 4) are less relevant for the context of this study, whereas the paragraph on North Sea primary production (page 4, lines 12-18) could be underpinned by some literature and more focused on the southern/coastal North Sea (those regions suitable for macroalgae farms).
Differences in productivity between the Norfolk and Rhine plume sites may be stated in the subsequent paragraph (page 4, lines 20-25).

4. Sect. 2.1.1. (Strangford Lough): I am no specialist in macroalgae at all, which is definitely one reason for the following two questions:
The actual farm consists of 2 lines with *S. latissima* (that is also implemented to ERSEM) and 19 lines with other species. Though, for the model 21 lines with the former are assumed. To what extent may this affect the results? Are there strong inter-species differences, e.g., with respect to nutrient requirements? Maybe a short note on that can be made in the discussion.
It is assumed "that the dry plant material consists predominantly of CH2O groups" (page 6, line 31) – is this a reasonable assumption? (Maybe underpin with literature.)

5. Sect. 2.1.2 (Sound of Kerrera . . .): The last sentences (page 7, end of line 10 to line 14) should be moved to the corresponding time series results or even to the discussion.

6. Sect. 2.2.2 (Macroalgae farms in ERSEM): It is stated that the farms are implemented to the model by means of number of lines, line length etc. for which the parameter values are provided in Table 2. However, no information is provided on how the actual description/implementation in the model is done, nor is a reference provided containing such description (if existent). From my point of view this step is quite essential when presenting the study as a "proof of concept". As I could not find any other literature attempting to include macroalgae farms in a large-scale 3D model, I assume that this manuscript presents the first approach. In this case the technical description of the implementation needs to be part of the publication – not necessarily as part of the main text, but in an (electronic?) appendix or as supplementary material. Regarding the model schema (Fig. 3) I also wonder whether the light climate in all parts of the water column below the farms is affected, i.e., including the below-farm parts of the grid cell with the farm itself, or only the grid cells below the farm grid cell. Depending on the vertical extent and the position of the farm, this may influence surface layer primary production by other algae (e.g., diatoms)

7. Sect. 2.2.3 (Model scenarios): For the farm scenarios, the model was run from 1 October to end of July of the subsequent year (page 9, line 33). Again a question as a non-specialist: How does this relate to the actual farming practice?

8. Sect. 3.1 (Model confirmation): Although it is probably the case, it would help to clearly state at the beginning of this section that the provided model confirmation/validation is based on the reference run.
With respect to the satellite vs. model comparison (Figs. 4 and 5) I wonder whether the map section could be reduced, more focusing on the regions of interest of this study (e.g., similar to Fig. 1). This would also allow for a more detailed description/discussion of the model quality in the areas of interest. Furthermore, it should be mentioned which months are used for the "summer", respectively, "winter" maps.

With respect to the time series (Figs. 6-8), I have several comments/questions:
On page 11, lines 15/16, it is stated that peak spring bloom chlorophyll a at Warp Anchorage (Fig. 6) is about 10 mg m-3. Does this refer to the observations as the model shows much higher values (up to 30 mg m-3)? If so, this should be made clear. Since chlorophyll is the main quantity used in the validation, it would help to provide brief information on how the chlorophyll concentration is calculated/derived (prognostic, diagnostic using fixed/variable chlorophyll-to-carbon ratios) by the model in the model description (a reference is sufficient).
I further wonder about the large discrepancy between simulated and observed salinity and in relation to that – as also mentioned by the authors – nutrients. Does this relate to the applied river forcing? Or are there other likely causes? Although not in the focus of the study, a brief comment on that would be useful.

9. Sect. 3.2 (Environmental effects): As stated in my general comments this aspect is barely addressed in the manuscript. The authors mention "maps of differences in biogeochemistry and plankton dynamics" without further specifying what kind of maps. Considering, e.g., that the kelp farms affect the light availability in the deeper model layers (as indicated in the schema in Fig. 3), I wonder whether only surface maps were analysed or whether quantities in deeper layers (affected by potential changes in the light climate) were also analysed? At least at the farm scale, I would expect changes in the light climate in the water column below each farm. Such change may not necessarily lead to distinct changes in the biogeochemistry, due to the small farm sizes or nutrient limitation, however, it may be used as an indicator when considering an up-scaling, i.e., larger farms. Therefore, I would propose to either specify what kind of quantities were analysed without including additional results, or to show and briefly discuss the difference map of one meaningful quantity (e.g., light availability in the deeper layers during the phytoplankton spring bloom period and/or corresponding spring bloom primary production). This would strengthen the manuscript with respect to that objective of the study.

Furthermore, the authors refer to “differences between the two reference runs” (page 12, line 7) – based on the scenario description (Sect. 2.3) I do not understand what the second reference run is? I suppose the scenario run (incl. the farms) is referred to? This should be made clear.

10. Sect. 3.3 (Strangford Lough time series): A short in-text definition of the structure-to-mass ratio (as given in the caption of Fig. 9) would be helpful. Principally, the authors may consider defining a Sect. 3.3 “Kelp farm performance/productivity” (or similar) with the current Sects. 3.3-3.8 as subsections (then 3.3.1-3.3.6), providing a more distinct separation from the “environmental effects”.

11. Figs. 9-13: panels k and l: What do negative uptake rates mean? Do the plots show the net uptake, i.e., uptake minus respiration? This should be clarified in the text. Panels c and d: To me it is not fully clear to what the provided extinction coefficients relate – is it the one in the water column above the kelp farms or the average of the grid cell in which the farms are located? I suppose it is the former as the figure captions state “excluding contribution of macroalgae”. However, it would help clarifying this in-text.
Similarly, I wonder about the irradiance – is it the irradiance at depth of the macroalgae, that directly at the sea surface, or that at the centre of the grid cell? Considering, e.g., the Rhine plume farm, with a line depth of 2m and high extinction coefficients, this may result in well different values.
It may help to include a brief general description of the quantities displayed in the time series at the beginning of the time series section.

12. Discussion/Recommendations: In relation to the authors’ statement early in the introduction (“this study is a proof of concept”) and the general performance/setup of the model (e.g., partly unsatisfactory reproduction of observed chlorophyll concentrations, coarse spatial model resolution), I understand that those two sections are

rather general and focus on the discussion of the results on the farm performance, and provide suggestions for improved analyses of environmental impacts and farm performance.
However, I would request a more detailed discussion of which limitations of the study affect the results in what way (e.g., low spatial resolution vs. small-scale environmental effects or small farm size vs. larger scale environmental effects). Part of this is already indicated during the course of the manuscript (e.g., small farm size in Sect. 3.2). Following the “proof of concept” approach of the authors, a discussion subsection dedicated to the limitations of the setup in relation to the model outcome should be provided, incl. potential effects of an improved setup, where applicable. Some of the potential limitations I raised in the previous comments.
Such subsection would also provide a good basis for the recommendations section, in which the suggestions for an improved study setup can be made. From my perspective, setting up the discussion/recommendation like this would clearly strengthen the “proof of concept” aim and provide a good basis for future, more detailed studies taking into account the suggestions by the authors.

Technical corrections/comments
1. The authors should go through the in-text citations thoroughly and check for consistency regarding punctuation (e.g., commas before years, semi-colons between multiple citations), ordering of multiple citations (chronological or alphabetical – if I am not mistaken the journal has a preference for one of the two), in-text author names and names in the reference list (e.g., “Grosse” in-text, “Große” in the references; in “van der Molen” the V is partly uppercase, partly lowercase).

2. When providing ranges of a quantity (e.g., 15-25m) or areal extents (e.g., 5x5km) I would recommend providing the unit after both numbers.

3. Abstract: Line 2: I would rather write “and for biofuel production”.

4. Introduction:
Page 2, lines 26/27 and 29/30: "associated with high biodiversity is doubled.
Page 4, line 13: "reduced" instead of "reducing"?
Page 4, line 15: "matter" instead of "material"
Page 4, line 31 to page 5, line 2: I would propose re-ordering these sentences such that there is no jump in the description from currents to depth and back to currents.
Page 5, line 4: the farm site "is" located (instead of "was")
Page 5, line 10: "The site range from 15-25m depth" sounds a bit odd. Maybe "The depth ranges from 15-25m at the site"?
Page 5, line 27: Would it be suitable to write "nitrate" instead of "nitrogen" as only nitrate uptake by kelps is considered?
Page 5, lines 23-30: I would change the order of the two paragraphs, as the latter is related to kelp in general while the former is region-specific.

5. Methods
Page 6, lines 25 and 30: "MPA" is used without introducing the abbreviation (line 25), while later "Marine Protected Area" is used (line 30).
Page 6, last paragraph: I would suggest to include a sentence stating that the conversion factor used in Table 3 (24.919) results from the combination of the two factors listed in this paragraph.
Page 7, line 17: The abbreviation "ROFI" is introduced on page 3, line 11, so simply use ROFI here.
Page 7, line 29: I think, 2 digits after the comma are sufficient for the geographical information of the site.
Page 8, Sect. 2.2.1: Some links are shown as hyperlinks, others are not. Should be consistent.
Page 9, line 15: typo in "diynamics"
Page 10, lines 16/17: Should the parenthesis be closed after the link, and the closing

parenthesis at the end of the sentence be removed?
Figure 2 has a rather poor quality/low resolution and the green box indicating the Norfolk kelp farm is hard to find as the map contains a lot of information. I wonder whether this map is actually necessary or if the in-text description is sufficient. If the authors prefer to keep this figure, a smaller map section may help – or addition of an arrow pointing to the farm site. In case of keeping the figure, its resolution needs to be increased.

6. Results
Page 12, line 21: I think it should be 0.05 kg C m-1 instead of 0.5 g C m-1.
Page 12, line 24: typo in "carbohydratate"
Page 13, line 13: It is stated that Lynn of Lorne shows a slightly higher yield than Sound of Kerrera. There's a factor of 2 between most of the years that is clearly more than "slightly higher".
Page 13, very last sentence (ending page 14): should be moved to the description of the study site and its representation in the model in the methodology section.
Page 14, line 3: "Mortality did not increase as much as at some other sites". To my understanding this only applies to the Lyne of Lorne farm among "the other sites".
Page 14, Sect. 3.7 and Table 3: As the in-text description only refers to the wet biomass, I would display these numbers first in the table and show the harvest in parentheses or even omit the harvest and only state in the table caption that wet biomass was calculated from the harvest with reference to updated methods section (see my previous comment on the conversion factor).
I would further omit the information on the individual Norfolk farm grid cells in the table, as the differences are quite small. Related to that the hint on the differences in the discussion could be removed (page 15, lines 22-24).
Figs. 9-13: Panels d: The unit of the irradiance is μmol m-2 s-1, however, in Sect. 3.8 (page 14, lines 14 and 23) μE m-2 s-1 is used. Although both are in fact the same, consistent usage of one of the two units (preferably the latter) might be helpful for the

reader not too familiar with this irradiance unit.
Panels k: Those could be named as nitrate uptake (as the parameterisation by Bloch and Slagstad only considers nitrate uptake).
Fig. 14: I would recommend re-ordering the figure panels according to the order in which the study sites are described in the text and presented in Figs. 9-13 (excl. Lyne of Lorne)

7. Discussion
Page 15, lines 12-14: It is stated that kelp production at Rhine plume was lower than in the Sound of Kerrera and Lyne of Lorne. For the former, this is obviously not the case (see Figs. 10f and 12f). Also, light availability and extinction are quite similar for Rhine and Sound of Kerrera sites.
Page 15, lines 19-21: There is a factor of 2 between the production per metre between Norfolk and Sound of Kerrera, which I would not describe as “comparable”.
Page 16, line 2: “although” in combination with “however” sounds like a doubling to me. Maybe omit the latter?
Page 16, line 25: a period is missing in “eg.”

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