Responses to reviewers' comments on the manuscript "Effect of tidal stream power generation on the region-wide circulation in a shallow sea", by G. I Shapiro. Submitted to Ocean Science Discussions (OS-2010-47).

REFEREE #3

General Comments

The premise of the paper, i.e. that a 3D baroclinic tidal resolving model can be used to investigate the impact of tidal energy extraction, is sound and will provide useful understanding for the development of tidal energy schemes. The paper would benefit from some improvements to the experimental design and needs some tidying/ improvements with respect to the quality of the text and figures.

Specific Comments

COMMENT: The assumption of linearity in the friction term does not seem justified or neccessary, especially given the Bjorn et al and Wu papers cited do not seem to be relevant to this case. A more realistic quadratic equation (6) would negate the argument in the last paragraph of section 2 that efficiency would decrease with increasing flow as the Power and KE terms would both be proportional to Uˆ3.
RESPONSE: I cannot agree that a 'quadratic equation' is 'more realistic'. The reason is that the thrust coefficient representing the drag exerted by the turbine on the flow is not a constant but a decreasing function of the flow speed (see e.g.Figure1 and 2 below), thus making a linear (not quadratic) approximation of the force a much better option. This important feature of the flow through turbines immediately translates in the Extracted Power being proportional to U^2 in contrast to the overall energy flux being proportional to U^3. Hence the loss of efficiency of turbines at higher speeds.

I admit that the use of a non-constant drag coefficient and the linear frictional scheme is an important fact and it needs a better explanation in the paper. To address this issue, in the revised version an extended justification to this effect is added which includes a new sub-section of the text (between Eq(5) and(11)), extra citations, and a new Fig(1a) - a copy of this sub-section is included in the responses to Refereee1. Reference to the Wu et al paper has been removed as advised.

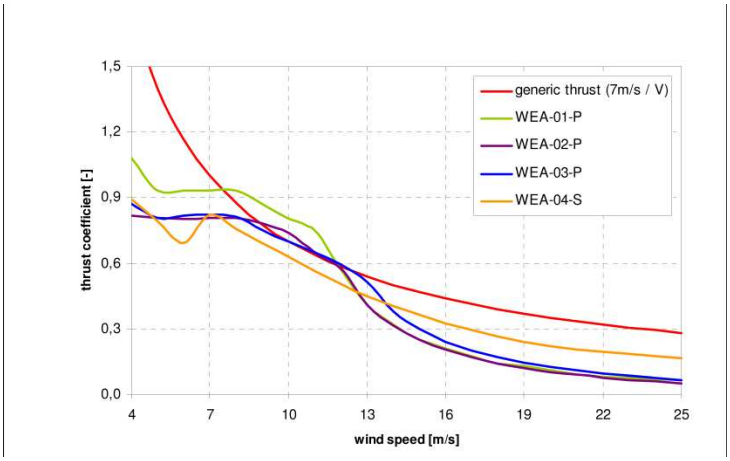


Figure1. Thrust coefficient as function of wind speed , from Frohboese and Schmuck (2010)

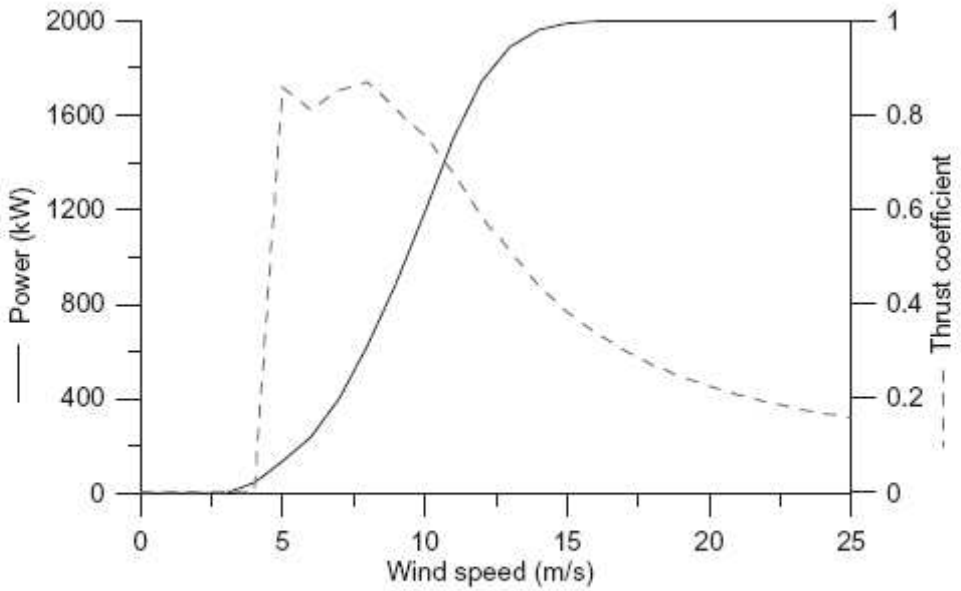


Figure 2. Bonus 2MW turbine power curve (P) and thrust coefficients (Ct)

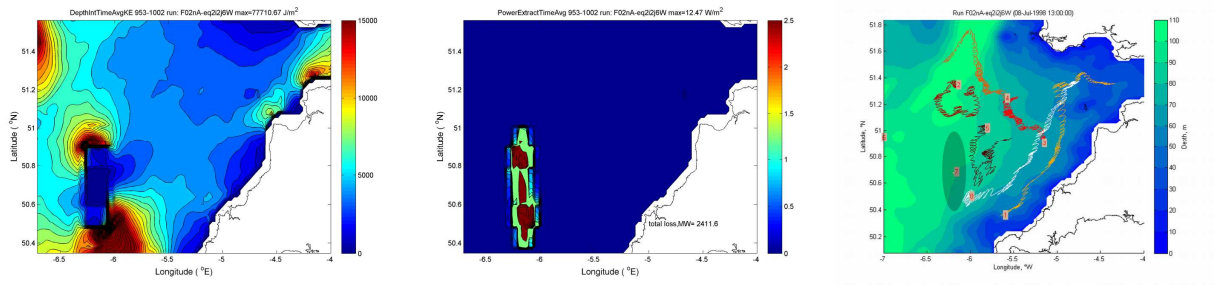
from R. J. Barthelmie et al, Wind Energy , 10, 517–528, 2007.

COMMENT: The fig 1 power curve would also seem to be fitted better by a cubic, not quadratic.
RESPONSE: No, the cubic curve gives a worse fit.

COMMENT: Equation 8, which describes the Rayleigh Coefficient and therefore represents the tidal energy extraction intensity seems a strange function to use. A more realistic option would seem to be to have constant coefficient where there are sites, zero elsewhere.
RESPONSE: The Gaussian distribution of the Rayleigh Coefficient in the paper is a smoothed out version of what is proposed by the referee. Some smoothing is needed to avoid numerical instabilities.

COMMENT: It would also be helpful to experiment with different shapes/sizes of the installation. An installation that is a line of turbines placed perpendicular to the major axis of the tidal ellipse would seem an obvious choice. A cup shape to catch more of the refracted tidal wave would also seem an interesting test.
RESPONSE: I agree that this exiting subject (in-stream tidal farms on the shelf) generates a wide range of ideas to be explored. However the detailed investigation of different shapes, locations other parameters of the farm goes far beyond this initial paper. Just for comparison I added Fig.8. showing results for a ‘linear’ rather than

‘circular’ farm. The linear farm has the same number of turbines, it generates more energy but produces a greater disturbance to the currents.



New Fig.8 (KE, Extracted power, drifter tracks)

COMMENT: Fig 6 shows the depth averaged KE, which is a useful measure. Another more useful parameter for diagnosing the potential environmental impact might be the bottom stress, with particular attention paid to a threshold value for critical stress for bed erosion.
RESPONSE: I fully agree, and I have the figures for the bottom stress. However the sediment transport is a separate and complex line of study, so that interpretation of the bottom stress figures in the wider context is well beyond the scope of this paper.

Technical corrections
COMMENTS:
Section 1, line 12 – Weinstein, 2008). Rogue space, full stop.

Section 1, line 25 – world (2010) – reference needs inclusion correctly.

Section 4, line 9 – the subscript M is used where H is meant for the Rayleigh Coefficient.

Fig 2 – please mark the tidal energy site.
RESPONSES: All done

Fig 5 -7 –difficult to read axes and other text.
RESPONSE: The original high resolution figures will be uploaded at the production stage. The .pdf files for Ocean Discussion are automatically generated and I have no control over resolution of the figures.