Response to Referee #1

Anonymous Referee #1

Received and published: 28 August 2017

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 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.

Reply: We sincerely thank the Referee for his careful reading of our manuscript and constructive comments and suggestions, which are of great help in improving our study. We have addressed all these comments; our responses are given below.

In this response, the Referee's comments are copied in black, our replies are shown in red, and the following abbreviations are used:

OM - original manuscript,

R1 – Revision #1 - an updated manuscript, which will be submitted as a supplement to this response.

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.

Reply: In the "Introduction" of R1, we have added the following sentence to describe the magnitudes of the constituents S_2, K_1 and O_1 relative to M_2: "Compared to M_2, which has a maximum amplitude of over 2.2 m, the amplitudes of the rest of the constituents are much smaller. The maximum amplitudes of S_2, K_1 and O_1 observed at 11 coastal gauge stations reported by Jan et al. (2004b) are 0.66, 0.39 and 0.27 m, respectively".

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. . .

Reply: Since this study focuses on the tidal dynamics in the strait, we describe mainly the progress in the dynamic aspects without giving a comprehensive review of the progress in the studies of tides in the strait.

- Page 2, Line 12: “was the main component” –> “is the main component”.

Reply: Revised as suggested.

- 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.

Reply: "the extended Taylor’s method " in the OM has been replaced with "an extended Taylor method" in R1. (Here, we replace “the” with “an” according to the comment from Referee #2).

- 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.

Reply: “Roos and Velema” has been changed to “Roos et al.”, and the citation in the References is also revised in R1. The statement “all of the studied basins” in the OM is not accurate, and thus “all” is replaced with “most of” in R1.

- I cannot find Table 1 in the .pdf-file that for this review.

Reply: “Roos and Velema, 2011, Table 1” in the OM has been replaced with “Table 1 of Roos et al., 2011” in R1.

- Hendershott & Speranza (Deep Sea Res 1971) is worthwhile mentioning as they followed a similar approach to study the Gulf of California (two Kelvin waves)

Reply: Hendershott & Speranza’s paper has been cited in R1.

- Because of the depth-step, one may consider reference to Roos&Schuttelaars(Ocean Dyn 2011)

Reply: Roos & Schuttelaas (2011) has been cited in R1.

- Figure 2: “amphidromic chart” seems better, because it is both co-tidal and corange information that is plotted here. Also: is it Chen and Andersen or Cheng and Andersen?

Reply: We replaced “cotidal” with the more accurate term “tidal system” in R1. “Chen” has been replaced with “Cheng”. The reason for not using “amphidromic” is that there is no amphidromic point in the TS, especially in the area shown in Fig. 3.

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

Reply: The section title has been revised as suggested.

- 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.

Reply: According to this comment, we have added the following statements to the text: “The equations in (1) are two-dimensional linearized shallow water equations on an f-plane with the momentum advection neglected. The equations are the same as those used in the work of Taylor (1922), except that the bottom friction is incorporated, as in Fang and Wang (1966) and Rienecker and Teubner (1980).”

- The pressure gradients in Eq. (1) should have spatial derivatives (∂/∂x and /∂y)

Reply: Corrected as suggested.

- Page 3, Line 8: “channel” –> “rectangular channel”

Reply: The word “rectangular” has been added.

- Line 11: “by introducing a collocation method” –> “by applying a collocation method”

Reply: The word “introducing” has been replaced with “applying”.

- Page 4, Line 5: “for open rectangular basins” –> “accounting for the finite length of the basin”

Reply: Revised as suggested.

- Line 13: please mention “depth-averaged”

Reply: Added as suggested.

- Line 15: this approximation is known as the f-plane

Reply: The word “f-plane” has been added into the next line.

- Line 16: “cosine wave” is perhaps better rephrased as “monochromatic”

Reply: The word “cosine” has been replaced with “monochromatic”.

- Line 17: please put brackets { and } after the real part: Re {(ζ, u, v) exp(iσt)} and please introduce (ζ, u, v) as the complex amplitudes of the quantities introduced previously.

Reply: The brackets have been added.

- Page 5, Line 4: please add “each with a different uniform depth h_A and h_B”

Reply: Added as suggested.

- Line 9: would be nice if your radiation condition would include bottom friction. How large us mu typically?

Reply: In the present study we take $\mu = 0.15$ so that μ^2 is of the order of 0.02. If friction is considered, the expression of the radiation condition will become complicated: besides a change in the amplitude ratio, there will be a phase difference between the velocity and elevation. These changes would cause very few differences in the computed results (of the order of μ^2) and are thus ignored in this study.

- Line 23: the formula for wave speed also holds in absence of friction only. . .please reorder

Reply: The sentence has been reordered according to this comment. Here, the formulas hold only under the condition of no friction; if friction is considered, the formulas for wave speed and wave number should be modified {e.g., the wave speed is equal to $\mathrm{Re}\left(\frac{\sigma}{\beta}\right) = \left[\frac{2gh}{\sqrt{1+\mu^2}+1}\right]^{1/2}$, which is equal to $0.9972\sqrt{gh}$ for $\mu = 0.15$}.

I would put the details of Eqs.(9)-(12) and Eqs. (17-24) in an appendix.

Reply: The derivation of these equations has already been made in previous works (e.g., Fang et al., 1991), so it seems unnecessary to give further details in this paper. Furthermore, these equations will be mentioned in the text that follows. Therefore, for convenience, we wish to retain these equations as they are.

• Line 22: I think it is unnecessary to introduce Q, because you can immediately write $Q^2 = \alpha^2 - \beta^2$.

Reply: Yes, Q^2 is equal to $\alpha^2 - \beta^2$. In R1, $-Q^2$ has been replaced with $-\alpha^2 + \beta^2$ in the expression of s_n (Eq. (16)).

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 =.

Reply: The main purpose of this study is to reveal the dynamics of the M_2 wave formation in the strait. We think visual comparison is capable of meeting this goal. No attempt is made to best fit the model results to observations in this study. For the same reason, the basin dimension and orientation, locations of the sidewall and open boundaries have all been chosen through visual inspection. The diurnal constituents in the strait are small and have a simple structure (please see the following tidal system chart of the largest diurnal constituent K_1). Thus, we are not interested in the study of their dynamics. The dynamics of S_2 is the same as that of M_2, so we just pay attention to M_2. The title of Section 2 has been changed according to this comment in R1. "If" has been replaced with "In this study"; "equal to" has been replaced with the sign "=".

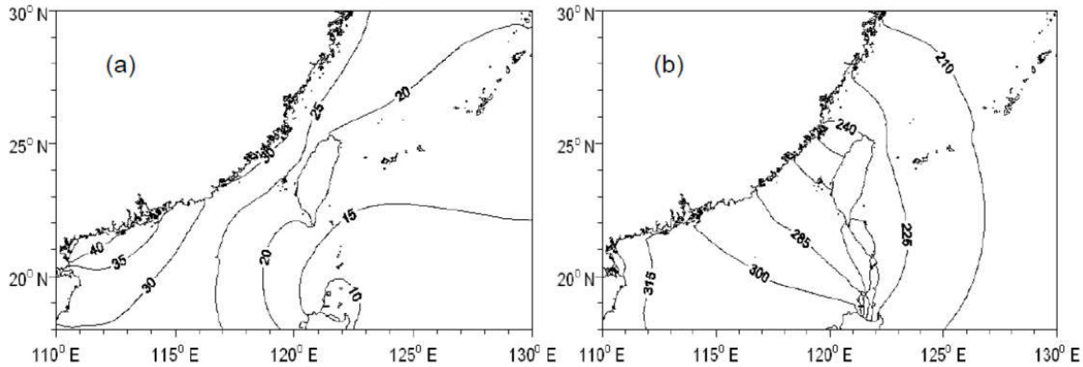

Fig. 1.1. K1 tidal system in the Taiwan Strait and its neighbouring area, (a) amplitudes in cm (b) phase-lags in degrees (from Zhu et al., 2009).
(In the figure number, the first "1" represents "Author's Response to Referee #1")

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.

Reply: In the first paragraph of Section 3.1, we have added the following statement: "From these parameter values, we can obtain the wavelength of the M2 Kelvin wave as 1009 km. Since the basin width is smaller than half of the Kelvin wavelength, the Poincaré modes can only exist in a bound form (Godin, 1965; Fang and Wang, 1966). The e-folding length of decay of the lowest Poincaré mode is approximately 63 km, that is, the amplitude of this mode reduces to approximately 37% relative to its maximum value at a distance of 63 km away from the boundary. Equivalently, it may also reduce to approximately 20% relative to its maximum value at a distance of 100 km. The length scales of decay for higher order Poincaré modes are even shorter."

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.

Reply: Here, the words "the Coriolis and frictional forces" have been replaced with "the Coriolis force and the weaker northward wave" according to this comment.

- Page 7, Line 23: "inclusion of the Poincaré modes improves"

Reply: Revised as suggested.

- Page 8, Line 10: Also possible is that the assumption of uniform depth is too restrictive in this Taylor approach. . .

Reply: We agree with this point of view and have therefore added "at a uniform depth" following "Kelvin wave", such that the statement is now "This amplitude variation cannot be completely represented by the superposed Kelvin wave at a uniform depth".

- Page 8, Line 21: this is a basic statement about progressive waves and therefore not really insightful in my opinion.

Reply: The words "due to propagation direction" have been deleted in R1.

- Page 10, I do not understand the statement on resonance. This may hold for closed basins, but here we have a topographic step. . .

Reply: Resonance is also possible in a basin with a topographic step. This can be illustrated with one-dimensional problems as follows:

For a basin of uniform depth h and length L, if it has a closed end at x=0 and an opening at x=L, where tidal elevation is given as $\zeta(L,t) = a\cos\sigma t$, then the elevation in the basin is $\zeta(x,t) = a\frac{\cos kx}{\cos kL}\cos\sigma t$, where $k = \sigma/\sqrt{gh}$. Resonance will occur if $kL = \frac{\pi}{2}, \frac{3\pi}{2}, ...$ Please see Godin, 1993 (continental Shelf Res., 13(1), p. 103).

For a shallow basin of uniform depth h and length L, if its mouth is at x=L, where tidal elevation is given as $\zeta(L,t) = a\cos\sigma t$, and it has a topography step at x=0, which connects with a deep basin of infinite-depth, then the elevation in the shallow basin is $\zeta(x,t) = a\frac{\sin kx}{\sin kL}\cos\sigma t$, where $k = \sigma/\sqrt{gh}$. Resonance will occur if $kL = \pi, 2\pi, \ldots$. Please see Jan et al, 2002 (Journal of Oceanography, 58, p. 849).

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).

Reply: R1 has been edited for English by a native English speaker from a language service company. Please see the following certificate issued by them

AMERICAN JOURNAL EXPERTS

EDITORIAL CERTIFICATE

This document certifies that the manuscript listed below was edited for proper English language, grammar, punctuation, spelling, and overall style by one or more of the highly qualified native English speaking editors at American Journal Experts.

Manuscript title:
An analytical study on M2 tidal wave in the Taiwan Strait with an extended Taylor method

Authors:
Di Wu, Guohong Fang, Xinmei Cui, Fei Teng

Date Issued:
October 23, 2017

Certificate Verification Key:
A75B-6EB6-9F56-B486-FF3A

This certificate may be verified at www.aje.com/certificate. This document certifies that the manuscript listed above was edited for proper English language, grammar, punctuation, spelling, and overall style by one or more of the highly qualified native English speaking editors at American Journal Experts. Neither the research content nor the authors' intentions were altered in any way during the editing process. Documents receiving this certification should be English-ready for publication; however, the author has the ability to accept or reject our suggestions and changes. To verify the final AJE edited version, please visit our verification page. If you have any questions or concerns about this edited document, please contact American Journal Experts at support@aje.com.

American Journal Experts provides a range of editing, translation and manuscript services for researchers and publishers around the world. Our top-quality PhD editors are all native English speakers from America's top universities. Our editors come from nearly every research field and possess the highest qualifications to edit research manuscripts written by non-native English speakers. For more information about our company, services and partner discounts, please visit www.aje.com.

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

Reply: Revised as suggested.

- Line 8, "The extended Taylor's method", remove "'s": and is it sufficiently clear what this means?

Reply: Revised as suggested.

- Line 10, "but" –> "and" (because this does not really signify a contradiction!)

Reply: Revised as suggested.

- Line 10: "friction forces" –> "bottom friction"

Reply: Revised as suggested.

- Line 16: "can further improve" is unclear. Better: "Inclusion of Poincaré modes further improves"

Reply: Revised as suggested.

- Line 18: "can be reflected" –> "is reflected" (I guess this is what you mean)

Reply: Revised as suggested.

- Line 21: same with "can" as in Line 18.

Reply: Revised as suggested.