Ocean Sci. Discuss., 9, C218–C224, 2012
www.ocean-sci-discuss.net/9/C218/2012/

Ocean Science Discussions

Interactive comment on "On the Shelf Resonances of the Gulf of Carpentaria and the Arafura Sea" *by* D. J. Webb

D.J. Webb

djw@soton.ac.uk

Received and published: 24 April 2012

First I would like to thank all three referees for their detailed reading of the paper and for their comments. As a reviewer and ex-editor myself I know it can be a time consuming and thankless task.

Ocean Science requires me to respond in detail to each of the reviews - which I do so below.

Reviewer 1

1.1 "I struggle to see the purpose of the paper and in scientific terms I find it relatively weak. Do we learn a lot more about tidal resonance?"

In response I'll concentrate on just the 3-D figures - the ones which prompted me to write the paper. Where have you seen something like that, the actual distribution of poles as a function of real and imaginary angular velocity, the pattern of Gravity wave and Rossby/topographic wave modes and the way they interact? Previous studies of resonances have only worked on the real axis and they often ignore the effects of friction or consider only single isolated resonance. I hope that the results show, if nothing else, that the oceanic spectrum is much richer than that.

1.2 "... the paper is poorly referenced ..."

The current version is honest in that it 'references' the key papers which influenced the research presented in one way or another. I will add some of the references suggested by this referee and by referee 3, but as many of them are not open access I warn potential readers of this paper that they are not essential for understanding the background of the present work.

1.3 "... but the shelf seas still dampen tides, don't they?"

I'll try and clarify the paragraph.

1.4 "How does the model perform against newer datasets?"

This would require lengthening the paper and I do not see much being gained. There have been other studies of the Gulf region since the original paper which confirm the pattern of diurnal and semi-diurnal tides shown. I'll see if I can find a good reference.

1.5 M2 and K1 periods in figures 4-7.

I'll add suitable markers.

1.6 "...we assume a constant c. What is the error in doing this?"

WKB theory is often used to investigate systems where the depth varies by only a small fraction over distances corresponding to an inverse wavenumber (= wavelength/(2*pi)). Under these conditions the local increase in phase with distance is given to a good approximation locally by assuming the wave speed is equal to sqrt(g*depth). If the depth changes significantly within a wavelength then reflection becomes important and resonances become possible.

1.7 "The first part of section 6 is an enigma to me."

I am sorry to have lost you at this point - as it is the key section of the paper and I believe the most original.

Let me just say that a long time ago I published a figure showing resonances from another area of physics which I thought hinted at the sort of detail that might be observed in the ocean - and that the figures here are of the sort that I would have liked to have used then.

(Webb 1974, Green's Function and Tidal Prediction. Reviews of Geophysics and Space Physics, 12 (1), p103-116.)

1.8 "The discussion is more a summary ..."

How about "Conclusions"?

1.9 "27 figures in a paper of this length is a bit over the top ... "

I think twenty seven is necessary. The text carries a number of stories - the problems in interpreting data only available along the real axis, the actual distribution of resonances of a realistic region of the sea and finally the application of the methods to the Gulf region. The text would become really overloaded if the figures were not available.

OK, the number could be formally reduced by combining them, but that would not change the area of the document covered by figures. One could also make the case that the Rossby/topographic waves are not discussed in enough detail, but that would need more figures ... .

—-

Reviewer 2.

2.1 "Port Langdon K1 phase ... looks unlikely ..."

I agree - this maybe because the island is not properly represented in the model (the depth was set to the minimum value). The model also shows a similar phase error near Turtle Island. However overall the observations support the overall pattern of tides shown by the model and the model result that there is an amphridrome centred near the western side of the Gulf with the phase on the nearby coast in the region of 270 to 300 degrees.

2.2 "Torres Strait ... "

Most of the strait is blocked by reefs and shallow channels (see for example Google Earth). There are tidal stations at each end of the main shipping channel in the south. These show large differences in the tidal constants and the age of the tide.

The observations imply that the amount of tidal energy passing though the strait is small. As a result, the use of a solid boundary is reasonable as a first approximation.

2.3 "... figure 8"

Thanks, I'll change the list to include 8 and 9.

2.4 "... the origin and (30, 10i)"

I was trying to find a short way to describe the region. If I cannot think of a better way I'll use the formula you give.

2.5 "Figure 4"

Thanks, I'll correct the text.

2.6 " ... poles .. positive imaginary direction ..."

As you say, in reality there can be no poles in the positive imaginary direction - as they would represent modes growing with time. As discussed in Webb 1974 (cited above) they would also break causality.

Although the region plotted is all in the negative imaginary direction, some tests at a coarser resolution were carried out with positive imaginary components (and also negative real components) as part of the checks for programming errors.

2.7 ".. Kelvin gravity wave ..."

Thanks. I'll correct the text.

2.8 "The reason for this is unclear"

I guess I was covering myself. In another study, yet to be published, I started by using a Coriolis term which was a factor of two too small. When I corrected this the angular velocity of the Rossby waves changed by a large amount but there was less effect on the gravity wave type resonances. So why is the discrepancy so large in the case here? (the resonances at 14.5 and 19.1 radians per day are well into the gravity wave region).

I'll think about it and see if I can improve on the statement.

2.9 Size of the residual.

I worked on the basis that contributing resonances tend to produce loops and kinks, as in the small scale structures seen in figures 8 and 9. For each of the figures published I will have tried adding the other nearby resonances but then left them out if they did not significantly smooth or reduce the residual. There may be another paper for somebody in comparing different objective ways of doing this.

2.10 D and K

Again many thanks, I'll correct the text.

2.11 "last sentence"

I need to rethink this sentence - maybe I need to use phrase other than 'strong coupling'.

The background is Webb 1976, where I show that strong dissipation only occurs when the resonances are matched to the deep ocean - and by matched I mean that they have both the right real component of angular velocity and the "right" amount of damping. If the damping is too much or too little then the tidal energy is reflected from the shelf edge back into the deep ocean.

2.12 " .. B and its c.c"

Yes. I'll change the caption

—-

Reviewer 3.

3.1 "the model is linear"

The basic justification of using a linear model is that almost everywhere in the world the non-linear terms in the barotropic tide (due to friction, inertia, internal tides) are observed to be small. Secondly, once we start talking about resonances, the basic concept is really only valid in a physical system that can be treated as approximately linear.

3.2 "the boundary to the open ocean is held constant"

You will agree that the resonance frequencies of a linear system do not depend on the method of forcing. Also, as stated in caption 13, the eigenfunctions have zero amplitude on the open boundary.

One reason for this is that if the resonance 'j' has angular velocity 'w_j', then the response function very near to the resonance pole must be of the form "R_j/(w-w_j) + something that is approximately constant". At the boundary where the solution is fixed, the 'approximate constant' must equal the value on the boundary and 'R_j' must be zero.

A consequence is that, if the same normalisation method is used and the class of boundary condition stays the same (in this case Dirichlet), the eigenfunctions are independent of the boundary condition. For Dirichlet boundary conditions they may have a slope at the boundary but must have zero amplitude.

I don't want to get into the way that functions that are zero on the boundary contribute to non-zero values there but it is a standard problem and the solution is a bit like the Gibbs phenomenon where a series of sine waves can generate a full square wave.

I could have used different forcing functions and these would have changed the response function figures (figs 4-12). However by using the simplest possible boundary condition - interpretation of the results was simplified. Also after allowing for the mean phase and amplitude along the boundary (i.e a single complex constant), the forcing used is not that different from the M2 and K1 open boundary conditions.

3.3 "Where do we see an improvement in our understanding of coastal tidal resonant systems?"

Many of the ideas of this paper, including the complexity of the resonant system, were implicit in Webb 1976 (see above). What the present paper does is to confirm or underline the ideas and provide some concrete results.

See also response 1.1 above.

3.4 References

Thank you for the extra references. See response 1.2 above.

3.5 Validation

Further validation can always be performed but it looks like a lot of work for what can only ever be a marginal change in the paper. See response 1.4 above.

Interactive comment on Ocean Sci. Discuss., 9, 443, 2012.