Hydrol. Earth Syst. Sci. Discuss., 9, C1819–C1824, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C1819/2012/

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

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Received and published: 29 May 2012

Dear anonymous referee #1,

Thank you very much for detailed reading of our manuscript and providing us with your critical comments. Although we appreciate the critical reading and comments, we fear that some of the remarks are a bit too harsh. However, we shall take them at heart and will do everything to take away the negative impression that the paper apparently generated. The following is our reply to your comments one by one.

Ad 1.

About our model analysis. You indicate that we provide hardly any background on the

model. Indeed the information is minimum and we realise that more background on the assumptions and conditions is required. However, you should realise that this model has been extensively described and published in the literature, where it has been compared to other models, including the ones that you mention. The most recent and most extensive is in Savenije et al. (2008) and in Toffolon and Savenije (2011). Therefore, in this paper we decided to merely present the main equations, essentially to avoid duplication and a too long paper. We simply cannot repeat the entire methodology in every paper that builds on previous papers. But, you are right, the assumptions and conditions of the model should be clarified, and we shall also add a narrative of the theory behind the equations.

About our method being entirely different. Yes, it is based on an entirely different way of linearisation. In fact we do not linearise the friction term or neglect the advective acceleration term. Our friction term retains quadratic friction and the effect of a periodically varying depth. The damping equation is obtained by subtraction of the HW and LW envelope curves, after which explicit expressions for tidal damping, wave propagation, etc. can be obtained. The fact that our method is different from the mainstream approach does not mean that it is not correct (this has been amply demonstrated and justified in the papers referred to in the article). Although the method is different, it uses the same assumptions as mainstream approaches: it is only valid for small tidal amplitudes and small Froude numbers. In Savenije et al. (2008) we demonstrated that our approach is as good or even better than the classical approaches. By the way: what is wrong with providing a new and entirely different method? Is that not what scientific progress is about?

About your thought 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. As discussed above, we have already extensively done so in earlier publications. Savenije et al. (2008) provides this comparison. However, we think that a field application is the best way to justify the model, because we cannot be sure the results of other model are correct either. For HESS, the use of actual field information to test and validate the model is far more interesting than a purely mathematical comparison between models. Moreover, this paper is not about model comparison, but about showing that the explicit analytical method can be applied to a branched estuary system as well.

Ad 2.

About the seaward and landward boundaries, we indeed do not specify them separately. They are the estuary mouth and Datong station, which is the tidal limit of the Yangtze Estuary. We indicated them both in Fig. 1. Indeed, the dominant tide at the downward boundary is semi-diurnal. In the final paper we shall provide a Table with information on the tidal characteristics and on the discharge at Datong.

About conditions at channel junctions, we use the weighted average of the two channels whose cross-sectional areas at the junction are taken as the weight, which is mentioned in section 4.1 (Approach). We shall clarify this further in the final document.

About the “standing wave” indicated in our manuscript. We only refer to the apparently standing wave in the first reach of the North Branch, a very short reach (5 km). As the referee should have noted, we don’t say that a "standing wave" occurs, but that an "apparently standing wave" occurs. From the field data we can see that this is indeed the case, see Figs. 3 and 4. It is true that a purely standing wave does not occur in the North Branch, the right terminology is that it approaches a standing wave, or that it behaves like a standing wave (an apparently standing wave). In agreement with earlier discussions with Friedrichs, we termed it an "apparently standing wave" (Savenije et al., 2008)

About “the estuary functions as an entity”. It has nothing to do with the conditions we impose. It means that the branched channels can be combined into one, and connect well with the upper reach regularly, like a single channel. We can compute the values at junctions based on both separate and combined topography, the results are similar. This phenomenon was earlier described, in relation to salt intrusion, by Zhang et al.

(2011) and by Nguyen and Savenije (2006) for the Mekong (all referred to in the text).

Ad 3.

About the recent paper (C. Jiang, Geomorphology 2012) we indeed did not cite it, which surprised the referee. We presume it refers to “Effects of navigational works on morphological changes in the bar area of the Yangtze Estuary” by Chenjuan Jiang, Jiufa Li, Huib E. de Swart (Geomorphology 139–140 (2012) 205–219). However, the suggested paper is not about tidal processes at all. There is no clear reason why we should cite this paper. However, since it also deals with the Yangtze Estuary, we shall mention it in the revised Section 3.1.

Ad 4.

About the fresh water discharge, it indeed affects friction, with larger impact in the upper reach during the wet season. We did discuss this in the paper. Although the discharge of the Yangtze River is large, the tidal flow is much larger. Based on our calculation (Zhang et al., ECSS, 2011), using the average river discharge of 16,700 m3/s during dry season and 40,000 m3/s during the flood season at Datong station, the Canter-Cremers number N (N=QT/Pt, where T is the tidal period, Pt is the tidal prism) during spring tide is 0.0025 (dry season) and 0.006 (flood season) for the North Branch, and 0.1 (dry season) and 0.24 (flood season) for the South Branches, respectively. During the dry season the impact of river discharge is not large particularly in the downstream part of the estuary, which is the part of the estuary we consider. For the condition of the wet season, we also think that the river discharge must be included. However, it is impossible to do all these things in one paper, particularly due to the size limit. There are several ways to take account of the river discharge. In our methodology discharge can be taken into account using the method of Horrevoets et al. (2004), cited in the paper. Indeed Buschman et al. (2009) also take account of river discharge to calculate the residual slope, but the method of Horrevoets et al. (2004) is fully compatible with our method, while the method of Buschman is not. Buschman’s method requires the observation of the velocity amplitudes of the main tidal constituents (which they did with an ADCP). We don't have such information and our method also does not require it. In fact we prefer our method to be applicable to more readily available information. The referee advertises the use of Buschman's method, although it has two large disadvantages: 1) it requires detailed velocity observations and 2) it does not consider the depth fluctuation over the tidal cycle but merely U/U/. As a result Buschman et al. state that they use a different roughness for ebb and flood flow. This is supposedly due to a different bottom roughness during ebb and flood, but this is highly speculative and probably wrong. The fact that friction is higher during ebb is because of the depth being smaller. Our method takes this effect into account, and so does Horrevoets et al. (2004). Both of which use the envelopes at HW and LW to obtain the equation for tidal damping. We think that the inclusion of tidal discharge requires a paper of its own. Therefore, for this paper we decided not to consider the effect of river discharge, which we motivated in the paper.

Ad 5.

About the application to the Yangtze. In our paper, it is true that we do not present all the variables related to tidal dynamics. This is due to the limited data available. The results shown are mainly about the propagation of the tidal wave. If this is considered a real problem, then we could change the title to be about "tidal propagation" instead of “tidal dynamics”. You mention that ample data is available. We are not aware of that. We have a limited number of point velocity observations that we may include in the paper, but this provides very limited information. From the referee's reaction we understand that he is very familiar with the Yangtze Estuary and has access to more information. If he would be willing to share this with us, then we would appreciate this very much.

About some final remarks:

Thank you very much for your valuable suggestions and corrections. We agree with

most of your final remarks and shall make appropriate corrections, with two exceptions:

p2228 l9: The term ‘Ideal’ is subjective. Please use ‘synchronous’.

We disagree. The term ideal estuary is very common and correct (e.g. Savenije, 2005 and numerous references therein). An ideal estuary is an estuary where there is no tidal damping because the increase of energy per unit width as the wave propagates in a converging channel is exactly compensated by friction. Alluvial estuaries have the tendency to become ideal estuaries (e.g. Savenije, 2005 and further references therein). "Ideal" is a much better term than "synchronous", which suggests that water levels vary in sync and that HW and LW are reached simultaneously along the estuary axis. This is not true because in an ideal estuary the tidal wave propagates (undamped) with the classical celerity (c0). Only a 'standing wave' is synchronous, since for a standing wave HW and LW occur simultaneously in the entire estuary.

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.

No, this term is not obsolete. There is a residual slope in the envelopes that are used for the derivation of the damping equation. You are right that this residual slope is subsequently considered to be negligible and hence the sigma term is neglected. We shall mention this in the final paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 2213, 2012.