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Hydrology and Earth System Sciences Discussions

Interactive comment on "Comparison of empirical models with intensively observed data for prediction salt intrusion in the Sumjin River estuary, Korea" *by* D. C. Shaha and Y.-K. Cho

Anonymous Referee #2

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Review of Shaha and Cho

This paper compares five empirical models of the salinity intrusion in the Sunjin River Estuary and determine that of Nguyen and Savenije (2006) to be best, giving reasonable results for even stratified neap tide conditions. The goal of using data to make a comparative study of simple models of the salinity intrusion is worthwhile. However the paper is poorly structured, the methods are not clearly laid out in a section prior to the results section, and the arguments are unconvincing. Much work needs to be done before this paper is worthy of publication.

The comparison presented in Figure 5 is not a fair comparison since there is less scat-

ter in the data compared with the Van den Burgh, Savenije and Nguyen and Savenije models than the other three. The comparisons should be made based on the same data set for all model. Also the argument that they are better because of use of an exponentially tapered channel in the model has not been convincingly supported, and the argument that they are preferable due to having more parameters is not a good one, especially as the goal of the paper is to test simple models.

The authors mention 3 possible factors influencing the salinity intrusion; river discharge, tides, and bathymetry. They should first distinguish between the mean salinity intrusion and its variability since the factors influencing these two aspects of the intrusion may differ. Secondly, they should explain the mechanisms by which discharge, tides and bathymetry determine the mean salinity intrusion and its variability. Third, in discussing the relative influence of discharge and tides, they should be clear that it is the influence of variability in discharge and tides on variability of the intrusion which is being compared – which explains why bathymetry is not discussed in this comparison though elsewhere (example?) the authors state that all three are being compared. The influence of bathymetry is partly through modifying the influence of variability in runoff and tides, and this should be made clear. Finally, it should be clarified that since the influence of variability in runoff is assumed not to interact nonlinearly with the influence of variability in tides, the timescales of the assumed influence varies drawing into question whether the response can be considered as being in steady state for both subtidal and supratidal timescales (are all the models assumed to apply in a steady state? Is this clarified in the text?).

The authors state that the exponential-geometry based models are preferable. What is the justification for this? In any case, it is not proven that this is so and should not therefore be taken as a measure of a preferable model.

The authors state that models with more parameters are preferable – this is true only up to a very limited point (many tuned parameters is not a good thing), and generally simplicity is desirable (part of the authors' stated motivation for comparing empirical

models).

It would be useful for the reader to be familiarized with the theoretical relation, including common theoretical background/foundation, between these models and why they differ.

In the following the page number is indicated followed by line numbers for that page followed by comments. Abstract 9: remove “those of”

Page 3 1: replace “the” with “a semi-enclosed” – see classic definition of Cameron and Pritchard (1963) "An estuary is a semi-enclosed coastal body of water which has a free connection with the open sea and within which sea water is measurably diluted with fresh water derived from land drainage”. Without this added feature, the author’s stated definition would include many shelf seas and river-plume regions which are not estuaries. 5: “outflow” (one word) not “out flow” 8: winds may also play a role – see Scully, M.E., C.T. Friedrichs, and J.M. Brubaker, 2005. Control of estuarine stratification and mixing by wind-induced straining of the estuarine density field. Estuaries, 28: 321-326. 17: insert “or” before “extraction” 18: replace the word “instrument” with something like “model” or “method” – “instrument” implies a measuring device 23-24 “Based on . . . explain the” – this doesn’t clarify anything; need more detail here since I was already about to object to the unclear meaning of “material analysis” – is this the author’s choice of wording? If so it could be improved.

Page 4 2: replace “with” with “using” 3: replace “by” with “due to”; remove “a river’s” since a river may be monitored in several contributing tributaries 4: replace “balanced equation” with something like “advective-diffusive equation” if that is what you mean. “budget” is also acceptable but not as informative. 11-12: “on a real environment” is awkward wording and not very clear. 12: replace “der” with “den” (Van den Burgh) 13: replace “works” with something like “analysis” 15: replace “works considering real environments” with something like “studies in real estuaries” 16: not clear what “prototype measurements” means 16: replace “use” with “uses” 17: replace “measure” with something like “model” 18: replace “;” with “,” and remove the comma following

“whereas” 18: “reported the geometrical character” is unclear with respect to both the word “reported” and the word “character” 19-20: replace “based on” with something like “for application to” 21: replace “based on” with “for”

Page 5 1: remove the semicolon before “(i)” 2: insert “of” following “years” 3-5: move “in the Sumjin River Estuary” to be immediately after “2007” in line 3. Also, only data for 2005 and 2006 are shown and it is not made clear how or whether the other data are used or whether there is only one sample per season per year. 6-7: replace “in the stratified Sumjin River Estuary” with “stratified neap tide conditions” 7: it is usual to have, at the end of the introduction, a paragraph including text such as “In the following section, we . . .. Subsequently, in section 3, we . . .”, etc. This is a useful tool in that it clarifies the content of the paper (and approach to fulfilling the goals of the paper which should be clearly stated previously in the introduction) for the reader, assisting with full comprehension in reading the paper. 10: replace “falls into” with something like “enters”. If the portion to the east of what is labeled as “Gwangyang Bay” in Figure 1 is also considered part of the Bay, this should be made clear, or can the whole thing be called the Gwangyang Bay Estuary since the bay itself is undoubtedly influence by fresh water and is semi-enclosed? 10-13: “falls into . . . river mouth.” – clarify that though there are two entry points, the one chosen is more important (why?) and is considered the “mouth” and discuss the implications that since this mouth is within the bay, the salinity at the mouth is under significant influence from the river and not fixed at shelf-water values; this is important in flushing time estimation as done later in the paper. 11: “main stream” is unclear – be more specific 14: “downstream of the estuary” is unclear especially in light of the lack of clarity as to what constitutes the “estuary” vs. the “river” (there is undoubtedly some overlap) 15: move “surveyed in 1994” to the end of the sentence 15: replace with “was collected from” with something like “is provided by” 17: replace “this river” with “the Sumjin River” 18 replace “seaward (station 1) to a landward location (station 25)” with something like “at the locations of CTD stations 1-25, proceeding up-estuary from the mouth,” 19: the convergence lengths are quoted with unrealistically high accuracy – uncertainties on these geometric parameters would

make this quite clear 23: insert “reaches” following “upstream” and it would be helpful to insert something like “(above the tidal excursion)” following “of the river” if the gauges are indeed situated above the influence of the tide which is the preferable situation for measuring river discharge – typically an amplification factor is applied to the measured flow at the gauge to account for input downstream of the gauge.

Page 6 1: “on the day of” – one sample a day? One day a season? Make the data sampling frequency explicit. Refer to Figure 3 or give dates here. 8: insert “criterion” following “range” 17-19: add some clarification/explanation in support of this last sentence

Page 7 2: restate the goal of comparing these models, and why do this. Also need to motivate the use of these empirical models to some degree – are they based to some extent on some simplified physical models such as the classic advective/diffusive model? Is there some parameterization of the influence of tides relative to buoyancy inputs. At several points in the paper it is stated that comparisons between the relative importance of tides, runoff and geometry is being made. It is mentioned in the introduction, where winds have been ignored without comment, but the only explicit goal of the paper laid out in the introduction is the comparison of the empirical models and the test whether the Nguyen and Savenije model is applicable to stratified conditions. If there is a focus in this paper on comparing the importance of tides and runoff, this should be clarified at the outset and in the methods section (here entitled “Empirical Models Description – the authors should consider changing the section title and including the methods of the paper for using/comparing these models rather than simply describing them in this section). In this respect, the authors should clarify the reason for and approach to comparing spring/neap (partially-mixed vs. stratified) conditions with respect to comparing model performance. Of particular importance related to this is that the models should all be compared using the same data set, so if spring and neap data sets are being compared they should be compared for all models – Figure 5 gives the impression that the models are being compared for different data sets (not clear which

are spring and which are neap) which is not a fair basis for comparison. Also if the comparison is being made only on the basis of the R2 values shown in Figure 5, this should be made clear in a methods section. 4: the sentence “The most important output of these models is the salt intrusion length.” is stated twice in the paper without justification – it is not strictly true; at least the sentence should be rephrased to make it sound less absolute. 7: why “at the bottom” instead of depth-averaged? 7: use salinity units (psu) since when the authors later write “Salinity 1” this is unusual and not clear. 8: replace “;” with “,” 10: replace “;” with “,” 11: insert “is” following “f”; insert “is” following “Fd” 12: insert “is” following “F”; insert “is” following “N” 13: insert “is” following “a”; insert “is” following “Qf”; insert “is” following “D0” 13-14: define “dispersion coefficient” or at least give a reference 14: insert “is” following “K”; remove “elaborately”

Page 8 7: insert “is” following “H”; insert “is” following “C” 8: insert “is” following “d”; insert “is” following “NR” 12: replace “provided” with “provides” – all the rest of this paragraph is past tense 15: remove “Moreover” 16: insert “different”, “modified”, “slightly modified” or whatever’s appropriate before “formula” 17: replace “like” with “similar” or whatever’s appropriate. It’s important to detail here (not just in the table) the differences between these models (as well as the physical motivation for these models) since the choice of and comparison between these models is the centerpiece of this paper – or rather it seems the centerpiece is to show that the more recent Nguyen and Savenije (2006) model is an improvement over some prior empirical models; it would be better in this light to rephrase the goal of the paper so that it doesn’t seem like a comparison of a set of 6 models chosen for reasons which have not been made clear, but rather a test of the Savenije and Nguyen and Savenije models compared to some prior models, one of which (Van den Burgh) is a direct precursor to Savenije’s. Be explicit about the historical and conceptual relation between the models chosen. 19: The phrase “prototype measurements in estuaries”, used previously, is unusual and unclear. 19-20: replace “the exponential function for” with something like “an exponential model of the” 20: insert “, Eqs (6) and (7),” following “geometry” and replace “for predicting” with “to predict” 20-21: delete “and Eqs (6) and (7) as exponential functions for longitudinal

variations of the cross-sectional area and width of estuaries."

Page 9 1-3: rewrite this sentence in light of the suggestions previously made for this section (which again should be more generally a "methods" section instead of simply a description of the models chosen for comparison – which isn't even completely carried out in this section, as noted above. 8-16: some of this first paragraph was already stated – anything detailing the data-processing or methods rather than introducing the results should be put in either data or methods sections. 11: it is not clear what is meant by "all landward stations" partially because this definition has not been made sufficiently clear in the data section. 13: rephrase "measurement criteria of salinity 1" and use units, writing "salinity = 1 psu", to assist in clarity. 13-15: this sentence was previously stated, and should only appear in data and/or methods sections; no need to reiterate in the results section. 16. The method outlined in this last section has not been mentioned previously, sounds suspect, and should be carefully motivated in data and/or methods sections. 18-20: This first sentence is vague and unclear – where is the abrupt variation in bottom topography? Does this influence the exponential geometry model's error estimates (not give in the paper but they should be)? What is the evidence of the complex vertical salinity distribution? Does this mean the estuary is not partially- or well-mixed? What is the bearing of this on the salinity intrusion as measured by bottom values? If you had chosen to measure the intrusion as a depth-averaged value, as is often done, how would the "complicated" vertical distribution change things – or is this the motivation for using bottom values? It is not clear how this is related to the second sentence (20-21). 20: "was oscillated by tide" is awkward and unusual wording. 22: delete "than that during the spring-tide" 23: how is the "stratification parameter" determined?

Page 10 1: The estuary is either partially-mixed or well-mixed, not both at the same time. 2: looks like there is an erroneous paragraph-break following "tide" 3-4: need to specify the temporal distribution of measurements – partially the problem is that the temporal distribution was not made adequately clear in the data section, and if the

methods involve using subsets of the data, this should be made explicit (what exact temporal subset of that data are used) in a methods section. 4: insert ", respectively" following "4" 6: replace "appeared" with something like "occurred" 6: "during summer" of both years? Or all years 2004-2007? Again the temporal distribution of the data and subsets related to the methods has not been made adequately clear. 7: "were usually" – were there exceptions to this usual pattern? If so, what were they? 13: rewrite this first sentence – too confusion; use the word "respectively" when describing characteristics of two things, respectively. 13: insert "km" following "4" 15: rewrite "might increase" 18: insert "intrusion" following "autumn"; what about winds? 19-20: were runoff and tide conditions significantly changed? 20: replace "a little bit high" with "slightly increased" 22: "42% less" – both summer and autumn? 23: "summer 2006" – but not autumn? 23: replace ";" with "," but don't really need the commas around "therefore" in this context

Page 11 3-6: these last two sentences seem disconnected from the rest of the paragraph and pointless in relation to the goals of the study. If there is a point in discussing the tidal excursion here, make this clear instead of leaving it hanging at the end of the sub-section. 5: as mentioned previously, I feel that for clarity it would be preferable to write "salinity = 27 psu", or at least "salinity = 27" (if you object to the use of the units "psu" – "practical salinity units", adopted to replace "parts per thousand" and in common usage), than "salinity 27" 9: For sub-section 4.2, since the methods had not been adequately and clearly laid out in a preceding methods section, the results are difficult to evaluate and appreciate. It may be useful to briefly restate the methods here but they should have been clearly and completely detailed in a previous section. If they had been, the inappropriateness of comparing models based on different data sets, as is apparently done on the basis of the results shown in Figure 5, would be more readily identified. 10: Again, I object to the statement made in this first sentence, which has been stated previously with the exact same wording - perhaps the authors could indicate the usefulness of the empirical models in evaluating the salinity intrusion without implying that this is the only important "output" of these models. Certainly investigating

the relation between the physical motivation for the model parameterization and the salinity intrusion variability is as important as predicting the intrusion length itself. 16-17: the method of adding the tidal excursion to low-water slack measurements needs to be clearly detailed and justified in a preceding methods section. 17: replace ";" with "," and remove the comma following "however" 21: if the comparisons are solely on the basis of the R2 values shown in Figure 5, then this should be clearly stated in a preceding methods section and perhaps reiterated here. Again, Figure 5 seems to imply that the models are being compared on the basis of R2 values for linear regression to different data sets which have different amounts of scatter and may not even be equally linear – this is not a fair basis for comparison. For example, how can it be determined based on the R2 values for the center-left plot in Figure 5 (these plots should be labeled a-f for ease of referencing) that the Fischer model doesn't work well when there is clearly no linear trend in the data? And similarly, it seems obvious that the Savenije and Nguyen and Savenije models would be determined to work best since they are applied to the data set with the least scatter about a strongly linear trend. It appears that the authors need to improve their basis for model-model comparison, and if other factors than the R2 values in Figure 5 are being considered, this should be made explicit both in a methods section and reiterated here. 24: don't understand "but varied greatly with the observed lengths"

Page 12 2: what does "accounted for" mean here? 2: "Moreover" implies a relation to the preceding sentence – I don't see the relation. 3: "overestimated" – can this be seen in Figure 5 as a bias? I don't see it 6: replace ";" with "," 7: which "two models"? 7-8: the second half of the sentence is a separate idea, not directly connected with the first part. Don't see what is the point of the second part here anyway. The evaluation of the relative importance of tides and runoff, and the influence of the (assumed exponentially tapered) geometry in relation to the effects of variability in tides and runoff on variability in the intrusion, should be discussed separately from presenting the results shown in Figure 5. 10: this first sentence seems incomplete and uninformative – state in this first sentence how the mean discharge was used. Similarly, in the second

sentence, it is not at all clear what "considering all . . ." means – how are these considered and for what? These methods, i.e., how the runoff and tidal variability is being used, needs to be detailed previously in a methods section. 13: the only apparent method of comparison in looking at Figure 5 and reading the results discussion so far is the R2 value, which as mentioned previously is apparently being compared on an unfair basis. Therefore the reference to Figure 5 here, in relation to "considering" runoff and tides, is confusing. Please make all this very clear. 14: "capable" is acceptable but not the best choice of words, however the phrase "capable of computing" really sounds awkward. I would replace the word "computing" with "predicting" and I would leave out the word "highly" since I don't think enough evidence has been presented to make this determination. 15: this is really the first mention of the motivation of using an exponential channel taper and it has not been established why the choice to use an exponential taper implies a better model, and if it is because the other models were not formulated for exponentially-tapered channels, this should all be made explicit in an earlier section. 15: more parameters does not automatically make a model preferable – it can be an improvement in going from 1 to 2 parameters but not in going form 2 to 100 parameters; part of the motivation for using empirical models was the simplicity. 16-17: "satisfactory . . . compared to" – what does this mean? Satisfactory with respect to what? 17: "results" not "result" 18-19: what does it mean here that the intrusion depended on "morphological characteristics". It's not even really clear what it means here "strongly depended on the river flow". It is important to distinguish the mean conditions that determine the mean salinity intrusion from the factors that affect its variability. 23: "freshwater . . . ways" is too general for placement here; better for introduction section.

Page 13 1 Seems like some of the material in the previous sub-section, which I commented on above, would be better placed in this sub-section (4.3). 6: replace "function for" with something like "determinant of the" 6: the intrusion length and its variability may have different explanations 8-9: Hansen and Rattray do not give a power law relation for the salinity intrusion. 11: "previous research" – give examples of this research and maybe also some results 12: "different estuarine conditions" – what conditions

determine the difference? 14: it would best if you could estimate an uncertainty associated with the -1/5 power dependence determined for the Sumjin River Estuary 17: "prove" is too strong a word here. Try "determine whether" rather than "prove that" 18: delete "or both" 19: "regression fitting" – the approach should be detailed in a preceding methods section so the reader can know how it is determined that runoff (variability?) is the controlling factor. 21-23: this last sentence seems disconnected (not in logical order) with the preceding part of the paragraph. The mechanisms by which tide and river discharge (and bathymetry) influence (determine) the salinity intrusion and its variability, and any relevant historical references such as Uncles and Stephens (1996) should be laid out in the introduction, as well as an indication of how the paper will approach distinguishing which controls the intrusion.

Page 14 2-3: it was not clear from the preceding results section that this first statement was shown. In any case, remind the reader here what evidence this statement is based on. 5-6: this is the first mention of processes specifically at the "toe". However, mixing throughout the estuary determines the intrusion as a cumulative effect. 6: replace "arrives at" with something like "approaches" 8: "gravitational flux" – define this; gravitational circulation as in Hansen and Rattray is a larger-scale process 12: derive, explain, or give a reference for equation 13 – equations 14 and 15 can be used for this and in that case should precede equation 13 15: insert "is" following "R"

Page 15 1: "strongly depends on the river discharge" – the theory of Hansen and Rattray (see Monismith et al., 2002; also see Lerczak, J.A., Geyer, W.R. and Ralston, D.K., In press. The Temporal Response of the Length of a Partially-Stratified Estuary to Changes in River Flow and Tidal Amplitude. Journal of Physical Oceanography) give a 1/3-power runoff dependence of the salinity intrusion, which I wouldn't characterize as "strongly" dependent 1-2: is this sentence true for very low runoff? In that case it becomes tidally-dominated unless there are no significant tides 3: rewrite the phrase "there should have" 4: rewrite the phrase "excess amounts of the intercept value" 6-8: no need to restate the methods used here – I don't see the point of saying here

that the salinity of the adjacent sea is required, and anyway, given that the "adjacent sea" (at the "estuary mouth") is actually in the bay and therefore likely to be under significant direct influence of runoff variability rather than a fixed reference value as desired, this should all be discussed in a previous section such as the introduction or methods section. 8-10: rewrite this whole sentence 13: rewrite the phrase "dominated on" 13: rewrite the phrase "whereas the segment B was a combination of" – note that for the seaward internal segment the flushing time equation needs to be revised since there is significant exchange of salt at both boundaries – see Gay, P. & O'donnell, J., 2008, Comparison of the salinity structure of the Chesapeake Bay, the Delaware Bay and Long Island Sound using a linearly tapered advection-dispersion model. Estuaries and Coasts 14: insert a comma following Fint and delete "the" following "for" 15: rewrite "implies the strong" 16-17: the argument in this sentence is unclear. 18: replace ";" with "," – however this sentence does not convey any meaning to me 19-21: this sentence is a weak argument based on the preceding 23-24: this sentence should be part of the details of the empirical models presented in a preceding methods section, and it should be made clear in that section why this modification is expected to be an improvement – i.e., what was the motivation for the modification?

Page 16 2: replace the word "shows" with something else 2: the estuary can be either well-mixed or partially-mixed but not both simultaneously 3-4: why does it work better in spring than neap, and is part of this due to less scatter (more linearity) in the data itself rather than a characteristic of the model performance? In figures 5-7, the subplots should be labeled (a), (b), etc. and the figure captions should give details relevant to separate subplots using these labels. More detail is needed in the caption for Figure 5. 5: "also" compared with what, and why? 8: "reasonable prediction" – why? How is this demonstrated in the paper? 13: rewrite the phrase "the estuary demonstrated" 13: should be either partially- or well-mixed, not both 20: "a good indicator for determining the gravitational flux near the toe of the salt intrusion" – what does this mean? Anyway, this was not demonstrated convincingly – see preceding comments for page 14

Page 17 4-5: rewrite “might be the probable cause for the high capability” – again I object to saying that the model is an improvement simply because it has exponential geometry (this was not adequately demonstrated) and because it has more parameters (this is not in general true and contradictory with the spirit of the paper to use simple models) 4-10: these last two sentences really don’t have anything to do with the goals of the current paper even though they constitute planned future work of the authors. 17: replace “supports” with “support”

In table 2, define the terms here as well as in the text. In figures 3 and 4, use “salinity = 1 psu” or “salinity = 1” instead of “salinity 1” in both the figure captions and on the figures themselves.

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