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

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We'd like to thank Anonymous Referee 1 for the sharp review. We agree with most points made. First we address the main comments, than we give answer point-to-point.

I). 'Mainly, I think a discussion section on the model limits, including those arising from Fig. 4, is completely missed.'

In the article we discussed the most important model limit, which is that there should not be (classical) reflection.

Additionally, scatter can be observed in Fig. 4, where evaluations of Eq.(22) have been plotted from 100 different scenarios which are forced seaward by a sinusoidal excitation and have a (weakly reflective) open boundary landward. The scale is logarithmic, so that a scatter in the smaller values is visible, which may be significant. The scatter may be explained by numerical errors, or the scatter may be due to a missing a term in Eq.(22), which would indicate an additional limitation.

In our view any numerical experiment contains boundary errors because the exact functional relation at boundaries on both sides of the model domain is inherently unknown (since an analytical solution has not yet been found). These boundary errors are supposed to be (negligibly) small at large distance from the boundaries. However, in the 'ErrorImages' we attached in the supplementary material we are able to show that the errors/deviations of Eq.(22) indeed do originate from the boundaries. First, we have subtracted the left hand side of Eq.(22) from the right side and divided by the scaling factor F_{sc}:

$$f_1 = \zeta_t u_x / F_{sc} - u_t \zeta_x / F_{sc}.$$

Second, since the errors/deviations appear to dampen out farther from the boundaries (as can be observed in Fig. 3), we have corrected by:

$$f_2 = (f_1 - \mu) / \sigma,$$

where μ is the mean value of time series of f_1 and σ is the standard deviation of time series of f_1.

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.

Moreover, we have applied an additional method to obtain velocity time series from water level time series, by rewriting Eq. 27 as:

$$Au_t^2 + Bu_t + C = 0,$$

which can be solved by applying the quadratic formula:

$$u_t = (-B \pm \sqrt{B^2 - 4AC})/(2A).$$

From observation in the numerical models we concluded that from low to high water

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the sign is positive and from high to low water the sign is negative. The result of explicit time integration of $u_t = f(u, h, h_t)$ (with known properties of depth and width convergence and of friction) are compared to the numerical velocity and are added to the supplementary material. We call this method the 'Tidal Rating Curve' (TRC). Just as the 2 applications already discussed in the article, these time series show some deviation around high and low water, which corresponds to the scatter observed in Fig. 4. These deviations may be explained by an imperfect timing of the switch of the sign in this quadratic formula.

Since we don't have an exact analytical solution, it is not easy to explain the scatter. In view of the above we conclude that the major part of the 'errors/deviations' is due to the incorrect formulation of the boundaries, and in particular the downstream boundary.

II). 'Furthermore, in practice dealing with infinitely long tidal channels, means dealing with estuaries, which are typically characterized by the presence of a river discharge. Could the Authors provide any discussion on the possible implications of the above neglected effect?'

Even though landward an open boundary was applied instead of a discharge boundary, this does not mean that there is no discharge in the simulation. The only applied forcing is the excitation of the water level at the seaward boundary. The shape of this forcing determines the discharge that flows through the domain. For the sake of simplicity a sinusoidal forcing was applied in all simulations. Generally, this gives a discharge in landward direction (which is wrong for estuaries), related to Stoke's drift which is an artifact of sinusoidal forcing. This positive discharge is quite small (especially for the convergent channels), which is why it appears negligible compared to the tidal dynamics studied in this article.

III). 'Finally, the Authors provide a practical tool for reconstructing the temporal variation of the velocity based on local observations of the water level, but do not show any application of the above method in the field. Hence, I would suggest them at least to

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mention an existing site where the method could be applied in a satisfactory way.'

The applications follow from the validity of the governing equations and specifically of the open boundary equation. Theoretically, as mentioned in the article, the open boundary equation appears to work if there is no classical reflection in the signal. This means it should be possible to apply it anywhere in a tidal channel/estuary at a large (enough) distance from reflecting objects (e.g. sluices or dams). In order to check it's validity in the field, the open boundary equation should be measured directly, since it does not contain friction, convergence or bed slope which excludes errors in those parameters. To do this, at least two measurement devices are required which can measure high resolution time series of the cross-sectional average velocity (for u_t and u_x). Does anyone happen to have such devices (e.g. H-ADCP) available for testing the equation?

The analytical computations of the velocity (TRC) described above and included in the supplementary material are nice numerical illustrations of what in practice may be possible.

IV). Now follows a point-to-point reaction on uncovered comments.

2) 'Page 932, modify line 21. I think that the Appendix shows that some approximate analytical progressive wave solutions also satisfy eq(22), rather than derives equation (22) from simple analytical approximations.'

Thanks for rephrasing, the sentence was meant exactly this way (the term 'formal derivation' was used in the 'exact derivation' for the 'perfect channel').

3) 'Page 934, equations (26) and (27). Explain the advantage of deriving equations (26) and (27), which is not clear at this point of the paper. Indeed, Eq. (26) and (27) have to be solved numerically exactly like equations (1) and (2).'

These equations are shown to indicate that analytical work is no longer possible. They are the reason to start the numerical work for frictional and convergent estuaries. More-

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over, they are ordinary and therefore we believe they are significant.

4) ‘Page 934, line 20, give some details concerning the way the open landward boundary condition is imposed in the numerical model. Which are the limits of the chosen approach?’

I’ve asked Marco Toffolon to answer this question on the discussion forum separately, since he wrote the code.

7) ‘Page 935, line 11, figure 2a seems to suggest the term containing bed slope (light blue line) is negligible compared with the others. Give some comments on the chosen parameter values (small bedslope and relative tidal amplitude). Are non-linear effects relevant in this case?’

The parameters where chosen such that a location was found where the bed slope effect was big enough to see it’s significance for the chosen Eq.(26). The first attached figure has the same location in the same estuary as in Fig2 in the article, but now the term containing the bed slope is not added in the equation, which clearly shows worse results. Which non-linear effects do you mean specifically?

8) ‘Figure 2: It could be interesting to add a second figure showing the same comparison in a different estuary location (for example x/L=0.8).’

See the second attached figure. As the tidal wave becomes more steep towards the end of the estuary (x/L=0.8), the terms containing the squared derivatives become relatively large. This effect is even more pronounced in simulations with larger amplitude-to-depth ratios.

9) ‘Figure 4: Explain the meaning of the different colors.’

Each color represents dots from a different simulation.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/12/C412/2015/osd-12-C412-2015-supplement.zip

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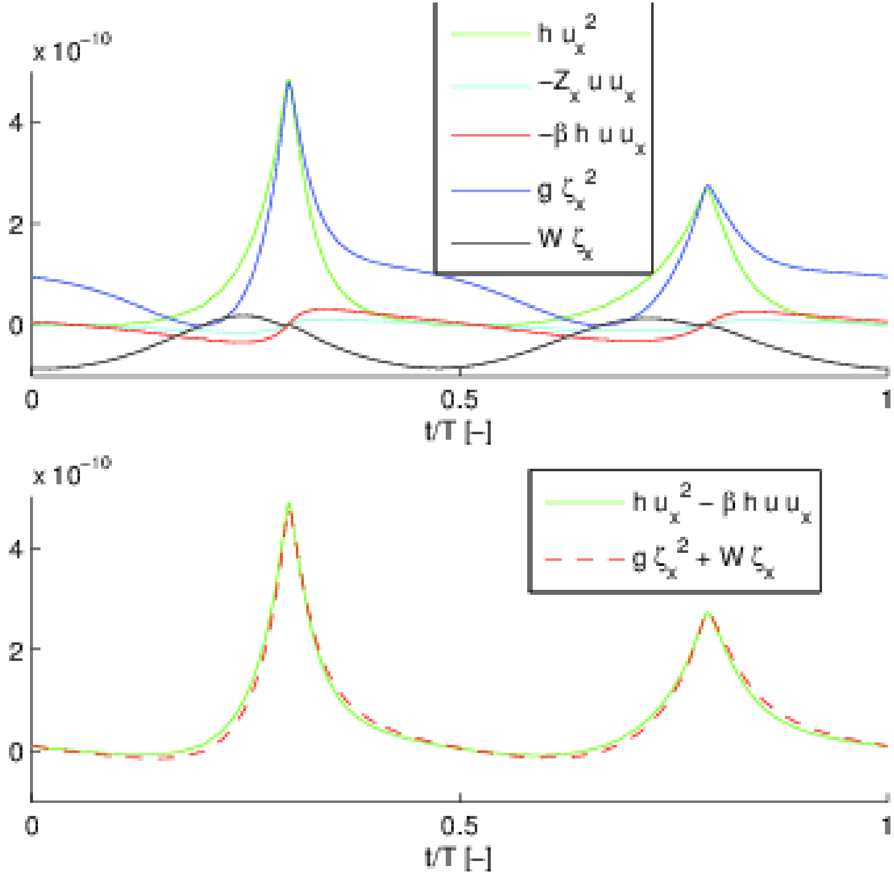

Fig. 1.

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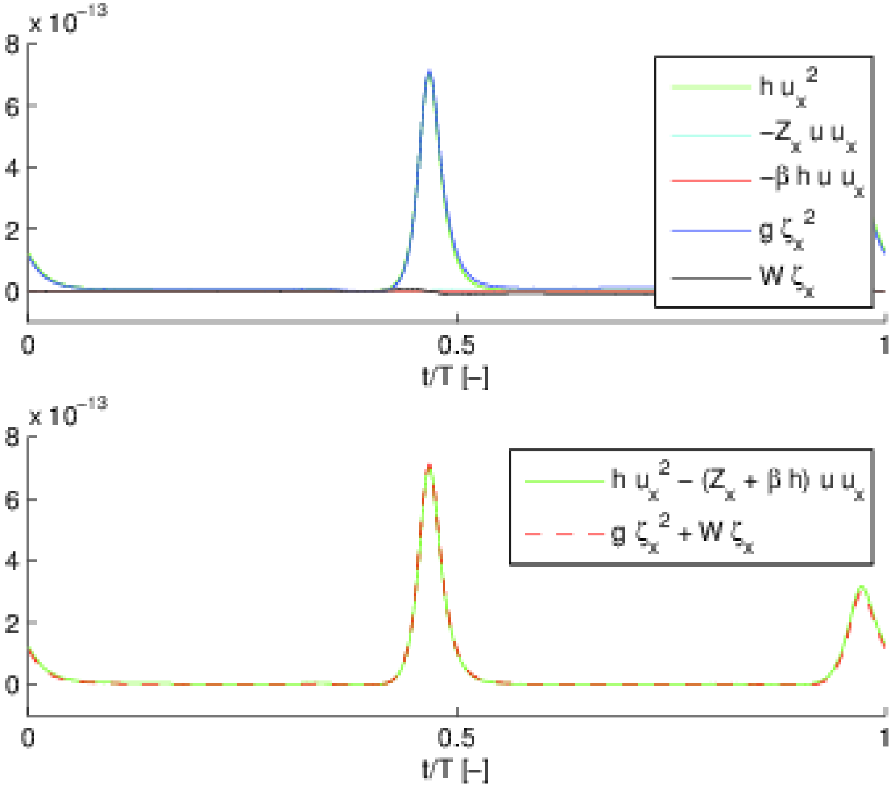

Fig. 2.

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