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Ocean Science Discussions

Interactive comment on "An analytical study on M_2 tidal wave in the Taiwan Strait with the extended Taylor's method" *by* Di Wu et al.

Anonymous Referee #1

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Dear Editor,

The authors present an interesting study into the tidal dynamics of the Taiwan Strait. Particularly, they apply a so-termed 'extended' version of the classical 'Taylor method' to reproduce and explain the amphidromic pattern of the semi-diurnal tide in that region. The word 'extended' here refers to the treatment of the (open) boundaries and the inclusion of bottom friction. This leads to an analysis of two Kelvin waves, propagating southward and northward, the superposition of which largely determines the amphidromic pattern in the Taiwan Strait. As to the sources that may contribute to the northward Kelvin wave, the authors conduct a further analysis in which the model is extended in various ways. To be honest, I find this part a bit far-fetched, simply because

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the rather 'crude' (as the authors acknowledge themselves) geometrical choices made here clearly ignore the true geometry of the sea surrounding the Taiwan Strait, particularly regarding coastlines. This makes the conclusions of this part less convincing to me, which is actually my first concern of this study. The same applies in my opinion to statements about the "superiority" of this approach in the conclusions. Other aspects that – in my opinion – require clarification or improvement deal with (1) description of the study site, (2) literature review, (3) model formulation, (4) comparison with observations, (5) interpretation of Kelvin and Poincaré modes, and (6) phrasing. These points are detailed below. Overall, I think the topic of the paper is appropriate for OSD. The novelty of the work is apparent, but the my concerns on how this has been done are substantial. Therefore, my overall recommendation is major revision.

Anonymous, 28 August 2017.

1) Description of the study site may be extended by presenting the relative importance of other tidal constituents (S2, K1, O1), e.g. expressed in the value of the form factor F. Why did you consider the M2-tide only? And what is known about the (magnitude of the) tidal currents? This helps interpretation compared to other tidal basins around the world.

2) Literature review should in my opinion be improved in certain respects.

- The large number of references on tides in the Taiwan Strait makes me wonder what has been found in those studies. . .
- Page 2, Line 12: "was the main component" –> "is the main component".
- Upon first introduction in Line 18, The extended Taylor method (when using "the", please remove the "'s") requires a reference and an explanation of what 'extended' means here.
- Roos & Velema should in fact be Roos et al (there are more co-authors). Also,

unlike suggested by the authors here, the presence of the Dover strait in the south is in fact an open boundary.

- I cannot find Table 1 in the .pdf-file that for this review.
- Hendershott & Speranza (Deep Sea Res 1971) is worthwhile mentioning as they followed a similar approach to study the Gulf of California (two Kelvin waves)
- Because of the depth-step, one may consider reference to Roos & Schuttelaars (Ocean Dyn 2011)
- Figure 2: “amphidromic chart” seems better, because it is both co-tidal and co-range information that is plotted here. Also: is it Chen and Andersen or Cheng and Andersen?

3) Model formulation contains some inaccuracies. First of all, the title of section 2 does not really cover the content. I think “Model formulation and solution method” is more appropriate.

- Please mention the important simplifications/approximations made here. This is a linear depth-averaged model, the validity of which is relevant. I think this should be discussed at some point.
- The pressure gradients in Eq.(1) should have spatial derivatives ($\partial/\partial x$ and $/\partial y$)
- Page 3, Line 8: “channel” –> “rectangular channel”
- Line 11: “by introducing a collocation method” –> “by applying a collocation method”
- Page 4, Line 5: “for open rectangular basins” –> “accounting for the finite length of the basin”

- Line 13: please mention “depth-averaged”
- Line 15: this approximation is known as the f-plane
- Line 16: “cosine wave” is perhaps better rephrased as “monochromatic”
- Line 17: please put brackets { and } after the real part: $\mathrm{Re}\{(\zeta, u, v)\exp(i\sigma t)\}$ and please introduce (ζ, u, v) as the complex amplitudes of the quantities introduced previously.
- Page 5, Line 4: please add “each with a different uniform depth h_A and h_B”
- Line 9: would be nice if your radiation condition would include bottom friction. How large us mu typically?
- Line 23: the formula for wave speed also holds in absence of friction only... please reorder
- I would put the details of Eqs.(9)-(12) and Eqs. (17-24) in an appendix.
- Line 22: I think it is unnecessary to introduce Q, because you can immediately write $Q^2 = \beta^2 - -\alpha^2$.

4) Comparison with observations is purely visual, which raises some questions. First of all, how did you choose the basin dimensions, orientation? How do you actually project the true geometry, with curved coastlines, onto the rectangular model domain? And, as before: did you consider doing the same for other tidal constituents? Other than that, I find the title of Section 3 confusing, since the model has already been introduced in Section 2. I suggest to change the title into “Application to Taiwan Strait”, because that is what is actually done in this section. Also, please avoid if statements when you specify coefficients (Page 7, line 2) an please replace “equal to” with an equality sign $=$.

5) Interpretation of role of Kelvin and Poincare modes can readily be deepened by further analysis. First of all, what is the wavelength of the Kelvin waves? (I see it is mentioned later on but already here it is relevant). And are the Poincare modes free or bound (from the depth and width values I guess they are all bound), and what is the typical length scale of decay of the lowest Poincare mode? This gives insight in the extent to which these modes affect the amphidromic pattern in the (interior of the) Taiwan Strait.

Also, I do not understand the statement that frictional force would be a major factor (as mentioned here and repeated in Section 5). I think this is not the case, in view of the mild amplitudes and large depths. Can you support this statement? I suspect you would still get a good fit if bottom friction were switched off.

- Page 7, Line 23: “inclusion of the Poincaré modes improves”
- Page 8, Line 10: Also possible is that the assumption of uniform depth is too restrictive in this Taylor approach. . .
- Page 8, Line 21: this is a basic statement about progressive waves and therefore not really insightful in my opinion.
- Page 10, I do not understand the statement on resonance. This may hold for closed basins, but here we have a topographic step. . .

6) Phrasing in general should be more precise in my opinion. For example, avoid the unnecessary and confusing use of the verb “can”. My suggestion is to consult a native speaker of the English language with knowledge of the topic to revise the text. Here I explain what I mean by giving some suggestions to improve the abstract (line 8-22).

- Page 1, Line 8: “M2” –> “semidiurnal lunar (M2)”

- Line 8, “The extended Taylor’s method”, remove “‘s”: and is it sufficiently clear what this means?
- Line 10, “but” –> “and” (because this does not really signify a contradiction!)
- Line 10: “friction forces” –> “bottom friction”
- Line 16: “can further improve” is unclear. Better: “Inclusion of Poincaré modes further improves”
- Line 18: “can be reflected” –> “is reflected” (I guess this is what you mean)
- Line 21: same with “can” as in Line 18.

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