

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 #2

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This manuscript presents a modeling study of hydrodynamics and biogeochemistry, aimed at evaluating the effects of increased drag (due to turbines) within a tidal inlet on phytoplankton dynamics. The paper is well-written and relatively clear, though lacking in detail throughout. Given the highly idealized nature of the study, I do not think the results are generally applicable to other systems, or provide any specific insight that couldn't be otherwise attained through a simple "thought" experiment.

For example, the primary result, i.e. residence time increased and had a measurable effect on phytoplankton concentrations, is specific to this idealized model domain. The magnitude of this change is directly a function of the inlet and basin configuration and turbine density. If the study evaluated multiple instances of configurations and

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densities, and generated a more widely applicable evaluation of residence time (and phytoplankton concentrations), then I believe this would be of broader interest.

The use of a 2D model, though defended in the discussion, is a major shortcoming. Given the depths in the model, it is unclear to me how the vertical profile of light, primary production, and phytoplankton concentration can be properly resolved. The assumption of a well-mixed water column, through a h/u³ criterion, is never actually quantified anywhere in the paper. And regardless of this criterion, one would expect significant vertical structure during neap tides when mixing is reduced.

The specification of drag force is never clearly explained. Representing momentum extraction and turbulence dissipation by structures is complex and needs to be explained in more detail. Again, given the depth of the channel, representing the three-dimensional structure of the hydrodynamics may be important.

The offshore, inlet, and basin configuration used here seem relatively uncommon. I cannot think of realistic systems with such a configuration. If the authors believe this to be some prototypical system, then more justification should be given.

Specific comments:

Abstract: more detail is needed here, regarding findings and implications.

Section 2.1: more detail needed on model setup, drag parameterization, forcing conditions. Mixing criterion is described here, but not evaluated anywhere else in the paper.

Section 2.2: more detail needed for PAR input specification. How is PAR averaged over such large depths to yield a realistic value for phytoplankton growth?

Section 2.3: Why is a 4-year spin up needed, if concentrations appear to fall to zero in the winter?

Section 2.4: This quantification for residence time is relatively primitive and should be compared with values obtained through particle tracking, dye release, or other means.

Again, vertical structure of hydrodynamics will be important in a system with these depths.

Section 3: There is almost no detail given for hydrodynamic results. What is the spatial structure of velocity? What is the spatial structure of the mixing criterion on spring and neap tides? When would the assumption of well-mixed conditions fail? How would that affect the implications?

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