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Hydrology and Earth System Sciences Discussions

Interactive comment on "An analytical solution for tidal dynamics in the Yangtze Estuary, China" *by* E. F. Zhang et al.

Anonymous Referee #1

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In this manuscript an analytical model, originally developed by Savenije (2005) for a single channel, is used to describe tidal dynamics in the branched Yangtze estuary. The main conclusion is that the model ′can very well describe the tidal dynamics in a branched estuary as well′ (p2231, l4-5).

My overall assessment however is that this manuscript should not be published, because of the following reasons: 1. serious lack of information about, and justification of their model analysis, which strongly deviates from the standard literature on this topic; 2. the model is applied to a branched estuary, but no information about domain and conditions at seaward boundaries, landward boundaries and junctions is given; 3. the authors ignore important work on tidal dynamics of the Yangtze estuary, which they should be aware of; 4. the model misses essential physics; 5. results shown are limited to water levels and travel times, for which a simple linear model would suffice.

Below I explain these points in more detail.

Ad 1. The way that the authors construct their solutions is completely unclear. First of all, the transformation of Eqs. (4)-(5) to Eqs. (6)-(9) is written down without any explanation about assumptions, conditions, physics, etc, except for a reference to earlier work by Savenije. In this way the authors force the reader either to consult earlier papers, or simply to believe what is written here. The way that p2216-2218 are written now is a bombardment of equations, without a proper story about what is behind all this. And why is it all needed (see my point 5)? The authors do not seem to realize that their approach is entirely different from what is done in the standard literature on this topic, i.e., assume a small Froude number and parameter r_s of order 1 and construct solutions by perturbation methods (cf. Parker, Tidal Hydrodynamics, 1991, Friedrichs & Aubrey, 1994). Does their model not have such constraints, as their Table 1 shows values of r_s up to 3? In other words, are their solutions valid for any value of the model parameters? I have serious doubts about that. What I also think that the authors should not only justify their model with field data, but also by showing that it can reproduce results of other models as well. Otherwise, for readers this will seem yet another model with no links to available literature.

Ad 2. As the authors state on p2220 l7 tides in the Yangtze estuary have an irregular semi-diurnal character. Yet, they do not specify any information on what tidal forcing they impose at their seaward boundaries. The same holds for the landward boundaries, as well as for the conditions that are needed at channel junctions. So, what domain and what conditions are used? The fact that in the manuscript frequently the term ′standing wave′ is written (e.g. p2218, l11; p2226, l28) makes me rather suspicious about the conditions used, as the Yangtze estuary has no reflecting walls in the tidal reaches. Besides, the authors frequently state that ′the estuary functions as an entity′ (p2222 l21, section 5.4). It is unclear what they mean by this; it seems that this has something to do with the conditions that they impose, but what?

Ad 3. It surprises me that the authors do not cite recent work on tides in the Yangtze estuary, carried out by researchers who work at the same institute as the first author (e.g., C. Jiang, Geomorphology 2012).

Ad 4. River discharge in the Yangzte estuary is extremely large, in particular in the wet season. Surely, fresh water discharge will significantly affect the friction experienced by the tidal wave. Although in the manuscript quite some text is devoted to fresh water discharge, it finally turns out (p2230, l25) that it is not taken into account. In my opinion, it is an absolute must to include the fresh water discharge, certainly given the fact that it is obvious how to do it (Buschman et al., Water Res. Res. 2009).

Ad 5. Results shown are limited to tidal amplitudes and travel times. For this, a linear model suffices, so what is the point in using their full model? If the full model is used, then please also add information about sea level curves, velocity curves, tidal asymmetry, interaction between different tidal harmonics, and compare these results with field data as well (there is ample of such data available).

Some final remarks:

* The end of the introduction should contain a short paragraph in which an outline of the rest of the paper is given. * Section 3 and section 2 should be exchanged; now the model uses information that is explained onlu much later. * p2214 l23 is not correct; also Lanzoni & Seminara (1998) maintain nonlinear friction (that is even the clue of their paper). * The term d(z_b+h)/dx in the definition of sigma is obsolete, as z_b+h=0. Also, it is unclear whether, and if so how, the term involving sigma is taken into account. * On p2224 l21 turbidity characteristics of the Yangtze estuary are mentioned, but no reference to measured values is given (it concerns information that has been published). * p2226 l1: Here, also Friedrichs & Aubrey (1994) should be cited, * p2228 l9: The term ′ideal′ is subjective. Please use ′synchronous′.

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