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Interactive comment on "Controls on oxygen dynamics in a riverine salt-wedge estuary – a three-dimensional model of the Yarra River estuary, Australia" *by* L. C. Bruce et al.

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Review of "Controls on oxygen dynamics" by Bruce at al.

This paper, which uses an existing 3D hydraulic and water quality model to investigate the oxygen dynamics of a salt wedge estuary, is not very innovative. It is essentially a model application to a case study, after which the authors look for statistical relationships with a number of physical parameters to describe overall performance. They then claim that these equations can be used for water management or for assessing the effects of climate change.

The new insights gained through this analysis are very limited, while the explanations given for the relationships found are sometimes flawed or trivial. In short, I don't find this paper suitable for the readership of HESS. Below I shall give more detailed comments as I go along the different sections of the paper.

1. The description of the estuary is very incomplete and unprofessional. Figure 1 reads like a riddle.

1a. Where is the 0 of the chainage used?

1b. Is this the same point as the downstream boundary of the model?

1c. From reading the figure, it looks as if the chainage is not correct. The distance between 5 km and 10 km appearss substantially longer than between 10 km and 15 km.

1d. It is not at all clear what the range of the model is. Fig. 1 suggests between Spencer Street Bridge and Dights Falls, but these points are not even represented in the detailed map and it is not clear what their chainage is.

1e. The 'gauging stations' Morell Bridge, Scotch College and Bridge Rd don't have a chainage. We have to guess where they are located.

2. The whole paper is about the occurrence of anoxia and hypoxia, but no word about the causes of the OD. A lot of hypothesizing about climate change, but isn't the cause of the anoxia the supply of polluted water from upstream, most probably stemming from diffuse agricultural sources, or maybe from insufficiently treated drainage? There is also no mention whether the OD is BOD or COD. If ever there is a question of management, then one should look at the causes of the anoxia and not at what the effect of a temperature rise would be on OD under a business as usual scenario. If the occurrence of anoxia is natural in these estuaries, then why worry? If the cause is anthropogenic, then address the problem. But in the paper there is not even mention of the potential causes. The four top right pictures in Fig 5 suggest that the OD comes

from upstream, and hence is anthropogenic.

3. I find the description of the estuary very poor. General information on estuary shape or on tidal characteristics is missing or even wrong (see below). No information on cross-sectional areas or width. Normally one would present a plot with the cross-sectional area, the width and the average depth as function of the distance, so we can clearly see the topographical features. Figure 1 gives an impression of the depth, and Fig 6 of the depth in the model domain, but there is nothing about cross-sectional areas or width. Regarding general information, the authors even interpret their data wrongly. They claim that the estuary has an average tidal range of 1.4 m. Clearly it can be seen from Fig. 2 that the tidal range is in the order of 0.5 m. Logically the tidal range is small, otherwise one would never have a salt wedge estuary. A salt wedge estuary only occurs in estuaries with a large river volume in relation to the tidal volume (what the authors call the R/V ratio, but which is the Canter-Cremers number). This number equals QT/AE, where QT is the fresh water volume entering the estuary during a tidal period and AE is the cross-sectional area times the tidal excursion (see Savenije (2012)). There is no information on the tidal excursion nor on the cross-sectional area. This information could have been easily derived from the model. But given that the estuary is rather shallow and the tidal range is small (0.5 m), the Canter-Cremers number is generally much larger than 1, which indicates a salt wedge estuary.

4. The assertion that Australian estuaries are different from Northern Hemisphere estuaries made on page 9818 L1-4 is nonsense, I am sorry to say. The reason lies clearly in the topography and in the Canter-Cremers number. The physics on the Northern and Southern Hemisphere is the same, I am afraid. And of course, maximum stratification occurs during high flow, when the Canter-Cremers number is largest. The anomaly that you observe is due to the sill (the shallow area near 8 km). It is not due to the curvature. The curvature has nothing to do with it. The observation in L15-23 on the same page is wrong. It is the sill and not the curvature. Of course shallow parts are often linked to crossing-over shallow parts between bends, but the curvature in itself

has no, or very limited influence.

5. The distinction between the Zones 1-4 is attributed to curvature. This is also a wrong conclusion. It is related to the sill around 8 km and the shawling at the upstream end. A simple inspection of the model simulations (in Fig.6) can tell you this. You can see that the passing of the salt wedge over the sill causes the anomaly in stratification.

6. I find the whole discussion on goodness of fit completely uninteresting. HESS papers should not be about how to apply or how to fit a model, but on how to interpret and explain observations. A model is just a way to represent our perception of what happens and to test our hypotheses, helping us to explain what we observe. Here the authors just apply an off-the-shelf model and do a poor job at interpreting what the model computes.

7. About the statistical analyses. I don't think that Fig. 8 provides a good fit if we look at the field data. Also Fig. 12 is not convincing.

8. Wrong interpretations:

9812L14: L2 is NOT the 'extremity of the tidal pulse'. What one could potentially consider to be the 'extremity of the tidal pulse' is the 'tidal excursion', which will not be more than about 10 km. Here we are talking about the length of the saline wedge. This length is the result of the balance of forces between on the one hand the density driven gravity gradient and on the other hand the sheer stress exercised on the interphase by the river flow. It is independent on the tide. One could say that this is purely the result of the river flow and the geometry and has nothing to do with the tide. Of course the wedge moves up and down with the tide, but the length of the wedge should be calculated from the ocean boundary where the water is completely saline. In the paper it is not at all clear where this boundary is; definitely further downstream from the 0 chainage in Fig 6.

9812L17: L15 is NOT 'the point where the ocean and river flow meet' or are 'in equal proportion'. The point where they are in equal proportion is the point where R/V=1, which is quite something different from the depth average salinity being 15 psu. By the way, on P9812L15 the L_15 is defined in relation to the 'downstream model boundary', which in my view has been arbitrarily chosen. Hence the L_15 is a completely arbitrary number which can not be attributed any physical meaning.

I find these interpretations very worrying. They show that the authors don't know the basics about estuary hydraulics. I recommend them to read a few basic hand books, such as Fischer et al. (1979), Savenije (2005, 2012) or Wolanski (2005).

9. Detailed comments:

9a. 9802L7 This strong variation in oxygen content is probably due to the tidal movement and has nothing to do with the observation in the previous sentence.

9b. Why is Fig. 5 not in colour. It is hard to distinguish between the grey and the black dots. Also the dots are too small. By the way, the fit for Morell Bridge is not so good. Maybe if you use larger and coloured dots this poor fit becomes clearer.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 9799, 2013.