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Interactive comment on "The open boundary equation" *by* D. Diederen et al.

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1 Summary

Using the special case of a 1D frictionless, straight infinitely long channel with horizontal bed the authors derive an expression between the spatial and temporal derivatives of water level and flow velocity (Eq. (22)). It is then claimed that this expression has a more general validity beyond this special case. This is then used to study the nature of phase lag between horizontal and vertical tide. Also, an application is proposed by which a combination of the shallow water equations (1) and (2) are used in conjunction with Eq. (22) to obtain cross-sectionally averaged velocities and roughness values.

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Interactive Discussion
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2 Judgement of the manuscript

I don't think this manuscript is acceptable for publication at all. The authors claim general validity of a relation (hereafter referred to as "Eq. (22)") but are not able to back this in any convincing way. In fact, it is easy to find counterexamples (see below). Additionally, the proposed application of Eq. (22) in Sect. 6 is cumbersome and inferior to already existing methods. Finally, I don't find that the issue on phase difference between horizontal and vertical tide is treated in an adequate way.

The critical case about the validity of Eq. (22) is presented very poorly and does not seem to have been contemplated sufficiently by the authors. This is - to me - of pivotal importance to the novelty and quality of the work. I think the authors have a lot of work to do at this point, more than is required for "acceptable after major revisions". Hence I find this contribution not acceptable for publication so that I am compelled to reject it. In the remainder I will point out in more detail which considerations have led me to finding the manuscript not acceptable. These comments may also give a few ideas about what the authors are expected to do for a manuscript that can be re-submitted. I must confess, though, that I am very sceptical about the validity of Eq. (22) beyond very specific cases.

3 Eulerian derivation of Eqs. (16) and (22)

It is useful to note that Eqs. (16) and (22) can be derived from an Eulerian approach as well. Multiplying Eq. (1) by u_x and (2) by h_x, followed by substraction yields

$$\begin{aligned} h_t u_x - h_x u_t &= W h_x - \beta u h h_x + g\zeta_x h_x - h u_x^2 \\ &= W h_x - \beta u h h_x - h\left(u_x^2 - g\zeta_x \frac{h_x}{h}\right) . \qquad \text{(A)} \end{aligned}$$

OSD

12, C423–C431, 2015

Interactive Comment

If friction is zero and the channel is straight only the term is square brackets is nonzero. Assuming a horizontal bed then yields

$$h_t u_x - h_x u_t = h(u + 2\sqrt{gh})_x (u - 2\sqrt{gh})_x \ ,$$

which is indeed zero for constant Riemann invariants, that is Eq. 16 holds.
This is the point up to which I agree with the authors. I think Eq. (A) can be used to identify when and why Eq. (22) is not obeyed.

4 Major objections

4.1 The analysis regarding Eqs. (26) and (27)

I am completely at a loss as to why to use/present/discuss Eqs. (26) and (27). The authors want to check the validity of Eq. (22) and this can be done easily (at least in principle) directly (as they attempt with Fig. 4). Why they instead want to consider *two very nonlinear* equations completely escapes me. As far as I can see most of the part between Eq. (25) up to (28) is redundant for this reason. The agreement shown in Fig. 2 does not impress me in view of the large scatter in Fig. 4 (see below), that is: Fig. 2 may simply refer to a case for which (22) happens to hold to some extend. The discussion of the Pearson correlation coefficient is not relevant either: one wants to check an exact equality, say "$A = B$", not whether A and B correlate. Obviously $A = \sin(t)$ and $B = 0.001\sin(t)$ correlate perfectly but they are clearly unequal.
For completeness: while I don't understand why Eq. (26) and (27) are considered, it is inconsistent that the authors only elaborate on Eq. (26).

OSD
12, C423–C431, 2015

OSD
12, C423–C431, 2015

4.2 The validity of Eq. (22)

4.2.1 Problems with the presented evidence for Eq. (22)

In Sect. 3.2 the authors claim to present numerical support for the validity of Eq. (22). This is essentially shown in Fig. 4. I cite the author's main conclusion here (last part of Sect. 3.2):

"Small values of Eq. (22) demonstrate considerable scatter, which may be explained by numerical errors, which are relatively large in this range. Large values give an almost perfect agreement."

This is, as far as I can see, the closest the authors get to substantiating the validity of Eq. (22). Sorry, but this is by far not acceptable to even remotely claim that Eq. (22) has more general validity than the particular case discussed in Sect. 2.3. It is clear that only a small minority of the cases shown in Fig. 4 match the red line. It is not even clear which kind of cases do show good agreement with Eq. (22). Suggesting that the huge scatter is due to numerical errors does not mean that Eq. (22) holds for those cases and does not release the authors from the responsibility to demonstrate the validity of Eq. (22) explicitly.
Besides, numerical solution of Eqs. (1) and (2) is standard stuff and the parameter ranges mentioned in Tabel 1 don't look particularly weird to me. So the authors simply have to make sure that (1) and (2) are solved accurately for whatever case they consider. Period.
Additionally, the authors should discuss *in detail* which kind of cases do agree well with Eq. (22) and which don't and *why*. These are things that readers want to know. Also the authors should *quantify* the relative difference between the two terms in Eq. (22) for various cases. That is: be more precise about how small/large deviations from Eq.

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Discussion Paper

(22) can be. Perhaps Eq. (A) above may be helpful here.

4.2.2 Examples discussed in Appendix A

Regarding the examples discussed in the Appendix A I point out that (A1,2) actually refer to solutions which are constant on characteristics $\varphi(x,t) =$const. In that case Eq. (22) always holds and in fact, I think this is the *only* case in which it is true. I think that Eq. (22) is merely a mathematical reformulation of the existence of invariants *for a specific case* rather than a physics based law *with general validity*. I don't think it holds for propagating tidal waves that have a spatial variation of amplitude. If the authors think otherwise, they should come up with a clear example for which Eq. (22) holds and discuss it *in detail*.
I don't think that Eq. (22) holds for the case presented in A2 as Eqs. (A5) and (A6) actually constitute a counterexample (see below).

4.2.3 Counterexample

To further illustrate my doubt regarding the validity of Eq. (22) I mention an elementary counterexample for which Eq. (22) is not valid. Actually, this counterexample is of the form discussed in Appendix A2 and obeys Eq. (A12). It is the case of linear tide (i.e. $\eta \ll \overline{h}_0$) in an infinitely long straight channel with horizontal bottom, subject to linear friction (i.e. $W = ru/\overline{h}_0$). The latter has been used to obtain many of the results that the authors cite in the Introduction. The solutions to ζ and u read

$$\begin{aligned} \zeta(x,t) &= \eta \exp(-\mu x) \cos(kx - \omega t) \, , \\ u(x,t) &= \alpha \sqrt{g\overline{h}_0} \exp(-\mu x) \cos(kx - \omega t - \varphi) \, , \end{aligned}$$

OSD

12, C423–C431, 2015

Interactive Comment

where μ, k and φ are all related to the dimensionless friction parameter $r/(\omega\overline{h}_0)$. Evaluating $\zeta_t u_x - \zeta_x u_t$ then gives

$$\zeta_t u_x - \zeta_x u_t = -\frac{4\pi^2\mu}{k}\frac{\eta^2}{\overline{h}_0 T^2}\exp(-2\mu x)\sin\varphi\ ,$$

where $k = \omega/\sqrt{g\overline{h}_0}$. This quantity is in general nonzero. It is zero only if $r = 0$, which coincides with both μ and φ vanishing as well. The solution then is of the form discussed in Appendix A1.

This counterexample points to an error in Appendix A2. Indeed, for the above example Eq. (A11) reduces to

$$\tan(kx - \omega t) = \tan(kx - \omega t - \varphi)\ ,$$

which is clearly not true unless $\varphi = 0$. Another counterexample to Eq. (22) is the case of a linear tidal wave in a frictionless exponentially converging channel with a horizontal bottom.

I think it is very problematic for the validity of Eq. (22) if such elementary analytical cases do not obey it.

4.3 Phase difference between horizontal and vertical tide (Sects. 1 and 5)

The analysis in 5.2 raises questions. Why do the authors consider a Lagrangian analysis while the phase lag issue is posed from an Eulerian viewpoint? I don't think that Lagriangian phase lags translate into Eulerian ones in a straightforward way, certainly not for non-linear tides. So the explicit link between the two escapes me. I think the authors should clarify this relation thoroughly.

The analysis in Sect. 5.3 assumes that Eq. (22) is generally valid, which I think is not correct. Here I would point out the fact that there is a clear example where Eq. (22) will certainly not hold for an infinitely long channel either, namely the case of a linear

OSD
12, C423–C431, 2015

tidal wave in an exponentially converging channel without friction. In that case standing wave behaviour (infinite celerity) occurs if $2kb < 1$.

4.4 Applications (Sect. 6)

Leaving aside that I do not think the validity of Eq. (22) is demonstrated at all, I do not find the applications of too much practical use either. Explicitly, if water levels and the estuary's geometry (i.e. $B(x)$ and $Z(x)$) are known there is a far more powerful method to obtain velocities, namely the cubature method (e.g. ()Gosh 1998). This adopts mass conservation (Eq. 1) which can be recast in the form

$$B\zeta_{\mathsf{t}} + Q_{\mathsf{x}} = 0 \ ,$$

where $Q = Bhu$ is the instantanous discharge. From this discharge and width averaged velocity are readily derived provided the discharge is known somewhere in the estuary (e.g. upstream river discharge). This method is superior to the authors' proposal because

- it only uses mass conservation, which is an *exact* equation,
- $Q(x,t)$ is thus easy to solve accurately by standard numerical methods,
- it does not suffer from the "issues" the authors list in lines 7-15 on pg 942,
- does not require Eq. (22) to be valid.

Likewise, having obtained u from cubature immediately allows for the determination of the friction term W in (2). Only after obtaining W does on have to worry about the precise friction law (Chezy, Strickler, linear) to obtain hydraulic roughness parameters. Really, I don't see any added value or practical use to what is presented in Sect. 6.

OSD

12, C423–C431, 2015

5 Minor issues

Apart from the major objections I have pointed out above I also have some comments that are of lesser importance:

- lines 8-29 on pg 928 (also Sect. 5). Is this phase lag between velocity and water level really such a big issue? The literature that the authors cite should be sufficient for any serious researcher to realize that phase angle not a correct indicator of a standing wave. I think a standing wave is adequately characterized by the simultaneous occurrence of local high and low waters throughout (a part of) an estuary. That is: the celerity is infinite. I think horizontal and vertical tide then have a 90 degree phase difference although I don't think that it is formally proven. The converse (90 degree phase difference) is inconclusive, as is demonstrated by the cited Friedrichs & Aubrey (1994) paper.

- Sect. 2.3: what are the boundary conditions that are used here, in particular at $x = \infty$? If one assumes only a landward propagating wave, doesn't this already imply that $R_2 = 0$ as one does not want information to travel seaward? Please clarify this.

- Sect. 3.2, lines 19-20 on pg 935: "The seaward boundary ... wave to adjust". This sounds strange. Isn't the boundary condition at the seaward side something that can be accurately imposed numerically? The solution near $x = 0$ may have to adjust in time (depending on the initial condition) but not in space. I don't expect any effect from the reflected wave here as I expect it to have decayed. For the landward boundary this may indeed be different due to reflection.

- lines 11 and 13 on pg 936: "Small values", "Large values": inaccurate use of words. I think the authors mean small or large values of $\zeta_x u_t / F_{\text{sc}}$ and $\zeta_t u_x / F_{\text{sc}}$.

OSD

12, C423–C431, 2015

Interactive Comment

- lines 3-7 on pg. 937, Fig. 5. I think cases B, C and D are only relevant for transient behaviour, not for the long term (purely periodic) time behaviour. I think the authors' interest is with this latter case.
- Sect. 5.2, Fig. 7. How is an "ideal estuary" defined for the present case with non-linear bottom friction? Is this in a time averaged sense? If so, this is effectively the same as adopting Lorentz linearisation of the friction. Please clarify.
- derivation of Eq. (35) was not straightforward to me. Perhaps this could be done in an Appendix.

References

Gosh, S. N., *Tidal Hydraulic Engineering*, A. A. Balkema Publishers, Rotterdam, 1998

Interactive comment on Ocean Sci. Discuss., 12, 925, 2015.

OSD

12, C423–C431, 2015

Interactive Comment