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

Anonymous Referee #2

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1 Introduction

Below I will reply to the various contributions that the authors have posted following my first report. The response below is effectively split in two parts. The first part refers to contributions AC C455, SC C532 and also - to some extend - AC C412 and SC C526 that followed the comments of Referee #1. The second part is devoted to SC C558 and SC C588.

2 Summary

I have great problems with the replies I got with respect to Part 1, and based on this I am still not convinced of the validity of Eq. (22).
In Part 2 I will explain that the approximate validity of Eq. (22) - if it is found - actually seems to refer to the situation where the tidal wave propagates in a nearly undamped way. In that case, Eq. (22) is *approximately* satisfied as this is close to the first case from the Appendix of the manuscript. Actually, it is this near-undamped propagation (which is due to a balance between friction and convergence) that is remarkable, and Eq. (22) is merely a property of is. Hence Eq. (22) - if anything - is not so much of an "Open Boundary Equation" as an "Equilibrium Condition". Indeed, Eq. (22) itself is in general rarely satisfied near the boundary.

3 Part I: reply to AC C455 and SC C532

I have read these replies to my comments. Unfortunately these answers are not satisfactory to me. Of course I will explain this below. Briefly summarised I think the authors have not taken my points seriously, sometimes in a way that really bothered me.

3.1 Validity of Eq. (22)

3.1.1 Numerical(?) issues

I cite the authors' answer in their summary (pg C456)

In the article we indicate that the numerical analysis of convergent and frictional estuaries by definition has (numerical) errors, so that it cannot be used as conclusive evidence (page

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934, line 10). The only solution that can be used as a full reference test is an exact, analytical solution of the 'Saint-Venant' equations, which does not (yet) exist and may not exist for some time to come or may never exist.

I fact, in terms of method I am far more optimistic. I think that Eqs. (1) and (2) of the manuscript can be accurately solved by numerical mean and can very well be used to check Eq. (22). Indeed, one expects that the authors use a code that will give convergence to the full solution as the discretization is improved (smaller grid size, smalle time step). Apart from boundary conditions (see below) I really don't know where numerical problems should come from. Besides I don't think using a code that has significant numerical errors should be accepted, so if this is the case then the authors should simply redo their work with an improved code that *is* sufficiently accurate for their purpose.
As to the up-estuary boundary condition it has been pointed out (SC C526) that the adopted method is adequate in that it is believed to be non-reflective so I assume that this will not give numerical problems. If it does, then I would urge the authors to really solve the equations on a semi-infinite domain (i.e. infinite L). There are methods for this (e.g. rational Chebyhev polynomials, see ()Boyd 2001) so that "numerical reflection" is not an issue anymore.
As to the down-estuary (i.e. seaward) boundary condition the authors argue (SC C532) that the solution is initially affected by the boundary condition at $x = 0$ and then - as it propagates - becomes influenced by the internal dynamics of the basin. This is true and this is referred to as external (i.e. externally driven) and internal tides (which emerge from non-linearities). But these are simply two components of the tide and I have never seen this being associated with an "adaptation length". This would indicate that water level and velocity could exhibit a strong gradient near the boundary due to a mismatch between boundary condition and internal solution. This may happen for an elliptic set of equations but not for the hyperbolic system considered here. Likewise I have never seen solutions of these equations that shown such strong boundary

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layer behaviour when a water level Dirichlet boundary condition is adopted. Moreover, unlike the authors' argument the external tide usually does not disappear over a short distance as it propagates. So I see no boundary condition effect here, and no numerical issues either.

The analogy in SC (C532) is not relevant. It considers a *parabolic initial value problem* rather than the *hyperbolic boundary value problem* which is of interest here. The assymptotic result (two solutions that become the same) says that when a blob is spread over a domain that is large compared to its initial extend, the solution becomes independent of the details of the initial conditions. This is simply something that is typical of diffusive systems.

3.1.2 "Origin of errors"

In the reply to Reviewer #1 (AC C412) the line of reasoning on pg. C413 ("In our view ... travel with the wave celerity.") bothered me. First of all, I don't think a water level boundary condition will give such significant numerical errors (see above) and the landward condition is believed to be effectively non-reflective.
Furthermore I find statements like "From these images it can be concluded that errors/deviations from Eq.(22) enter the domain from the boundaries and travel with the wave celerity." non-balanced as they implictly suggest the validity of Eq. (22) and hence that deviations from it have a numerical origin. Please show this by elaborating on the adopted numerical scheme! As far as I can see, Eq. (22) may simply not hold and this "inequality" may equally be travelling into the domain.

3.1.3 Counterexample

In reply to the counterexample I pointed out (Sect. 4.2.3 in my comments) the authors state

However, we will further discuss the counterexample presented and we will show that it does not agree with the numerical solution of Eqs. (1) and (2), including scenarios with small amplitude-to-depth ratios.

So? This is then a reason to further ignore this counterexample? This really bothered me!
First of all, one *does* expect this difference since the counterexample uses a linear bottom friction law while the authors use a non-linear formulation. The latter case will generate a significant contributions to the M_6 constituent that modifies the M_2 solution that I put forward, and this will render $E = \zeta_t u_x - \zeta_x u_t$ time dependent. Likewise using a higher order analytical approximation in the linear friction case will also give a time varying E.
Second, while the authors regret the lack of exact solution for Eqs. (1) and (2) there is a very fruitful approach that can yield well-defined approximate solutions provided that the bottom friction is linear in velocity. This has been adopted widely. In fact the authors cite this type of work in the Introduction, and have used it themselves ((()Savenije 2006; ()Toffolon & Savenije 2011). Typically, assuming that α is small one can obtain approximate solutions as a series expansion, e.g.

$$\zeta = \zeta_0 + \alpha\zeta_1 + \alpha^2\zeta_2 \ ,$$

and likewise for u. For the counterexample this series is truncated after the first term. Expressions for ζ_0 etc. can either be found analytically or accurately solved for by numerical means.
My point is that this allows a study of Eq. (22) within the context of semi-analytical

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solutions. The problem is that this approach indicates that Eq. (22) does not hold. The authors should appreciate this as a problem for their claim *and study it* rather than ignore it.

3.1.4 Presented evidence

I now appreciate the origin of Eq. (26) in the manuscript. However whether one looks at this equation, correlation (which I still don't find relevant) or things like TRC the central question is: does Eq. (22) hold? An equation is something like $A = B$ hence $A - B = 0$. The most direct way to study this is to consider the ErrorImages that the authors included in their answer to Reviewer #1. This shows spatio-temporal behaviour of f_2 which measures the deviation from the average of E in units of the standard deviation of E.
These ErrorImages show values of f_2 that seem to vary greatly, e.g. the "most linear" cases 21, 24, 65, 74 and 98 show that E varies between roughly -10 and +10 while it should be "small". But what do the authors mean by "small"? When is this variation "small enough" and for which purpose(s)? And why? An in-depth well-motivated discussion on this issue is totally absent. To summarize: the authors claim an (approximate) validity of Eq. (22) that I simply do not see in the ErrorImages.

3.2 Reply to the "cubature method"

Here I reply point-by-point, citing the authors' answers first.

By only using the mass balance, the 'cubature method' will show larger errors farther away from the location where the discharge is measured, since it will use numerical approximation of the 'exact' mass balance.
With all due respect, but this is nonsense. The discharge follows simply from integrat-

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ing the (essentially tabulated) function $B\zeta_t$, which can done by any desired method (e.g. trapezoidal rule). Such procedures do not show large errors. There is of course an error related to the discretisation of $B\zeta_t$ but this error is present in the authors' approach as well.

The measurement of the discharge will contain measurement error
True, but for the tidally dominated part of an estuary this error in river discharge is not really important

Many estuaries in the world are ungauged
If "ungauged" means that water levels are not measured, I think this is a problem for the authors' method as well.

Finally, the applications in Sect. 6 also include the mass balance Eq. (1). Ad- ditionally, they include the momentum balance Eq. (2) and the additional open boundary Eq. (22).
Yes, and the authors are forced to point out three problems that arise because (1) is used - no such issues for cubature. Besides, the general validity of Eq. (22) is not substantiated which is also a problem.

4 Part 2: SC C558 and SC C588

I thank the authors for SC C558 as this finally gives a case that is more fruitful to analysize (horizontal bottom) than most of the numerical cases they sent earlier. This was a wise move. I also thank the authors for SC C588 which fullfilled most of my requests.

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4.1 Adopted measure of accuracy

The authors use time averaged values of $\zeta_x u_t$ and $\zeta_t u_x$ for their Figs. 2. This is not a wise choice. I think a more fair (and more commonly used) measure would be I/J, where

$$I = \sqrt{\overline{(\zeta_x u_t - \zeta_t u_x)^2}} \quad , \quad J = \min\left(\sqrt{\overline{(\zeta_x u_t)^2}}, \sqrt{\overline{(\zeta_t u_x)^2}}\right) \; .$$

I think this will yield far higher deviations for, say, the strongly convergent cases which really show $O(1)$ temporal differences (e.g. variant 1, Figs. 3-5) which do not show up in Fig. 2.

4.2 Approximate validity of Eq. (22)

From variant 0 I noticed that the degree to which Eq. (22) became more or less satisfied actually concided with the dominant M_2 water level and velocity becoming nearly constant. This can be understood from the linearised shallow water equation with the standard Lorentz bottom friction, i.e.

$$u_t + g\zeta_x + W = 0 \; , \tag{1}$$

$$B\zeta_t + [B\overline{h}_0 u]_x = 0 \; . \tag{2}$$

Here

$$W = \frac{8}{3\pi} c_{\mathrm{d}} U u \; ,$$

where U is the M_2 velocity amplitude and $c_{\mathrm{d}} = g/(K^2 \overline{h}_0^{1/3})$. Now in general U depends on x so the solution of this system requires iteration. However if we seek propagating constant amplitude solution, i.e.

$$u = U \exp[i(kx - \omega t - \varphi)] \quad , \zeta = A \exp[i(kx - \omega t)] \quad ,$$

we find

$$\omega^2 = g\bar{h}_0 k^2 \,, \tag{3}$$

$$U = \frac{3\pi}{8}\frac{\omega\bar{h}_0}{kb}\frac{K^2\bar{h}_0^{1/3}}{g} \,, \tag{4}$$

$$A = \bar{h}_0 \frac{U}{\sqrt{g\bar{h}_0}} \sqrt{1+\frac{1}{(kb)^2}} \,, \tag{5}$$

$$\tan\varphi = \frac{1}{kb} \,. \tag{6}$$

Here it has been used that the width variation for variant 0 is very close to exponential. Basically what happens is that the fact that the linear friction coefficient depends on U allows for a “tuning” between convergence and bottom friction. For a constant linear friction coefficient this tuning is not possible and b and the friction coefficient then need a very special relation. So in this latter case (which includes the counterexample) one indeed cannot expect Eq. (22) to hold.
For variant 0, this balance gives $U = 0.52$ m/s, $A = 0.64$ m and $\varphi = 35$ degrees. The amplitudes are only slightly lower than the numerical results while the phase difference agrees well. The results of Eqs. (3)-(6) are shown in the Table below.

Variant #	b (km)	K ($m^{1/3}$/s)	kb	x_{bvar} (km)	U (m/s)	A (m)	φ (degree)
0	100	45	1.4135	1381.6	0.51893	0.64179	35.278
1	25	45	0.35337	345.39	2.0757	6.29	70.538
2	300	45	4.2405	4144.7	0.17298	0.17943	13.269
3	100	20	1.4135	1381.6	0.1025	0.12677	35.278
4	100	80	1.4135	1381.6	1.6401	2.0284	35.278
5	100	45	1.4135	1381.6	0.51893	0.64179	35.278
6	25	45	0.35337	114.88	2.0757	6.29	70.538

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The parameter x_{bvar} is the distance over which $|dB/dx|$ has become smaller by a factor two as compared to $x = 0$. From this we see that variants 0 and $2-5$ are essentially cases of exponential width variation within the domain of interest.
Variants $2-5$ seem in reasonable agreement with the Table. Note for instance variant 4 where Fig. 7 is in good agreement with $U = 1.6$ m/s and $A = 2.0$ m. A similar agreement holds for variant 3. Hence for these cases it is observed that an approximate balance between convergence and bottom friction occurs.
Strongly convergent cases have $x_{\mathsf{bvar}} < 500$ km so that they become straight channels at relatively low x values; they become therefore damped well within the domain, with velocity and water level amplitude falling far below the equilibrium values listed above. Note that the water level amplitude (6 m, i.e. $\alpha = 0.6$) actually indicates that the linearised equations are not a good approximation anymore for strong convergence.
Due to the asymptotic constant width adopted *all* tidal waves will eventually become damped (albeit beyond $x = 500$ km for cases 0 and $2-5$). For such damped situations, Eq. (22) will not be applicable as it only holds for the near constant-amplitude phase.
From the above it is seen that the “adaptation length” near the seaward boundary is not really a boundary effect as such - let alone a numerical issue. It is simply the distance over which the wave travels before the approximate balance between friction and convergence sets in - *if* it sets in.

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