

Anonymous Referee #1 Received and published: 14 July 2017

The manuscript with the title "Modelling potential production and environmental effects of macroalgae in UK and Dutch coastal waters" by Johan van der Molen et al. provides estimates for existing and hypothetical seaweed farms within the UK and the Netherlands based on the simulations with a 3D marine ecosystem model. For the production sitethemodeldoesnotonlyprovideestimatesfortheoverallproductionbutalsoonthe quality in terms of carbon content of the macroalgae. In contrast, the summary of the environmentaleffectswasthattheywerenotdetectablewhencomparingthereserve run and the scenario run. For my understanding the later conclusion is too simple to justify the expression "environmental effect" in the title. The authors should re-think if they can support this claim in the title, some hints will be expressed in detail below, or drop this part in the title. Since clarification are needed on the production part as well, the manuscript should be published after major revision.

My suggestion to back up the claim to provide information related to "environmental effect" would be to calculate the flux of nutrient and carbon uptake by the farmed macroalgae in comparison to the phytoplankton usually used in the model. The nitrogen and phosphate uptake is already presented as time series for each farm site in Fig. 9 – 12 in the graphs k and l. With this quantitative information one could underpin in which way the phytoplankton, in the grid call where the aquaculture is applied, was still able to develop in a way that is not detectable by simply plotting concentration differences. Especially at sites where the nitrogen concentration is depleted in summer it would be very interesting to see how the phytoplankton could manage its uptake in comparison to the newly introduced macroalgae. In addition this information could be supported by the rather simple calculation of the nutrient content in the water column exposed to farming both in winter and in summer, or other crucial important period like autumn. In doing so one can still not define in which way changing transport of either nutrients or phytoplankton led to the indistinguishable concentration differences between the two runs, but one would provide some quantitative background information on the "environmental effects".

In principle, comparing the nutrient uptake with that of phytoplankton to get more information on environmental effects is a good idea. However, from a practical point of view this is not possible with the current simulations as nutrient uptake by phytoplankton was not stored, and we are not in a position to repeat the model runs. From a biological point of view, it is not immediately clear what the meaning of such a comparison would be, because i) phytoplankton is advected by the currents, hence spends only a limited amount of time within the farm area and may compensate when it arrives elsewhere; within the farm area the phytoplankton population is continuously being replaced; ii) because of their much larger surface to volume ratio, phytoplankton out-compete macroalgae in terms of nutrient uptake, the niche of macroalgae is their ability to buffer and grow on nutrients accumulated in winter when phytoplankton can't grow. We did not only compare nutrient concentrations, but also phytoplankton biomass and found no significant change. The text will be updated to clarify this. We will remove the environmental effects from the title.

One more small detail, in chapt. 3.2 "Environmental effects" the "differences between the two reference runs" (Page 12, line 7) should rather be the difference between the reference run and the scenario with the seaweed farm application or is this a misinterpretation?

The text is correct. Because the model does not reproduce values exactly between two 'identical' runs, only differences between the run with farm and the first reference run that exceed the difference between the two reference runs are considered significant. We will adjust the text to make this clearer.

On the other hand the study goes beyond a simple feasibility study by providing information not only on the potential harvest that can be expected but also about the seaweed quality in relation to the use for biofuel in terms of the carbohydrate content. Especially for this “fledging industry” the relation of nutrient limitation leading to higher carbon content in context with higher product quality should be expanded into more general consideration that go beyond the individual site description. With Horizon 2020 calls on the co-use of technical structures like offshore windfarms in the North Sea for aquaculture or seaweed farms, this aspect has the potential to go beyond a pure coastal application and would raise the impact of this study towards more general consideration in this context. These additional consideration would compensate the problem that the model over- or underestimates the individual nutrients and/or chlorophyll-a concentrations at different validation or farm sites, so that it is difficult to draw more general conclusions from these aspects of the model performance.

This is a very good suggestion, but such work should be done thoroughly using a combination of experimental and 1D modelling work to rigorously cover the relevant parameter space. The current model results would only provide a few scattered ‘samples’. Work is in progress at NIOZ to start to address the effects of e.g. environmental conditions and time of harvest on carbohydrate and protein contents as well as composition in different native North Sea macroalgal species; we do not think it’s advisable to use the current results for this purpose.

When describing the nutrient validation results for Sound of Kerrera farm site (Fig. 10a and b), the question is not only on the nutrient concentration reached in comparison to the measurements. The more profound question is why the measurement show a rather different cycle compared to the simulated concentration. Is there any local source that is not reproduced in the model that brings about these differences?

This is a good point. We have wondered about this, but do now know of one. There could be other causes such as higher local re-generation than modelled. Without additional information, we thought it best not to speculate here, and still have that opinion, so we will not make changes on this point.

Since the mortality term is one of the key parameter for the seaweed yield a more detailed description of this process is needed. The simple hint toward “erosion” does not help the reader in which way the mortality process interacts within the simulation throughout the year. As a simple example, when at site A (Strangford Lough) the macroalgae biomass is about an order of magnitude lower than the observed one but the mortality is low throughout the farming cycles, one would interpret that the light and/or nutrient condition were not suitable in the model to efficiently reproduce the measured biomass. This implies that the mortality pressure is not an important factor when it is applied as proportional to the biomass, but this is not clear from the incomplete explanation provided.

The model uses the same equation that relates apical frond loss exponentially to frond area as used by Broch and Slagstad. We will add a few words to this effect.

Smaler details: For a more simple way of attributing the different sites, the characterisation A-G as used in Fig. 1 should be used as site specification next to the full locality description of the farms throughout the paper.

We have added further references at the first use in each section.

In this context the site specification for each farm and validation location should be highlighted in a different colour in Fig. 1 rather than simply black.

We think that the letters and numbers provide sufficient distinction.

In addition, since the Doggerbank and the Norwegian Trench are used in the characterisation of the North Sea hydrodynamics, these features should be more pronounced in the topography map in Fig. 1, e.g. in the smaller map showing the full domain.

We have changed the colour scale to make these features stand out more.

Is Fig. 2 in this full detailed information overview really needed?

Yes. Lumping all human use into one category would raise the question to provide more detail.

The general statement on the model confirmation that “These results are not reproduced here” (page 10, line 27) should be placed on top of this subchapter.

We cannot implement this suggestion in a form that would lead to a more legible text.

Page 20 line 19 The first author is Jo Folden not Foden

The text is correct.

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 macroalgae 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.

This section describes, among others, the approach. It is important that the reader is aware of the limitations from the outset so he/she can put the results into perspective as they appear. In our opinion, this is more useful than a description of the structure of the paper, which is very standard.

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.

We will add a line to the recommendations stating that high resolution simulations are desirable for the Norfolk farm.

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.

We have adhered to the classical structure of a scientific paper in which general knowledge is summarized in the introduction, the methods are described in a methods section etc. We also think that observations should be presented before models (but have in this case put the smart-buoy and satellite data at the end so as not to interrupt the flow between the farm observations and farm implementation in the model). We recognize that there may be other ways to organize this, but adhering to the classical structure of a scientific paper is preferable and makes it easier for the reader to find the information and to know what's old and what's new.

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).

We will remove reference to northern areas. We will add a line on differences in productivity between the Norfolk and Rhine plume sites. We will have a look at suitable references for North Sea primary production.

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.)

*We have only used field observations from the *S. latissima* long lines. We will add a sentence stating this explicitly. CH₂O groups: we will add a reference to Atkinson & Smith (1983).*

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.

We will remove this sentence, an equivalent statement was already included in section 3.4.

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.

It's simply a biomass concentration, like all other state variables in ERSEM. We will add a line to make this point more clearly.

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)

Both. We will add a line to make this point more clearly.

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?

Yes, this is a likely farming practice. We will add a statement to this effect.

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.

We will add this.

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.

We will change this and modify the text accordingly.

Furthermore, it should be mentioned which months are used for the “summer”, respectively, “winter” maps.

This information was in the figure captions, but will be added to the text as well.

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.

This should be: 'modelled peak spring-bloom chlorophyll concentrations were within 10 mg Chl m⁻³ of the observations'. We will correct this.

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).

Chlorophyll is a state variable and the model uses variable chlorophyll to carbon ratios. This information can be found in the list of references provided at the beginning of Section 2.2.2. We will add mentioning chlorophyll in that description.

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.

This may indeed be related to the river forcing or to resolution-related issues with the representation of the river plume. We will add a remark to this effect.

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?

This is described in section 2.2.3, with reference to an earlier paper that contains a more extensive description. The maps contained depth-averaged results, this was not mentioned and we will add this.

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.

As mentioned in section 2.2.3, we analyzed all stored model variables. We did not find significant effects for any of these, i.e. all the maps were essentially blank (give or take a few isolated spots). We would have liked to be able to present some results here, but we don't think it is useful to include and discuss a blank map... It may be possible to find short(er) periods of time that do show differences, but the objective was to look for substantial, meaningful changes, so we did not look at that level of detail.

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.

This is indeed an omission. Re-running the model with the same settings, forcings, etc. gives slightly different results. So we ran two instances of the reference run to provide a baseline difference that the scenario run needs to exceed to indicate a significant difference. We will expand the description in section 2.2.3 to reflect this.

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.

We will add this.

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”.

We will adopt this suggestion.

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.

We will change the caption to ‘net uptake’.

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.

The caption clearly states that this is at the surface.

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.

The caption clearly states that this is at the surface.

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.

We will insert a paragraph in the discussion on the limitations of the model.

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).

We will address this.

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.

We will take guidance from the editor on this.

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

We will change this.

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.

We will change these.

Page 5, line 4: the farm site “is” located (instead of “was”)

No, the text is correct, this farm is no longer in operation.

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”?

We will correct the text.

Page 5, line 27: Would it be suitable to write “nitrate” instead of “nitrogen” as only nitrate uptake by kelps is considered?

The text was correct, but we will change it into ammonium and nitrate and also modify Figure 3.

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.

The first of these two paragraphs is related to the one before, so we will keep the current order.

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).

We will correct this.

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.

We will add this.

Page 7, line 17: The abbreviation “ROFI” is introduced on page 3, line 11, so simply use ROFI here.

We will change this.

Page 7, line 29: I think, 2 digits after the comma are sufficient for the geographical information of the site.

As this is a matter of taste, we will retain the current precision.

Page 8, Sect. 2.2.1: Some links are shown as hyperlinks, others are not. Should be consistent.

We will remove the hyperlink.

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?

We will correct these.

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.

The map will be provided at adequate resolution. The map is necessary, and the information cannot be reduced or simplified.

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 “carbohydrata”

We will change these.

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”.

We will correct this.

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.

This is a chicken and egg situation because we need the result of the high extinction coefficients for this. So we prefer to keep it here. It is also included in the footnotes of table 2, which is referenced in the methods 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”.

We will remove the sentence.

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).

We have included both number to relate to different communities used to different units. If the editor insists we can swap the order.

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).

We think that the gradient is illustrative, and may be useful if a smaller farm is implemented than simulated here.

Figs. 9-13: Panels d: The unit of the irradiance is $\mu\text{mol m}^{-2} \text{s}^{-1}$, however, in Sect. 3.8 (page 14, lines 14 and 23) $\mu\text{E m}^{-2} \text{s}^{-1}$ 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.

We thought that we had removed all instances of the μE unit, and will correct these as well.

Panels k: Those could be named as nitrate uptake (as the parameterisation by Bloch and Slagstad only considers nitrate uptake).

Our implementation also includes ammonium uptake, but values are likely to be small.

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)

We will change the order of the graphs.

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).

This is indeed an error, we will change this.

Also, light availability and extinction are quite similar for Rhine and Sound of Kerrera sites.

The base-line extinction at Kerrera is 0.2 m^{-1} , and at the Rhine site 0.5 m^{-1} . This is not quite similar.

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”.

We will correct this.

Page 16, line 2: “although” in combination with “however” sounds like a doubling to me. Maybe omit the latter?

We will change this.

Page 16, line 25: a period is missing in “eg.”

We will change this.

Anonymous Referee #3 Received and published: 8 August 2017

I am the 3rd reviewer of this manuscript and, having read the other reviews, I agree with most of the comments made by the reviewers so I am not going to repeat those, although I will highlight specific ones I find more relevant, and I will concentrate on a general evaluation. Overall, I found the article interesting and eventually deserving publication. The article is well written but I agree with Fabian Grosse's comments 1 and 2 about structuring and organisation.

Please see our responses to Fabian's comments.

I have a number of general comments, below, that I think should be considered before the manuscript is acceptable for publication.

I has been highlighted by both the authors and the reviewer that this is largely a "proof of concept" study and I agree with this. In that sense, I am not particularly surprised that the environmental effects of the macroalgae farms were found to be negligible, taking into account the experimental and small scale nature of all of these farms, with the exception of the "hypothetical" one off the Norfolk coast. Incidentally, the suggestion by Referee #1 of using a "comparison to the phytoplankton usually used in the model" may be an interesting way of putting their environmental effect into context.

Please see response to comments of Referee #1.

Another consideration is that ERSEM does not seem to do such a good job of matching observational data. This does not surprise me because I fully acknowledge the difficulties involved (e.g. due to lack of a sufficiently comprehensive forcing dataset, etc.) but it does detract somewhat from the "real-life" applicability of the present study. Therefore, I would be tempted to suggest a lesser emphasis on how the model replicates observational data (if you have those data, by all means present the comparisons but maybe just present a subset of these in the main body of the text and move the rest to supplementary material) and to dedicate more space to:

1) the technical description of the new aspects of the model (the implementation of macroalgae farms, as suggested by Fabian Grosse's comment no. 6), probably by a combination of providing a bit more detail in the main text, in addition to supplementary material;

Strictly speaking, the model does not contain new aspects, just combinations of existing methods. The references to these should be sufficient. We will enhance the description to provide more clarity, and include a table with the parameter settings for P.

and 2) the potential large scale development of macroalgae farming and its environmental effects. In that sense, I found the combination of a number of "real" small scale farms and a large "hypothetical" one a bit unsatisfactory; maybe the real farms should be used to illustrate how well the model fits observations (but taking into account my comments above) and a number (>1) of larger ("commercial") scale hypothetical farms could be used on a separate simulation exercise to illustrate the harvest potential (in terms of quantity and quality) of a macroalgae farming industry.

This is exactly what is intended with the current manuscript, and this is stated in the introduction. For the hypothetical commercial farm, we have focused on a realistic possibility that is supported by regulatory bodies, rather than selecting a range of sites more or less at random, which could cause controversy. Moreover, we are not in a position to repeat the model runs within the current context and publication. This could be considered for further work.

This relates to the comment made by Referee#1 about "more general consideration that go beyond the individual site description".

Please see our response to the comments of Referee #1.

This may also help address the issue of potential environmental effects, which in the present version (as identified by both reviewers) receives insufficient attention to deserve an explicit reference in the title.

We will remove the environmental effects from the title.

Finally, the model limitations (Fabian Grosse's comment no. 12) should also be discussed, in particular scale and resolution aspects, as well as what aspects of the model should be developed and how.

See response to Fabian's comment.