Ocean Sci. Discuss., 10, C322–C329, 2013
www.ocean-sci-discuss.net/10/C322/2013/

Ocean Science
Discussions
Open Access

Interactive comment on "On the shelf resonances of the English Channel and Irish Sea" *by* D. J. Webb

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Received and published: 27 June 2013

I would like to thank both reviewers for their thoughtful comments and detailed corrections.

Reviewer 1.

I have already responded to some of reviewer one's comments. In addition:

1.1. Quality of the Model.

I agree with the comments on the quality of the model but I addressed this issue both at the end of section 3 and in the discussion. There is now a good case for repeating the work at higher resolution, in a larger domain and also in the form of the linearised

version of a fully non-linear model. But that is the next stage.

2.2. Open Boundary Condition

The model resonances do depend on the form of the open boundary condition but, as with other linear systems, they do not depend on the method of forcing.

My previous work on continental shelf resonances (Webb, 1976) showed that they were primarily 1/4 wavelength resonances between the coast and the shelf edge - the amplitude of the tidal wave being largest at the coast and near zero at the shelf edge

I also knew that if I forced a model by specifying sea level on the boundary, this would be like using Dirichlet boundary conditions. Mathematically these always produce modes which are zero on the boundary. As the tidal wave speed is fastest in the deep ocean I hoped that the resonant modes generated with the present boundary conditions would be close to those of a model which correctly allowed loss of energy into the deep ocean.

A test using a simple radiational condition has been written up as a technical report (http://eprints.soton.ac.uk/id/eprint/349410). All the modes there have a radiative component but otherwise they are similar to those of the present paper.

1.3. Satellite Derived Data

The M2 and K1 tide calculations were repeated using the Oregon State tidal fields for the open boundary condition. These produced only minor changes in the results. In the revised paper I am submitting I have kept the original comparisons as they highlight the problems connected with the Diurnal Tide to the west of Ireland. (A good topic for an MSc?).

1.4. Energy Conservation

Originally I assumed, like the reviewers, that the problem with the Coriolis term was well known. However although conservation of energy on an Arakawa-C grid is trivial when the Coriolis term is zero, I could find no papers which tackled the non-zero case without extra averaging (see Arakawa and Lamb below). Since publication of the reviews, one reviewer has also contacted me to say that there appeared to be at last one operational model in which the treatment of the Coriolis term did not conserve energy .

As a result of these comments, I made a further literature search and found a paper by Espelid, Berntsen and Barthe (Int. J. Numer. Engng, 2000). This proposes a similar but slightly different scheme to the one published here. I also investigated the NEMO model.

The NEMO models has a number of options but the standard model, which conserves both energy and enstrophy, uses a finite difference scheme which is based on Arakawa and Lamb (1981). The scheme conserves energy, but only if the kinetic energy is defined in terms of velocities at the corner points of fig. 1 instead of the standard Arakawa C-grid velocity points.

Thus although it is not critical to the paper, I do not think this section can be dropped. I have therefore converted the section to an Appendix and now refer to the Espelid et al. and Arakawa and Lamb papers.

1.5. Workington

Figure 2 shows that, in the model, the tidal amplitude at Workington is approximately 31% of that at Portishead. This is in good agreement with the ratio shown in table 2 (33%). There is a disagreement with the observed ratio (table 2, 63%) but this is primarily due to the large tides at Portishead found in the model.

I checked the response function at Workington. This shows that over the semi-diurnal tidal band, resonance D dominates but that there are a small roughly equal contributions from resonances E, F and the background term.

1.6. Resonance E

Underlying my interpretation is the idea that there was a barrier between Land's End

and the centre of the open boundary then there would be a Bristol Channel 1/4 wavelength mode and a similar Gulf of St Malo mode. Removing the barrier allows them to be coupled giving one in-phase and one out-of-phase mode.

I agree that at some angular velocity one might expect to see a 1/2 wavelength cross-channel mode. However as the D and E modes have similar angular velocities this implies that the Bristol Channel - Shelf Edge and St Malo Shelf Edge 'distances' are half the St Malo - Bristol Channel 'distance' (More accurately it is not the distance but 1/sqrt(g*depth) integrated along each path). Possibly the reviewer is correct but I suspect that the cross-channel simple mode is mixed into one of the higher modes of the full system. (Another topic for an MSc ?)

Detailed Comments

2.1 "... at a particular real angular velocity ..."

This is what I meant. By limiting the model area I also limit the density of resonances, i.e. the number of resonances per unit change in real angular velocity. In a full ocean model I would expect many times the density of this limited area model.

As a thought experiment, consider what would happen if outside the area modelled the rest of the ocean was deep. If we start with a barrier between the two regions then initially the deep ocean will have a large number of weakly damped resonances. On the basis of Platzmann's results one might expect a deep ocean resonance every 0.3 rad/day in the region of the semi-diurnal tides.

If we slowly remove the barrier - the resonances will become coupled - and the expectation is that they would become coupled primarily to resonances with similar angular velocities on the other side of the barrier.

So in the global ocean there should be resonances around 12 rad/day which, in the model region, have spatial structures similar to the of resonances D and E. In other parts of the world ocean they should have very different behaviour. Because they are close in angular velocity to the semi-diurnal tidal band they will be more strongly excited by the tidal forcing than distant modes and together will generate the high M2 tides in the Gulf of St Malo and the Bristol Channel.

2.2 Section 2.1

See comments 1.4 above.

2.3 Definition of 'Loop'

I agree that these is a problem with the use of the word loop but I do not know how to make the definition more quantitative at this stage.

An individual isolated resonance is known to produce a single circle in which the locus defining the circle has positive curvature, i.e. it moves in an anticlockwise direction. If there is no friction, the circle will be centred on the origin.

Similar behaviour may be expected from a group of resonances which are closer to each other than to the real axis and closer to the real axis than to other resonances. At the other limit, if one forces one end of a long channel, the response seen at the other end consists of a series of circles (or a spiral if friction is important) centred on the origin - the combined effect of many overlapping resonances.

On this basis, I consider a 'loop' of interest to be an extended region of positive curvature which is distinguishable from the background. This may be due to one or more resonances. Such regions often end in a kink of negative curvature which I take to be the point at which another resonance or set of resonances is becoming dominant.

2.4 Page 409, line 21.

Each eigenfunction really is zero on the boundary. This is covered in many books on mathematical physics. Mathematically the fixed value of the boundary is replaced by a delta function forcing term just inside the boundary such that the integral over the delta function equals the boundary value.

2.5 Figures 2, ... , 10

The OSD figures can look poor especially when printed on a low resolution printer. I have used similar text sizes and line widths for previous papers and they reproduced well as single column figures when printed in OS or on a good quality laser printer. I'll make sure the same is true for the final figures here.

Reviewer 2 ==========

General Comments —————-

3. 1. One can always go to higher resolution but computational costs increase rapidly. I decided that the grid size used gave a sufficiently realistic result for what is really a preliminary survey. As in my response 1.1 above, there is a case for more detailed studies in future and I am sure that some of these will use much higher resolutions.

3. 2. Page 395, line 7.

The comments on skill have been corrected.

3.3. Page 395, line 17.

The reviewer is correct. My comments on the matrix size have been changed.

3.4. Section 2.1, the Coriolis Term

For the finite difference schemes I have already given the Arakawa reference. He considered constant depth, a constant Coriolis term and a constant grid spacing. Under these conditions, eqns 6 to 8 become the standard Arakawa-C grid equations.

3.5. The model uses spherical coordinates with the Coriolis term varying with latitude. This is explained in the revised text.

3.6. Section 2.2 Use of Height Boundary Condition

The honest answer is that I used tidal height as boundary conditions because I am used to using them and I have found no problems in the past. If I wanted to justify its use I might say (a) why not? or more seriously (b) observations are usually of tidal height whereas the velocity is usually a derived quantity and open to additional errors.

3.7. Use of Satellite Derived Tidal Data

This is included in my response to Reviewer 1.

3.8. Use of Extended and Standard Domains

A good point. As discussed in the paper, if the Coriolis term is turned off, there is an Irish Sea mode close to the diurnal tidal band. When the Coriolis term is turned on this becomes strongly coupled to the continental shelf waves. Then because these are represented poorly, the shape of the coupled mode near the shelf edge is probably wrong. In the run I did with a radiational boundary condition (see NOC Report) the effect of the continental shelf waves was smaller and the shape of the mode changed near the continental slope. Elsewhere there was little change.

3.9. Discussion of Loops

This is included in my response to Reviewer 1.

3.10. Is the Fine Scale Structure Physical

Yes the fine structure is physical. It corresponds to Rossby waves and Shelf waves. The K1 solution is typical to the solutions obtained when forcing at real values of angular velocity in this region. None of the solutions showed any evidence of two grid point features. When the boundary conditions are changed to include radiation (see NOC report) the fine scale structure is still here but there is less shelf wave noise.

3.11. Method of Iteration

Yes, four new values are calculated around he new estimate and the process repeated.

3.12. Calculating the Resonance Position

The resonance angular velocity was calculated making use of values at the point in

the model where the first of the four solutions had the largest magnitude - i.e. the point at which the resonances appeared to have the greatest effect. Using other large amplitude points produced differences at the level of rounding errors.

3.13. Notation

In the revised text the notation is corrected.

3.14. Effect of Friction

The 1-D study (Webb, 2012) showed that in the limit of low values of friction, the imaginary part of the eigenvalue is proportional to the friction term.

3.15. Figure 8

For the spatial figures the resonance residue was calculated at each point of the model. Together the residues give the spatial structure of the model.

3.16. Aphysical Behaviour

These shelf modes must be being affected by reflection at the open boundaries. For this reason the details of their structure and angular velocities must be wrong. However I would expect the shelf waves of the real ocean to produce similar bands of resonances in the response of the full ocean.

The bands will will consist of separate modes, because the ocean is finite - but there may be a limit point corresponding to the point where the wavelength tends to zero.

There will also be more than one band, corresponding the the different cross shelf modal structures. They may be coupled to Rossby waves and other modes of the deep ocean if they have similar angular velocities, damping times, spatial structure, vertical structure ... - but that is a problem for another day.

Interactive comment on Ocean Sci. Discuss., 10, 393, 2013.