A review of

'On the Shelf Resonances of the English Channel and Irish Sea',

by David J. Webb

Overview

This paper uses a numerical model to investigate tidal resonances in the vicinity of the English channel. In order to work in frequency space and thus directly probe for resonances, the numerical model is restricted to linear dynamics. This, along with some resolution issues, comprises the accuracy of the model, but it is probably sufficient to investigate the underlying resonant structure. This is done using response functions (in the complex frequency plane), revealing two strong near semi-diurnal resonances (both involving the Bristol channel) and near diurnal resonances (involving topographic Rossby waves).

The paper is well written and nicely illustrated, although I think some technical points could be made clearer. I am not an expert on response functions, but it seems to be interesting to work in the complex frequency plane, and the results thus generated are of some interest. There are some concerns about the convergence of the numerical model, but I don't think these are insurmountable (given suitable caveats).

Detailed comments

1. A general problem is that the tidal model is not very accurate. Presumably, this is in part due to neglected nonlinear terms. However, from the comments at the start of section 3, it seems that the solutions have not converged numerically (i.e., there are still significant changes as the grid size is reduced; lines 199–201).

   However, one might argue that, even though the amplitudes are awry, the model can still be used to make a first stab at understanding the resonance structure. Presumably, the general methods outlined here will be combined with a higher resolution model giving more reliable results.

2. Lines 25–26: this initial description of the skill of the numerical model is somewhat inconsistent with the moderate accuracy described in Section 3.

3. Line 35: your assertion that the 'size of the matrix becomes impractically large' for global problems is dubious. I believe that the earliest tidal solutions (of Accad and Pekeris) were computed in this way, although one could argue that their resolution (a couple of degrees) was too low. However, Hill et al (JGR, 2011) give global solutions with over 10^6 unknowns (at about 1/3 degree resolution) calculated using sparse matrix inversion.

4. Section 2.1: these finite difference equations are rather standard, so I thought a couple of references might be in order. For the case with grid cells of equal volume, it would also be nice to spell out to which of the standard schemes does (8) reduce.

5. Section 2.1: could you clarify whether you are using Cartesian equations with constant f, or spherical polar coordinates with f a function of latitude – or something else?

6. Section 2.2: why do you use height boundary conditions, rather than velocity boundary conditions?

7. Line 173: I wonder if one of the TPXO global solutions would give more accurate open ocean boundary conditions than those of Cartwright?

8. Line 212: you talk about modelling K1 with an extended domain, and then using boundary conditions from this to force the standard domain. But for the results in section 4, presumably just the standard domain is used? If so, does the compromise the accuracy of your resonance results in the diurnal band?

9. Line 260: even though you give a couple of reference, I think it would be helpful to give a couple of sentences explaining what is shown in the polar plots of Figure 4, and why these might be useful. For example, what is the significance of multiple loops in the polar plots, rather than circles? I also guess that the number of resonances is related to the number of cusps.

10. Figure 7: is all the fine scale structure physical, or is it an artifact of the numerical model? Have you looked at ζ in that frequency band to check that the tide looks physical (rather than having grid-scale features)? Are the fine scale structures related to the nonphysical boundary features shown in Figure 9?

11. Line 328: when you say that the solution is iterated, do you mean that once a value for ω_j has been found, the whole procedure is repeated with the nearest neighbours of that ω_j?

12. Section 5.1 (and Table 3): I lost track of which data was being used. Is the analysis for a single location? Or are you somehow fitting over several locations at once to estimate ω_j more widely? Or do you obtain different ω_j for the different locations? If the latter, then it would be nice to know how the ω_j differ between the sites.

13. Equation (12): it seems strange to make such a confusing change of notation from equation (11).

14. Table 3: presumably the imaginary parts of the resonant frequencies shown in Table 3 are highly dependent upon the friction coefficient κ?

15. Figure 8: at first, I assumed that direct output from the model at a fixed forcing frequency was being shown in this figure. But in lines 351–354 you talk about some kind of fitting procedure for R_j, ω_j, A and B, and Figure 8 seems to be the outcome of this. So what is shown?

16. p.18: the aphysical behaviour shown in the lower panels of Figure 9 and noted in lines 381–384 is rather worrying. What confidence should be placed in the corresponding resonant frequencies? Is the accumulation of the B modes somehow related to these boundary effects? One possible way to suppress these effects would be to add a sponge layer along the edge of the domain.

Typographical errors, etc.

1. Equation 6: missing subscript in $\zeta_{\zeta:i,j}$.
2. Line 242: remain $\rightarrow$ remains.
3. Line 285: plain $\rightarrow$ plane.
4. Line 360: needs to be modified.
5. Line 380: ‘of of’.
6. Line 386: spurious ‘is’.
7. Line 404: $w_j \rightarrow \omega_j$.
8. Line 424: ‘leave leave’.
9. Line 502: ‘the the’.