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Comment on hess-2021-75

Anonymous Referee #2

Referee comment on "River-enhanced non-linear overtide variations in long estuaries" by Leicheng Guo et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-75-RC3, 2021

This manuscript discusses the effect of river-tide interactions on the generation of overtides, specifically the M4 tide. First, several mechanisms causing the M4 tide in the 1D shallow water equations are computed and analyzed following the method of Gallo and Vinzon (2005). Second, and their main finding, is that the total energy in the generated M4 tide varies with the river discharge and displays a maximum for an, in their range, intermediate discharge. This is further explained conceptually.

I like the idea of the main finding that the energy in the generated M4 tide varies with river discharge and displays a maximum and I think such a thing would be an insightful finding. However, I do not think the conclusions are actually valid and certainly not sufficiently demonstrated. To summarize my main comments (full details are given below): (1) I have good reasons to think that the conclusions are actually only valid for a few cases that look very much like the chosen case study and carry little generality for other estuaries. (2) The conclusions about the spatial characteristics of M4 are not supported by the results, which are integrated over the length of the channel. (3) I think the explanation of the maximum of M4 energy for intermediate discharge as balance between dissipation and generation is incorrect and actually caused by a different mechanism.

Furthermore, the method employed by the authors is shaky: the key equations that much of the results rely on contain multiple quite essential errors and the case study is very specific (also see details below).

This leads me to the recommendation to reject this paper.

Main comments about the conclusions

- In fact you study the transfer of energy from two harmonic components (subtidal and M2) to another (M4). The generated M4 tide in itself is a wave that may propagate according to its own dynamics. This highlights two big problems with the present analysis:

Firstly, your case is friction dominated and for a long estuary without reflection at the head of the estuary. This means that travelling waves will decay. Hence, a small increment in M4 tide generated in some location will not propagate very far. Thus, you dominantly see that locally strong generation of M4 results in a locally strong M4 with some spatial smoothing due to the propagation. I expect that this is totally invalidated in estuaries that are not dominated strongly by friction everywhere or which are shorter and reflect the incoming wave. Hence, your results only represent a small portion of all estuaries. Some of the strongly converging, less frictional branches of the Yangtze estuary itself (which are not considered in this study) could already be a counterexample. Hence, I am of the opinion that a 'general theory' as presented here is not so useful and one could just as well study the handful of actual estuaries satisfying it.

Secondly, you explain the maximum in integrated M4 energy for river discharge as a balance between dissipation of the river flow on the M2 tide vs generation of M4 by tide-river interaction (Fig 8). This is not necessarily true. What you actually find is redistribution from the subtidal and M2 water motion to other frequencies. This happens primarily through the term u|u| in the bottom friction, which you may easily show has a maximum for (approximately) R2T=1. So the actual generation of M4 has a maximum. Dissipation is an additional effect but I would guess it is not essential.

- In section 4.1 and figure 7 you then draw some of the main conclusions on how the local R2T affects the local M4 generation. This is not addressed by your theory, which considers the total integrated M4 energy. Therefore this conclusion is not supported by your results. I expect that this conclusion indeed works in the friction dominated – long estuary setting here but not in general, where the M4 may propagate.
- Ln 491-492 actually address the phase of the M4 (relative to the M2). You don't show any results related to the phase, so this conclusion cannot be drawn.
- ln 554-556: 'In this work we see that the quadratic bottom stress term also leads to significant M4, through river-tide interaction': this is stated as the main novelty, but is not new.
- Section 3.2: I don't see the hypothesis underlying this section. Your case without convergence still features a friction-dominated M2 tide. Since the M2 is similar to the case with convergence, I don't see why the M4 generation should be so different. In any case, just one example of a case without convergence does not prove much. This section does not add anything for me.

Main comments about the method

- Equations (3) and (4) are inconsistent. You assume that only a subtidal and M2 water motion are present, but the numerical computation also allows for all overtides. Implicitly, you assume here that all overtides are much smaller than subtidal and M2, i.e you employ scaling (you do this explicitly on ln 283). This is weird, because in ln 184-195 you argued why models based on scaling analyses are not good enough for your study and you need to use a fully numerical model. If you want to do this, I'd recommend using scaling analysis formally in the analysis. This becomes problematic when the M4 tide is not small compared to M2.
- Eq 10 and therefore 11-13 (i.e. the main decomposition that you rely on in the results) is wrong. This is not what Godin (1999) uses. You need to use a scaling factor U here:

u|u| = U(a*u_scaled + b*u_scaled^3),

such that u_scaled ranges between -1 and 1. On a more detailed level, the coefficient a, b you choose in Eq. 10 are Heron's approximation while Godin (1999) argues that one should better use Chebyshev's approximation.

- Eq 11-13 contains another mistake: 'theta' is forgotten everywhere. Hence the phase information is lost. This is essential.
- I am not entirely convinced of the comparison (fig 5) between the 'discharge gradient' term (Eq 11) to the advection and friction terms (Eq. 12-13). The first appears in the continuity equation and the latter terms in the momentum equation. To create the same unit for all terms, you scale with two different quantities, but why can I compare these? I know Gallo & Vinzon (2005) did the same, but to me this is a very inexact analysis. I think you may at most compare the results on order of magnitude and conclude that all terms are of a similar order of magnitude.

Other remarks

- Ln 184-191: I don't think this gives a proper reflection of the literature. Some of the analytical or semi-analytical literature actually resolves (part of the) overtide and various nonlinear terms, e.g. Friedrichs & Aubrey (1988), Lanzoni & Seminara (1998), Ridderinkhof et al (2014), Alebregtse & de Swart (2016), Chernetsky et al (2010), Dijkstra et al (2017). Indeed full treatment of the nonlinearities is not done this way, but since the M4 tide is still generally small compared to the M2 tide, these methods could still work.
- Ln 226-241: I don't think a morphodynamic computation is necessary at all. One could just compute hydrodynamics for a given bathymetry (this would be different when computing sedimentation rates or such). If you do this: what is the final bathymetry?
- Ln 263-264: how can the depth be constant after the morphodynamic computation?
- Eq 8: brackets missing in the cosine.
- Eq 13: is a minus missing in the first term or did I get confused with the sign of u0?
- Ln 332: why do you need the M4 amplitude and phase? It does not appear in Eq. 11-13.
- Section 3.1: I missed the calibration or setting of friction parameter. How was this done?
- Ln 360-363: why include S2 now? This seems inconsistent with the entire method section.
- Fig 5a: you find a contribution from bottom friction while there is no discharge. Is this the effect of tidal return flow?
- Ln 495 'majority of estuaries': I don't think this is obviously true. This would at least need a reference.
- Ln 424-435: why discuss this here. You don't seem to do this explicitly, so this is more a discussion to me. I don't find this very insightful, because naturally the linearized friction does not contain any transfer of energy from one frequency to another.
- Ln 525-527: you should either prove this or don't mention it.
- Ln 531-535: I can't follow this. Again this refers to the local discussion I commented on earlier.