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Ocean Science Discussions

Interactive comment on "A new parameterisation of salinity advection to prevent stratification from running away in a simple estuarine model" *by* S. Blaise and E. Deleersnijder

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The one-dimensional modelling approach which is here suggested by the authors, does surely open new perspectives for principal estuarine and coastal studies. Under conditions of relatively weak tidal mixing (large horizontal Richardson number, see Monismith et al., 1996) and subsequent relatively stable stratification the general assumption of spatially and temporally constant horizontal density (or salinity) gradients leads to complications phrased as "runaway stratification". With the alternative approach suggested here, this latter assumption is not made, such that temporally and spatially varying density gradients are constructed in a physically sound way. For these reasons, I suggest that the present modelling approach is published in Ocean Science, however, only after a number of revisions.

The major problem of the paper is that it intends to parameterise density advection, but, indeed, horizontal density gradients are parameterised. Once the density gradient is calculated, the advection term is explicitly resolved, since the velocity profile is prognostically calculated. Therefore, the simulations in which the density gradients for the internal pressure gradient force and the salinity advection are different, are inconsistent.

In this context, it is also wrong to discuss the run-away stratification as a numerical artefact. Instead, it is simply a consequence of the physically inconsistent assumption of constant (in time and space) horizontal density gradients.

I do therefore suggest to the authors to rewrite their manuscript in terms of parameterising the horizontal density gradient. As new title, I would suggest something along the lines of "Parameterisation of horizontal density gradients in one-dimensional water column models for estuarine circulation".

As I understand the method, it (maybe in slightly modified form) would even give some physically sound results for a salt wedge estuary, where the surface salinity is zero, and therefore also the horizontal density gradient at the surface, whereas in the lower layer strong salinity fluctuations may occur due to the movement of the salt wedge. Such a situation is shown in a recent paper by Burchard and Hofmeister (2008), see figure 4, where we reproduce the Warner et al. (2005) 2DV estuary test scenario. It may indeed be interesting to try to reproduce such scenarios by means of the 1D model presented in the present manuscript.

Apart from these general comments, I have a number of more specific comments which should also be considered for a revised version:

p. 189, line 4: make sure that the reader understands that water column models are discussed here (and not along-estuary 1D models).

p. 189, line 7: you may also want to mention the older 3D model (in 2D mode) by Burchard and Baumert (1998).

p. 190, lines 16-: tidal mixing asymmetry (Jay and Musiak, 1994) should be mentioned here as well, since it has been shown (Burchard and Baumert, 1998) that this mechanism may have a stronger influence on the velocity profiles than the internal pressure gradient.

p. 190, line 22: the velocity profiles are close to logarithmic, and not parabolic-like.

p. 191, line 22: give value for beta.

p. 192, eq. 3: please add the information that the water depth is constant and Earth rotation is neglected. Please generalise the internal pressure gradient term for vertically variable horizontal density gradients.

p. 192, lines 6-7: the only correct bottom condition for velocity is u=0 (no-slip), and this does not need to be parameterised at all. The quadratic friction is introduced in order to account for the fact that in staggered grid models the lowest velocity point is not located at the very bed. The correct way to calculate the drag coefficient is assuming a logarithmic velocity distribution in bottom grid box, such that the drag coefficient will be a function of the bed roughness length and the grid box thickness. This roughness length must be the same as the one used for calculating the mixing length at the bottom. With this, the quadratic friction is part of the discretisation, and not part of the physics.

p. 192, lines 17-18: here, it is inconsistent to state that the salinity gradient is constant.

p 192, lines 18-20: the method of tuning the barotropic part of the pressure gradient such that the tidally averaged transport is zero looks a bit unhandy to me. It should be described how it is done. What we have done since long time (Burchard, 1999) is to formally calculate at each time step the barotropic part of the pressure gradient such that a prescribed vertical mean velocity is reproduced. With this, any observed

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or idealised tidal velocity cycle can be obtained, including the effect of residual run-off from the river.

p. 193, eq. 7: it should be noted that often a salinity gradient is to be prescribed as a constant, e.g. as estimated from observations (see, e.g., Simpson et al., 2002). Also for some idealised studies, it is sometimes better to use exactly the prescribed salinity gradient, not modified by the method presented here.

p. 193, line 18: The effect of mixing and its tidal asymmetry needs more discussion here (and in figure 3).

p. 199, lines 2-8: I do not see the point why for ROFI applications Earth rotation should be important, but not for estuarine applications. The transverse velocity patterns due to Earth rotation shown by Simpson et al. (2002) could look very similar in an estuary.

p. 199, lines 20-23: please delete the two last sentences of the conclusions. The first of these two sentences is too emotional ("somewhat regrettable"), and the second one is redundant.

References:

Burchard, H., Recalculation of surface slopes as forcing for numerical water column models of tidal flow, Appl. Math. Modelling, 23, 737-755, 1999.

Burchard, H., and H. Baumert, The formation of estuarine turbidity maxima due to density effects in the salt wedge. A hydrodynamic process study, J. Phys. Oceanogr., 28, 309-321, 1998.

Burchard, H., and R. Hofmeister, A dynamic equation for the potential energy anomaly for analysing mixing and stratification in estuaries and coastal seas, Estuarine Coast. Shelf Sci., 77, 679-687, 2008.

Jay, D.A., and J. D. Musiak, Particle trapping in estuarine tidal flows, J. Geophys. Res., 99, 445-461, 1994.

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