

Interactive comment on "Using coupled hydrodynamic biogeochemical models to predict the effects of tidal turbine arrays on phytoplankton dynamics" by Pia Schuchert et al.

Anonymous Referee #1

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This paper aims to investigate the local scale (< 30 km) impact of tidal turbine (TED) arrays on phytoplankton dynamics (i.e. mean and peak concentrations) in coastal and inshore areas around the British Isles. An unstructured 2D hydrodynamic model coupled with a simple NPZD model was used to simulate this impact in an idealized domain. The results suggest that, in an idealized environment, the presence of TED arrays leads to a slight increase in water residence time followed by a decline in phytoplankton concentrations; however, such impact is much smaller in comparison to other environmental variables that are subject to substantial natural variability, such as PAR as demonstrated by the model. This is one of the first attempts to investigate the local scale impact of large TED arrays on lower trophic level food web dynamics, and I

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agree that the results presented in this manuscript will ultimately shed light on water resource management and guide policy decision making. However, in order to satisfy the interests of BG readers, a good number of concerns must be addressed in a revised manuscript before publication can be considered. Some of my quibbles (major points and specific comments) are listed below.

Major Points:

1. The importance of nutrients and light to phytoplankton growth has been well known. It is also convincing that natural variability of these variables can be substantial, therefore scenario experiments should be conducted (4 PAR scenarios \times 2 TED setups) in order to understand the relative impact of TED arrays. However, it does not seem to justify that the full range of natural variability in PAR can be sufficiently represented by 'random' measurements made between 2004-2014. Which four years were selected? Are those data year-round, time-series measurements? How do they look like, and how does seasonal variability compare to interannual variability? Did the authors choose the four PAR scenarios to represent high, medium, and low light conditions? The authors should provide these observational data in the manuscript and discuss this further in depth. I would also recommend the authors to analyze remotely sensed observations (e.g. 4 km MODIS) to assess the long-term variability of PAR in that area.

Along the same line, why did the authors not include nutrient scenarios in their numerical experiments? The authors mentioned that anthropogenic eutrophication may result in serious environmental issues in coastal regions. While the model has such capacity (e.g. by changing the initial nutrient concentration), the impact of eutrophication on phytoplankton is not being addressed in this study.

2. I understand that it is not necessary to describe the detailed setup of the hydrodynamic model as it follows a previously published study. The authors mentioned that the hydrodynamic impact of TED arrays is presented as sub-grid structures using a drag-law; however, I suggest the authors should at least describe the equation of drag force as well as other model parameters (e.g. turbine thrust coefficient) they used in this study. Also, the resulting modifications in current velocities and flushing time might not play a major role in driving phytoplankton growth. The simplifications made in the 2D model simulations (i.e. omitting vertical variability and benthic flux) is in the meantime ignoring physical processes that are critical to phytoplankton. For example, turbid turbine wakes (up to 150 m in width and several km in length) have been frequently observed from satellite images (e.g. Vanhellemont and Ruddick, 2014). These wakes not only increase light attenuation thereby reducing light availability, but also enhance nutrient supply to phytoplankton. These caveats should be further discussed in the manuscript.

3. The authors chose to run two model simulations, one with no turbines and one with an extremely large TED arrays (55 turbine). How was this number (i.e. 55) chosen? The hydrodynamic model suggests that the effect of TED arrays on flushing time is very small (5%). Are the modeled results sensitive to the setup of TED arrays at all (e.g. turbine location; number of tidal turbines; turbine parameterization)? Is 5% within model error? The authors should perform tests to determine the sensitivity of the hydrodynamic model results and the robustness of the conclusions on the effect of TED arrays on hydrodynamic processes.

4. The linkage between the deployment of TED arrays and the modeled decline in phytoplankton concentrations was poorly discussed in the manuscript. I would assume that nutrient flux across the channel is reduced with the presence of TED arrays, resulting in a decrease in phytoplankton growth. It is surprising and unclear to me why zooplankton grazing rate would be enhanced instead.

Minor Points:

1. '... following Fennel and Neumann (Fennel and Neumann 2015)' should be '...following Fennel and Neumann (2015)'

2. P.3, I. 25-28: Eq.(1)-(4) should be Eq.(2)-(5).

3. P3 I. 30-40: I suggest the authors tighten up the wording here and move some of the model descriptions into the caption of Fig.2.

4. P.4, I. 8-9: "Initial concentration..., without dimensions, respectively". Here the authors claimed that the 4 variables used in this study are non-dimensional. However, at P.5, I. 7-9, the authors added unit for the phytoplankton concentration, please clarify or eliminate.

5. P.4 l.16: Eq. (2) should be Eq.(6).

6. P.4 I. 22-24: The authors write "While nutrient concentrations,.....": I do not agree with this statement. Indeed the variables in NPZD model are dependent on each other. However, the results for each variables also have unique phenomenon. Fig.4a and Fig.6 gave good examples – a lag between phytoplankton production and grazing by zooplankton. Also the other variables could be useful to explain the reason why phytoplankton concentrations decrease due to the deployment of TED arrays. I suggest this sentence be removed and the authors should provide more modeled results.

7. P.4, I. 30: Lindeman, Merenda and Gold (Groemping 2006, Lindeman et al. 1980); what is the reference for Lindeman, Merenda and Gold? Is Groemping 2006 and Lindeman et al. 1980 referencing Lindeman, Merenda and Gold?

8. P.6, I. 37-38: Please add reference for this sentence.

9. P.6, I. 39-40: "van der Molen et al (2015)" should be "van der Molen et al (2016)".

10. P.14, Fig.4: Should use same upper bound of y-axis for all the three figures.

11. P.15, Fig.5: The caption of Fig5 is not complete. Please check.

12. P.16, Fig.6: Why was only zooplankton for Scenario D shown? What about A-C?

13. In the sections of results and discussion, only the averaged (over several sampling locations) phytoplankton concentrations have been shown for the three different areas (e.g. Fig.5; Table 2). However, spatial variation is expected within each area (especially

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inside the basin). Such variation should be presented as standard deviation in each table cell.

14. P.20: Table 3: The authors write "Results of General linear models for mean, max and cumulative values of phytoplankton and maximal and cumulative value of nitrogen." This sentence is not consistent with the text (P.5 I. 12-14) "Linear regression and hierarchical partitioning for annual mean and maximal concentration of phytoplankton with". Please check and clarify.

In summary, the authors present a potential minor effect of tidal turbine (TED) arrays on phytoplankton concentrations in an idealized environment using a coupled hydrodynamic and biogeochemical model. This manuscript might be potentially of interest to the BG readers. However, the current version of manuscript is structured more like a report. The description of the coupled model and the discussion of model results are superficial and the authors should spend additional effort to improve it. The authors should also pay attention to the formatting of references in the text. I recommend the editor reject this manuscript, but permit a resubmission upon a major revision that addresses the concerns listed above.

References:

Q. Vanhellemont, K. Ruddick, Turbid wakes associated with offshore wind turbines observed with Landsat 8 Remote Sens. Environ.,145 (2014), pp. 105–115

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C5