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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Salt intrusion in the Pungue estuary, Mozambique: effect of sand banks as a natural temporary salt intrusion barrier" *by* S. Graas and H. H. G. Savenije

Anonymous Referee #1

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General Comments: This is a very interesting paper that further develops the theoretical framework of salinity intrusion in alluvial estuaries by Savenije (2005). It is well suited to HESS because it addresses relevant scientific questions related to the salinity intrusion model, thereby extending its adaptability to explain the effects of variation in the alluvial sediments (sandbars which impede the salinity intrusion). It presents an application of the model to the Pungue estuary in Mozambique, showing how water abstractions upstream will affect the viability of future municipal supply from the existing intake. The manuscript is generally well-written and organized, with only minor awkwardnesses in language in some locations. I recommend moderate revisions focusing on two areas.

The first major concern I have is that the authors do not sufficiently emphasize what I see as the major contribution: the specific effect of the sandbars on the salinity intrusion. They do discuss and present in a figure the longitudinal displacement of the salinity intrusion curve in an empirical way, but the paper would be much stronger if this analysis were integrated numerically into the theoretical framework. For example, it would seem that the formation of the sand bars effectively damps the tidal excursion. Since the tidal excursion is also apparently described by an exponential relationship (is this the first time this has been suggested?), perhaps the effect of the sand bars can be accommodated within the theory by a change in the tidal excursion convergence length? Alternatively, using the salt intrusion length equation (eqn7), is there a way of showing that the shift in the salinity curve is related to a reduction in salt intrusion length L through an effective reduction in Qf at the location of the sand bars?

Secondly, the authors present a re-interpretation of some salinity data for the Pungue estuary previously analyzed in Savenije (2005). In my view, there is an adequate basis for this reinterpretation, which results in a different calibrated value for the Van den Burgh coefficient K, but they need to justify this more thoroughly because K is supposed to be constant for a given estuary. In particular, this could be done by evaluating the fit of the observed data to the simulation curve using a common criterion analogous to the R-squared value. Such an analysis would add to the impact of this work because it shows not only another application of the original salinity intrusion model but a major advance in how such a theoretical framework can accommodate even such physical changes in estuarine sediments.

Specific comments: km scale needed on figure 1, also helpful to label city of Dondo and perhaps other locales mentioned

p.2528 L6-7: note that alluvial estuaries change in sediment geometry over time, and the change in the Pungue described later (p.2530) would seem to require some expla-

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nation that Ao may vary

Eqn. 5: is this a new development of the salinity intrusion theory? I don't recall seeing the tidal escursion expressed as an exponential function in Savenije (2005)?

p.2530 L19: a=b=16611m this must be a value derived from the trends in Figure 2 - probably not justified to assume that much precision, maybe ~16600 m is better?

p.2530 L20: The width data is very convincing, and I understand the difficulties of working in the field in Africa, but it would be preferable to have additional data for cross-sectional area A, and depth h between about 20 km and 60 km from the mouth on Figure 2. Even a few points would help. Are the lines fitted to the data, and if so, what are the R2 values?

p.2532 and Figure 4: The K values shown in this paper for the Pungue are 0.1 which is quite a bit different from the K=0.3 values presented by Savenije (2005). I understand that this is because of the different single-branch model used here compared to the two-branch model in Savenije (2005). However, since the data for 12/10/93 and 27/02/02 are the same but just reinterpreted here, it would be better to explain this more thoroughly because K is supposed to be constant for any estuary. For example, is the fit of the simulated salt intrusion presented here quantitatively better using the new interpretation compared to the two-branch analysis for those dates? In that case, the earlier K value was just not accurate?

I see the authors have some explanation of th change in K values later (p.2533 L12-16), but I'd suggest moving it earlier and justifying it in terms of a better fit between the field data and the simulated curves in Fig 4

p.2533 L1-3: This is a very interesting point and one of the most important parts of the paper. Instead of just "moving" the salinity curve horizontally to account for this empirically, could this be integrated in a more numerical way in the theoretical framework? For example, it would seem that the formation of the sand bars effectively damps

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the tidal excursion. Since the tidal excursion is also described by an exponential relationship (is this the first time this has been demonstrated?), perhaps the effect of the sand bars can be accommodated within the theory by a change in the tidal excursion convergence length? Alternatively, using the salt intrusion length equation (eqn7), is there a way of showing that the shift in the salinity curve is related to a reduction in salt intrusion length L through an effective reduction in Qf at the location of the sand bars?

p.2533 L16: is the topography different (simpler) now, or was the original analysis of the geometry of the estuary not as accurate as this analysis?

p.2533 L21: "different monthly discharges" or different exceedance times for monthly discharge? I assume the salt intrusion length curves for Fig 5 are calculated for high water slack using Do at HWS, since eqn7 depends on eqn8, and this should be specified. It would also be worth mentioning that if the tidal range is lower than 6.5 m, the salt water intrusion would also be affected by the sand bars. Also specify what "natural situation" is. I assume it's measured monthly discharge augmented by the known diversions for irrigation and municipal supply. Also make horizontal black line thicker showing distance from mouth of the water intake - it's not very obvious.

p.2534 L21: I think that if the effect of the sand bars on the salt intrusion can be integrated into the theoretical framework as I suggest above, then this is a substantial advance, and should be more prominently summarized in the Conclusions and Abstract. In particular, this shows the robustness of the theory, its adaptability to explain the effects of such variations in the alluvial sediments, and its application to a real-world need to foresee the effects of future water development and usage.

Technical corrections/comments:

p.2524 L8: rewrite: "in the dry season, the water intake often has to be shut off because ..."

p.2524 L11: "compared to" rather than "confronted with"?

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p.2526 L20: "on" not "in" the order of

after "tidal average depth" add "h" p.2527 L15

modify p.2527 L22 to read: "(see Savenije, 2005, p.155-156):"

p.2528, L1: Van den Burgh's?

p.2529 L7: semi-empirical

p.2530 L15: 75% of the cross-sections "are" dry

p.2531 L4: all measurements "were" not "where"

p.2532 L3: where is the location of station Bue Maria (E66)? Can it be shown on Figure 2? What about the tributaries mentioned?

p.2532 L9: mention of Figure 5 before Figure 4. Maybe omit mention of Figure 5, just say "as presented below"

p.2532 L27: rewrite: "In between spring tides, there resulted an accumulation of fresh water"

p.2533 L4: "...salt water intrusion was not measured..."

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