

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.

Anonymous Referee #1

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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 site the model does not only provide estimates for the overall production but also on the quality in terms of carbon content of the macroalgae. In contrast, the summary of the environmental effects was that they were not detectable when comparing the reference 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

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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 is 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”.

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?

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 Hori-

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zon 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.

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?

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.

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. 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. In addition, since the Doggerbank and the Norwegian Trench are used in the characterisation of the North Sea hydrodynamics, these features should

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be more pronounced in the topography map in Fig. 1, e.g. in the smaller map showing the full domain.

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

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.

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

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