

ATM Response to Comments, Questions, and Concerns of the Technical Approach to Vertical Mixing in the Savannah Hydrodynamics and Salinity Model

Part II

1. Response to questions and comments raised by the Federal Agency reviewers

The comments from each of the reviewers are answered in the following order below.

- a. USACOE – Dr. Sung-Chan Kim
- b. USGS – Paul Conrads
- c. USEPA – James Greenfield
- d. GAEPD – Paul Lamar , Roy Burke III
- e. SCDHEC – Wade Cantrell
- f. SKIO – Dr. Jack Blanton

The parties listed above will be referred to as reviewers in the following text, and ATM and other modelers will be referred to as researchers. For clarity, each comment will be repeated verbatim and followed directly by the response from ATM. In addition, to make discussion and referral more direct, comments that were not numbered by the reviewer have been numbered here.

Review on ATM’s approach to “vertical mixing”

Sung-Chan Kim, USACE-ERDC, Vicksburg, Mississippi

Comment #1: ATM took an empirical approach to represent “vertical mixing” in **BFHYDRO** despite there are many other similar applications based on more rigorous physics of turbulence. An empirical approach is only considered when there exist enough data to show the tight relationships over broad range of conditions.

ATM response: An overview of the response to this comment was addressed in the discussion in the opening section, (Part I) of the document. In addition, although some similar applications exist, few exhibit the specific dynamics shown in the Savannah River estuary. In addition, many researchers have had difficulties in getting the sensitive response necessary for the simulation of the stratification and de-stratification process seen in the estuary. Examples include the ongoing Chesapeake Bay work (USACOE, HydroQual) and the work performed on the Columbia River (Baptista).

It is clear that there is a distinct relationship between the tide range and the stratification – destratification sequence (as acceded to in Comment #2 by the reviewer below) and that the relationship was demonstrated graphically and analytically, over six months of observations, in two years, as reported in Appendix Q of the model calibration report. We showed in the 1997 report entitled “Analysis of the Historical Data for the Lower Savannah River Estuary” that salinity is a function of three primary mechanisms, i.e. tide range, flow, and mean water level. Our method accounts for the impacts of tide range through our vertical mixing formulation, we account for flows through the Richardson

Number type scaling, and we account for the mean water level through the pressure term in the model.

Comment #2: ATM noticed the close relationships between tidal ranges and stratification-destratification sequences (**Figures 2a, 2b, 2c**). The state of stratification is determined by the difference between surface and bottom salinities. From **Figure 2b**, one may see the clear relationships between tide range at the entrance and the stratification at GPA-04. **Figure 2c** shows more salt intrusion at GPA-08 during neap. But it is not clear the stratification (which is indicated by the difference between surface and bottom salinities) is related to neap-spring cycle. With the time scales of the figures, it is difficult to judge whether the second peak in August has higher surface salinity than bottom one.

ATM response: The researchers agree that there is a close relationship between the tide range and the stratification – destratification process. This relationship was shown clearly at both GPA-04 and GPA-08 on repeated occasions in the observations from the two monitoring studies.

The data during the second peak in August noted by the reviewer was unfortunately misleading data which was clearly missing much of the signal (see Figure 1 below). The rising curve and peak values appear to have been truncated during the period between about 8/9/97 and 8/16/97. The bottom salinity during the second peak in August would almost certainly have been higher than the surface as it is in every other case during the 1997 and 1999 monitoring periods.

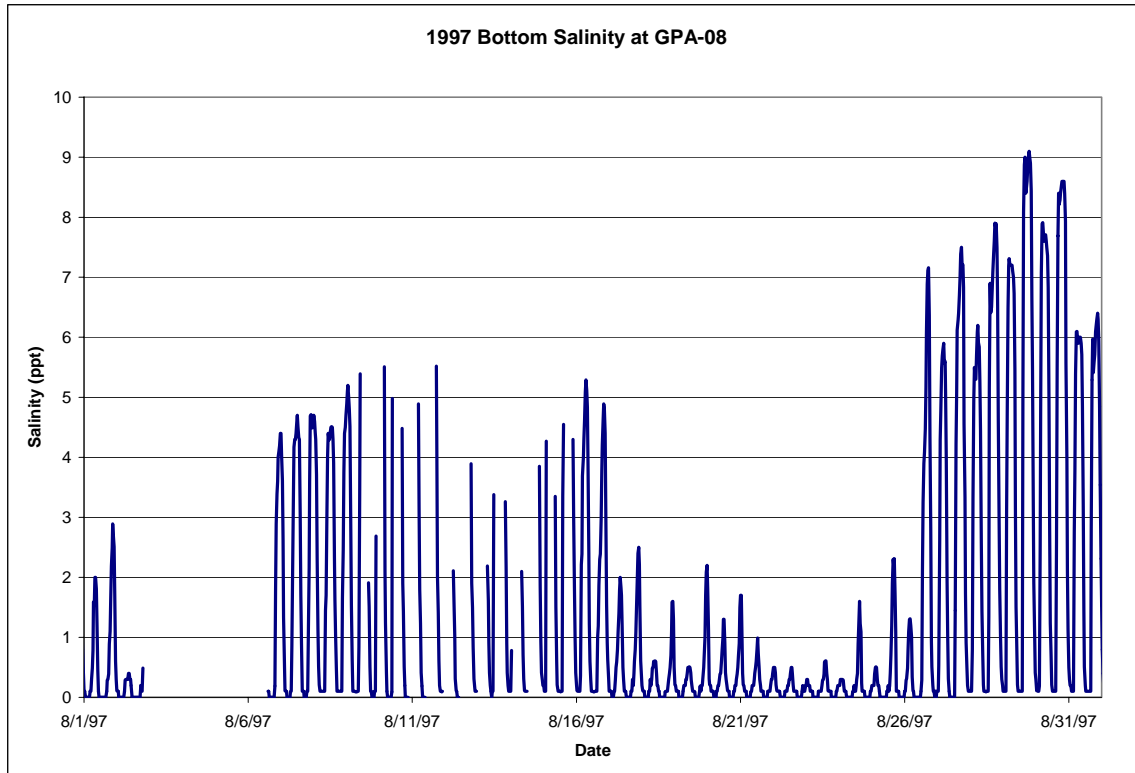


Figure 1 1997 bottom salinity at GPA-08 displaying missing signal between 8/9/97 and 8/16/97.

Comment #3: Then, ATM presented the relationships between Richardson number (Ri), which is an indicator of stratification, and tidal range (H) (**Figures 9, 10, 11, 12**). In this analysis, tidal ranges were smoothed (but the method such as filtering frequency was not specified). I am not sure about comparing two quantities with different time scales ($H \sim$ day; $Ri \sim$ hours?). The correlation coefficients, r^2 , were 0.63 for GPA-04 and 0.33 for GPA-08. I can only see weak relationships. In addition, all the Richardson numbers were above 1 at GPA-04 for the tidal range below 2.6 m (**Figure 9**). In **section 3.2**, it was stated that ‘mixing’ may be ‘negligible’ if $Ri > 0.25$. This means there is almost no mixing if $Ri > 1$. Then, is it really meaningful to take any calculation if $Ri > 1$?

ATM response: The analysis described by the reviewer was developed at the reviewers request in order to examine the proposed relationship between the tide range and vertical mixing. The figures depict the relationship between the Ri number and the tide range and the Ri number and the river flow. As the researchers postulated from an earlier analysis, there is a distinct trend in the relationship between the Ri number and the tide range but no discernable counterpart for the flow.

The reviewer is correct in suggesting that the comparison would have been more appropriately performed either on raw data for both the independent and dependent variables or on filtered data for both. The curve fit coefficients would also almost certainly benefit from either of those alternatives. The values termed gradient Ri number

are actually defined by the difference between the surface and bottom observed values and therefore relate to the entire water column. As such, the $Ri > 0.25$ transition from turbulent to laminar flow indicator is most likely not as strict as implied in Appendix Q.

Comment #4: Equations (3) and (4) are relating local (meaning at a certain depth in a water column) viscosity/diffusivity to the neutral viscosity/diffusivity via Ri (also locally determined). **Figures 7 and 8** show Ri has broad range at a time. However, in **Figure 20**, Ri is not the local value anymore. Each time it has only one value. **ATM** termed this as gradient Richardson number and it seems it is a bulk Richardson number. Then it is not justified to use **Equations (3) and (4)**. But in **Figure 22**, a form of **Equation (4)** was shown with $a = 0.1$ and $n = -2$. It is a puzzling adoption of **Equations (3) and (4)**. D_{v0} in **Figure 22** is a bulk value whereas the one in **Equation (3)** is a local value (neutral diffusivity).

ATM response: Figures 7 and 8 actually have unique values of Ri for each time step, but are simply plotted as points at $\frac{1}{2}$ hour intervals rather than connected as a continuous curve. The continuous curve presented in Figure 20 is from an older analysis and simply has the points connected. In both cases the gradient was determined from available data, therefore over the water column (salinity data available only near the surface and the bottom), but none the less in gradient form. The presentation in figures 21 and 22 are also from the earlier analysis and were developed to demonstrate the clear trend in the relationship between the tide range and the vertical mixing. Also in Figure 22 equation (4) was shown with a positive exponent (n) and the negative included in the value of n , shown as $n = -2$. The equation should have read $D_v = D_0 (1 + a Ri)^{-n}$ as in equation (4), with an exponent of $n=2$.

Comment #5: Also in **Figure 21**, it was stated that the filtered Ri was used. But, it is not clear whether it followed the tidal range estimation method (by applying moving averages according to the spreadsheet **ATM** supplied separately from the report). In many modeling applications, **Equations (3) and (4)** modify the vertical structure depending on the state of stratification. But, **ATM** developed its own method (**Equation 6**) which redistributes diffusivity throughout the water column and warrants zero diffusivity at $d\rho/dz = d\rho/dz_{max}$. This will effectively separate bottom water from surface water (or vice versa). This is very simplified view on the vertical distribution of diffusivity.

ATM response: For Figure 21 the filtering of the Ri number was done in a manner similar to that done for the tide range, by applying moving averages. The researchers agree with the reviewer that the relationships given in equations (3) and (4) are often used in closure models as a method to correct the vertical eddy terms for the influence of buoyancy. The form given in equation (6) is similar to equations (3) and (4) as it also corrects the vertical mixing for the influence of buoyancy through a scaling based on local stratification and a parabolic distribution and accomplishes the same end. **ATM** agrees that the method does provide a simplified view of the vertical distribution of

diffusivity but based upon the performance the degree of vertical exchange appears reasonable.

Comment #6: In **Figure 20**, $Ri > 1$ for GPA-04. Only a few times GPA-08 shows Ri below 1. In **Figure 21**, minimum for Ri is taken as 1. In **Figure 21**, GPA-04's minimum Ri is about 4. Does this suggest it is always stratified? But there is spring-neap cycle for stratification (see for example, **Figure 2a**). How come we don't see any small Ri which would be the case for spring tide? In **Figure 22**, $\alpha=0.1$. That's because the Ri used in this analysis is one order higher than it is supposed to be. All the studies (**Table 1**) show it is order of 1 (for example, Officer's coefficients are $\alpha = 1$ and $m = 2$). Then **Equation (5)** was determined with $r^2 = 0.46$. It is another judgment call to accept whether this is a good statistics. The analysis seems incomplete considering the ranges of Ri and artificially very small value of α . There is also another issue of the sensitivity to the tidal range estimation. According to **Equation (5)**, D_v is very sensitive to H (proportional to the 10th power of H). Depending on the smoothing scheme, D_v can vary a lot. In the spreadsheet **ATM** provided in addition to the report, 3 repetitive moving averages were applied for elevation data. Moving average is a kind of low-pass filter but is not exactly the way to estimate tidal range. **ATM** should specify the definition of tidal range used in the analyses.

ATM response: The analysis of the observations to determine the Ri number for the plots was performed to evaluate the relationship between the tide range and vertical mixing. The plots shown were from the original report and show the beginning of the determination of the coefficients for the log law model and the process by which the values were tied back to the physical observations. There is a clear trend in the relationship presented in the older analysis as well as the evaluations discussed in **Comment #3** above. For the evaluations we have performed and for all of the model runs, the input for use in the vertical mixing scheme is defined as the sub-tidal component of the tide range. What we were pursuing as the overall objective was the relationship between stratification and salinity intrusion and tide range with stratification seen in the observations.

Comment #7: The above analysis draws question about the tightness of the relationships. There is another question of the range of environmental conditions of the empirical relationship. The background hydrology during the 1997 and 1999 only covers lower half of hydrological conditions of the Savannah River. According to 68-year statistics of the USGS stream gage at Clyn, 50 percentile of the flow is 12700 cfs (20 percentile is 8034 cfs and 80 percentile is 22000 cfs). This means 1997 period (with 5600 – 13000 cfs) is relatively low flow conditions (below ~ 50 percentile). 1999 was even drier (between 250 cms (=8829 cfs) and 150 cms (5297 cfs)). It will be difficult to extend the relationships over the higher flow range because the formulation is based on the assumption of no impact of flow to diffusion.

ATM response: Since the completion of the calibration the researchers have completed several additional simulations for extended periods covering the entire calendar years of

1997 and 1999. The 1997 calendar year in particular covers a number of higher flow periods with a maximum flow of 20600 cfs and is a period where the flow was above the 50th percentile flow of 8860 cfs most of the time. Although there is not as much data available as during the summer period field programs, the USGS maintains 4 continuous gages at several points in the estuary that were used for statistical comparison of salinity. The results of the comparison are presented in the table below. In addition several other periods were run, including a 3 month period during 1992, which was prior to the deepening to the present depth of the Front River shipping channel.

For continuity of this section, the results of the 1992, 1997 and 1999 simulations are presented at the end of this report. The results clearly show that the model is capable of not only of simulating higher flow periods but is also capable of predicting changes due to deepening (or shoaling) of the channel.

Comment #8: When reviewers found the diffusivity estimation from the spreadsheet different from the report, it was natural to become suspicious of the scheme itself. The spreadsheet showed that the estimation is composite from two sets of coefficients in **Equation (5)**, which is not mentioned in the report. In addition, there was offset adjustment, which is also not mentioned in the report. This is disturbing. Also during the model execution, I noticed the eddy viscosity has no vertical variation while diffusivity has vertical variation. According to **Equation (7)**, eddy viscosity is linear function of eddy diffusivity through Prandtl number. Unless the screen output is something else, it is difficult to accept that eddy viscosity has no vertical structure whereas diffusivity has.

ATM response: The essential reason for the analysis presented in Appendix Q of the final report was to demonstrate the relationship developed between the tide range and the vertical mixing in terms of the physics of the system. The general form and the development of the functional relationship was presented in some detail in the appendix, including much information that was originally presented in a paper delivered in 1999. As with many technical aspects of the model system, the specific numerical implementation was not discussed, in favor of the overall theory of the formulation. When requested, the specific numerical formulation was presented in a timely fashion. The formulation as presented in the appendix and as calculated in the spreadsheet is identical to the original formulation developed in 1997, including the curves, coefficients and filtering. The researchers regret and apologize for any confusion or uncertainty this approach may have caused. The failure to mention the offset was an unintentional and unfortunate omission to the final report.

Comment #9: One may argue that there is nothing wrong about an empirical approach. But, an empirical approach limits oneself to the quality and quantity of data over which the relationship is determined. There has been great advancement in our understanding of turbulence especially during the past decade. There are many models using physics based turbulence model. One example is the GOTM (general ocean turbulence model) initiated by many international researchers. There are many publications and tests one can find at GOTM web site (<http://www.gotm.net>). This raises the question about the defensibility of

the model. The rational is jumpy (see for example, using local diffusivity relationships of **Equations 3 and 4** to draw the bulk relationships of **Equation 5**). While **BFHYDRO** was calibrated for 1999 and verified for 1997, the coefficients in the mixing scheme were obtained from 1997 data set and verified with 1999 data set. Having two sets of coefficients makes me suspect that the empirical relationships were based on both 1997 and 1999 data, which confines the use of the coefficients to these two specific sampling periods. The approach is qualitative at best.

ATM response: The researchers hope that the question regarding the limitations of the approach based on the calibrations period has been somewhat answered by the additional time periods simulated. Rather, the scheme was based on a clear relationship, worked extremely well, and passed the tests forwarded by the reviewers as well as the researchers. However, if it is the recommendation of the reviewers at this point that the project can not proceed with the present mixing scheme and that the researchers investigate the adoption of an alternative mixing scheme we would be amenable to that suggestion.

The coefficients chosen were developed for the 1997 data set and never changed. They were not based on both years worth of data. One of the striking features of the scheme is that the relationship developed for the 1997 data set actually worked on the 1999 time period without modification! That very test was a clear indication that the scheme, empirical though it may be, is more robust than it might initially be considered.

Comment #10: ATM tried justifying the mixing scheme by hindcast study. **Figure 2-57** shows the most change would occur at stations GPA-21, GPA-6, and GPA-22 (about 3 ppt change). **Table 2-16** shows the model prediction at Port Wentworth is 1.1 ppt and the observation was between 0.55 and 0.67. This will give error of 0.55 ppt and 0.43 ppt. Should we think this is good because the difference is less than 1 ppt? The relative error is between 100 and 64 percent. Then, should we conclude the model is bad? ATM didn't prove anything for hindcast. I don't see the rational behind the hindcast study. It is inconclusive.

ATM response: The development of the hindcasting was initiated in order to provide some test of the vertical mixing scheme under a deepening. The thought was that we did a multivariate regression analysis back in 1997 on salinity data before and after the 1993 deepening project. This multivariate regression analysis related salinity at the available USGS stations (Houlihan Bridge, Lucknow Canal, and USF&W dock) to tide range, flows, and mean water level. The analyses showed as discussed previously that the highest correlations existed to the tide range and relationships relating salinity to tide range, flows and mean water level were developed (from the data before the deepening and after the deepening). Using these relationships, with the same input forcing, gave an estimate of the salinity conditions at Houlihan Bridge (GPA-09) before and after the deepening. We then calculated the relative change at that station. While not perfect and certainly containing errors, this estimate was the best we had at the time.

ATM would certainly have liked to have data during this last deepening at stations where a greater response would be expected, i.e. further down in the harbor but the reality was that the Houlihan Bridge Station (GPA-09) was the most downstream salinity station on the Front River and the changes determined at the Little Back River stations (Lucknow Canal and USF&W Dock) were barely at measureable limits.

ATM acknowledges the reviewers concern but this was the best example we had to try and demonstrate the models response. It was probably not the best presentation and inaccurate to show the results from the data analysis down to 2 significant figures but the feeling was that this did provide a somewhat quantitative test of the model response under a deepening.

Subsequently we have used the model to simulate salinity over a 3 month period prior to the last deepening (June 1 to September 1, 1992) and compared it directly with the available data. We also ran the model on another 12-month period (9 months outside of the calibration/validation time frames) post-1993 deepening (January 1 to December 31, 1997), as well as for the entire 1999 calendar year. The statistics for all are presented in the tables at the end of this document. The model was clearly able to perform well over the various different bathymetric, flow and tidal regimes. The statistics indicate that performance was comparable to the performance experienced for the calibration data set.

Comment #11: In summary, I am uncomfortable with the ATM's approach in vertical mixing. I see the approach rudimentary and limited. In addition, the procedures are not consistent. Vertical mixing was calibrated with 1997 data and verified with 1999 data whereas **BFHYDRO** was calibrated with 1999 data and verified with 1997 data. The methodology has to be clearly stated with all the assumption and justification of the choice of the coefficients. There also has to be the statement of the limit of the applicability of the scheme to hydrological conditions. Should this approach be replaced by more acceptable physics based turbulence model (such as Mellor-Yamada's model or k- ϵ model), it would be more defensible.

ATM response: While ATM believes that the present formulation is sufficient for use in the Harbor, we acknowledge some of the reviewers concerns and questions and feel that the vertical mixing relationship could be recast in terms more acceptable to the reviewer while maintaining the "spirit" of the approach, i.e. a simple method that works and develops the vertical mixing variations on a subtidal time scale.

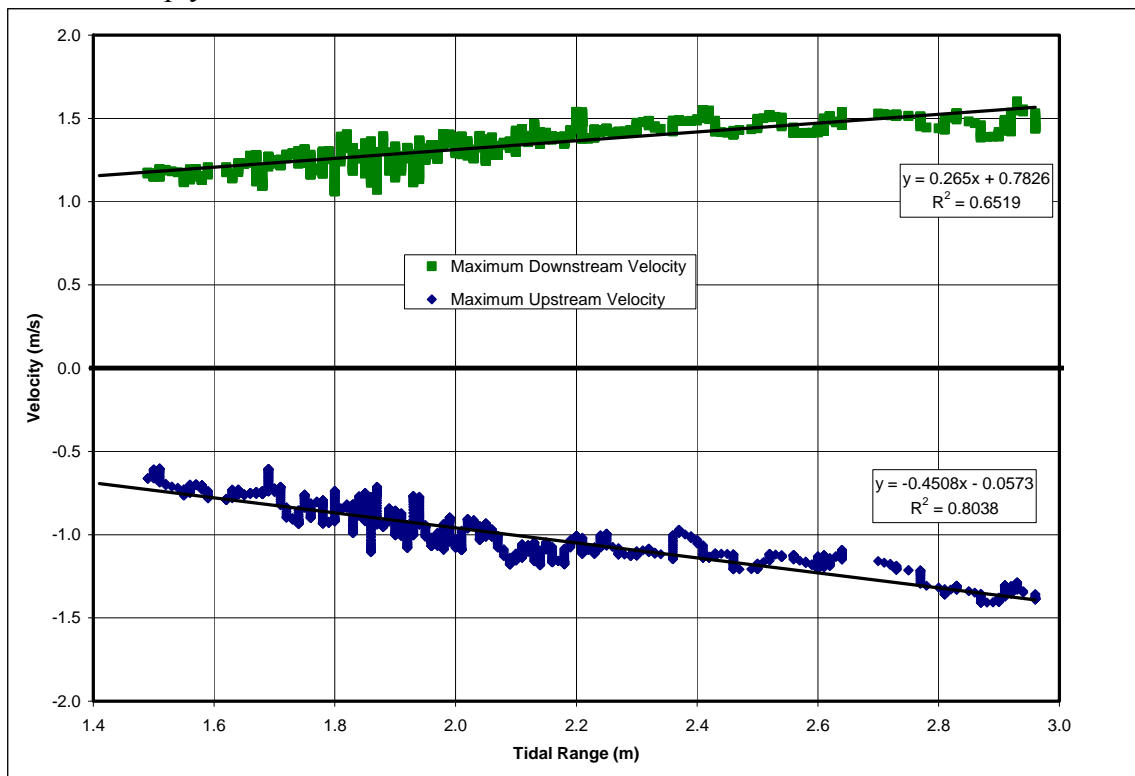
Comments, questions, and concerns of technical approach to vertical mixing in WQMAP-ATM

Paul Conrads-USGS

Comment #1: The vertical mixing formulation is not based on the proper physics of the system. Turbulence is largely a function of bottom friction. A scheme using local friction velocity would likely work much better than one based on tide range at the downstream boundary.

ATM response: The calculation of vertical mixing in the model is actually based on the physics of the system in the following manner. As stated by the reviewer, much of the turbulent mixing is due to the velocity and the interaction with the river bottom in terms of shear. In the case of the present mixing scheme, the tide range is used as a proxy for the water velocity as there is a direct relationship between the velocity and tide range through the slope of the water surface elevation in the system.

The example figure shows the min and max bottom velocities as a function of the tide range. The relationship is clear, and a regression line has been added to indicate the goodness of fit. In addition, the relationship between velocity, bottom friction and vertical mixing is always parameterized, has followed many different forms in the literature and requires empirical coefficients for application. The scheme used in the model is simply a different form.



Given the relationship between bottom velocity and tide range, it is possible that our methodology could be easily cast in terms of bottom velocity (or shear velocity) rather than tide range without any real any loss of predictive performance. We would want to hold on to our approach however, that focuses on the subtidal issues relative to vertical mixing and that it is the longer time scale results we want to capture to keep the method simple but defensible.

Comment #2: Formulation should also include local depth of flow because the depth greatly affect turbulent eddies.

ATM response: Subsequent to the initial development and application of the mixing scheme, attempts were made to include a depth term in the analysis and to evaluate scaling of the mixing based on depth. These variations did little to enhance the predictive properties of the model at the time and were discarded. As with the use of the velocity relationship discussed in response to Comment 1, the incorporation of a length scale could also be done relatively easily without significantly altering the results or the general characteristics

In the light of the two previous comments, a test of the model’s predictive properties was made to better evaluate the effects of deepening on the salinity predictions and the influence that the velocity and depth invariant vertical mixing might have on those calculations. In order to do this analysis, three test cases were run on a test grid with similar dimensions to the Savannah River for the 1999 time period with an identical setup to the 1999 calibration. The first case was essentially for a 12m channel depth. The second case was run for a deepened channel; to a 14m depth. The results of the change in the velocity field (approximately 8% change) between the 12m and 14m cases was used to adjust the vertical mixing curve, resulting in an 8% decrease in the mixing value. The third case was a re-run of the 14m channel case with the decreased vertical mixing. The results are presented in the following tables.

Deepening test case for evaluation of velocity scaling on vertical mixing and predicted salinity

Velocity (m/s)		Upper Estuary	Mid Estuary	Lower Estuary	Avg
Depth H=12m	Avg =	0.18	0.18	0.22	0.19
	Max =	0.40	0.51	0.67	0.53
Depth H=14m	Avg =	0.17	0.16	0.19	0.18
	Max =	0.38	0.48	0.58	0.48
Difference	Avg =	-0.01	-0.01	-0.03	-0.02
	Max =	-0.02	-0.03	-0.09	-0.05
% Difference	Avg =	-5%	-8%	-11%	-8%
	Max =	-5%	-6%	-13%	-8%

Salinity (ppt)		Upper Estuary		Mid Estuary		Lower Estuary	
		Bottom	Surface	Bottom	Surface	Bottom	Surface
Depth H=12	Avg =	1.2	0.5	17.8	8.2	26.0	16.0
	Max =	6.1	3.7	23.9	14.0	33.5	22.9
Depth H=14	Avg =	2.5	0.9	22.7	9.8	28.5	16.9
	Max =	8.5	5.1	28.5	15.0	34.6	23.5
Depth H=14 with velocity scaled Dv**	Avg =	2.5	0.9	23.0	9.5	28.7	16.5
	Max =	8.7	5.0	29.1	14.7	34.8	23.3
Difference from deepening (H14-H12)	Avg =	1.30	0.39	4.83	1.57	2.56	0.93
	Max =	2.36	1.40	4.65	1.06	1.17	0.67
Difference from deepening (H14-H12) with velocity scaled Dv**	Avg =	1.38	0.36	5.18	1.28	2.79	0.50
	Max =	2.56	1.30	5.18	0.75	1.30	0.38
% Difference in salinity between scaled and unscaled deepening cases	Avg =	1%	-2%	1%	-3%	1%	-2%
	Max =	2%	-1%	2%	-2%	0%	-1%
% Difference in salinity difference between scaled and unscaled deepening cases	Avg =	6%	-7%	7%	-19%	9%	-46%
	Max =	8%	-7%	11%	-29%	11%	-43%

**Note: Dv was scaled linearly with the average velocity difference of 8%

In summary, the tables above indicate that the percent difference in the salinity predictions are relatively small, hovering around 1%-3% of the total signal. The percent differences in the predicted delta salinity however can be as high as 46% in the lower estuary where the velocities are generally higher (in the test grid anyway) but generally remain around 10% of the predicted value, or in other words, about 90% of the predicted variation is due to factors other than the vertical mixing (e.g. increases in the baroclinic pressure terms).

Comment #3: The vertical mixing formulation assumes that the vertical mixing coefficients are the same throughout the model domain. Local channel morphology plays no role in the mixing scheme. The mixing coefficients are derived from the vertical velocity profiles at Fort Jackson (GPA 04). It is difficult to imagine that the physics at GPA 04, downstream of the confluence of the Back and Front Rivers and upstream of the

divergence between the Savannah River and South Channel, is representative of the physics (vertical mixing) of the model domain in the area of concern.

ATM response: The assessment by the reviewer is correct that the initial mixing scheme predicted vertical mixing is initially constant throughout the system, but variable in time. Some spatial variability enters through the Ri number type scaling of the buoyancy influence. The coefficients were developed from analysis of the physics of stratification and destratification observed at GPA-04 but based on the overall relationship between the tide range and velocity in the system which is significantly more universal (see figure in the response to Comment #1).

Comment #4: There are better methods to bring in the buoyancy effects than Equation 6 (Appendix Q), which prorates the vertical diffusivity based on the spatial gradient of salinity. Why not adopt a Munk and Anderson type model given in equation 4 and use the model predicted velocities and depths of flows?

ATM response: The researchers agree with the reviewer that Ri number relationships are often used in closure models as a method to correct the vertical eddy terms for the influence of buoyancy. The form given in equation (6) (Appendix Q), is a similar type of equation as it also corrects the vertical mixing for the influence of buoyancy through a scaling based on local stratification and a parabolic distribution often used in mixing length theory for open channel flow and accomplishes the same end. Since there is already a relationship of this type in the model it should not be too difficult to implement a Munk and Anderson form of the equation.

Comment #5: The correlation between tidal range and Richardson for GPA 04 and GPA 08 are 0.63 and 0.33, respectively. The assumption is that freshwater inflow is unimportant to vertical mixing. The correlation coefficients imply that tidal range explains two-thirds of the variability at GPA 04 and only one-third of the variability at GPA 08 for the flow conditions. Does the difference in coefficients indicate that there are differences in the physics between the two locations? Other hydrodynamic factors explain the two-thirds of the variability at GPA 08 not explained by tidal range. Could streamflow account for some of the unexplained variability?

ATM response: This comment was addressed substantially in the response to Dr. Kim's Comment # 3. As discussed above, the comparison between the Ri number and the tide range would have been more appropriately performed either on raw data for both the independent and dependent variables or on filtered data for both. Examination of the data at GPA-08 and also GPA-09 clearly show a relationship between salinity intrusion and the spring-neap cycle or tidal range, ATM does not believe that the poor correlation coefficient is indicative of a lower correlation at this location. The differences in the correlation coefficients are probably more a function of mixed time scales than of a lack of correlation to the salinity intrusion (a function of stratification) and tide range.

Comment #6: It isn't clear why the velocity gradient values were smoothed using a 3-hour moving average and the salinity values were not smoothed in the Richardson number (Ri) calculation. In Figures 9 and 10, what tidal range was associated with the 3-hour average Ri calculation?

ATM response: For consistency the salinity values should also be smoothed at the same frequency as the velocities although little change is seen in the results. In general, the velocity signal from the ADCP was quite a bit noisier than the salinity signal. The analysis we were trying to perform was focused on illuminating general trends and magnitudes in the observations that could potentially be hidden in the noise. Because the Ri number is based on gradients, it can be more sensitive to noise than other parameters. The smoothing on the velocities was centered on the tide range.

Comment #7: For Figures 7-12, $Ri > 10$ were set to 10. In Figure 20 many values are greater than 10 for several tidal cycles - especially at GPA 08. What would the relation between tidal cycle and Ri and streamflow and Ri look like if all the Ri had been plotted (Figures 7-12) on a log scale as in Figure 20? Would this have changed the correlation coefficient and the assumption that tidal range is the only significant factor in vertical mixing in Savannah?

ATM response: For Ri numbers greater than a critical value of 10 there is effectively no mixing (Blumberg, 1986) and therefore little additional information is garnered by including those values in the development of a relationship. The form would probably have been different but less meaningful in terms of the real relationship between tide range and mixing. Tide range is certainly not the only factor but, referring to the comparison of the smoothed Ri number and the smoothed tide range it is clear that the tide range is a dominant factor and that no such direct relationship for river flow was observed. The buoyancy influence is also a factor as described above in the response to Comment #4.

Comment #8: What was the rationale for the selection of Officer's coefficients for application of equation 4? Were the other coefficients (Munk-Anderson or Bowden-Hamilton) tried or fitting the Savannah data to the function?

ATM response: The set of coefficients attributed to Officer were selected from review of his comparison of observations to the relationship for estuarine conditions (Mersey River) and their simplicity. The variations between the results developed using the various different coefficients is not large however yielding an overall effect of the Ri number scaling that is relatively consistent.

Comment #9: Appendix Q did not adequately describe the computation of the Dv time series. Equation 5, as it is presented in the report, is not used in the computation. An

additional constant was added to the application of Equation 5 that was not described in Appendix Q. Figure 22 shows the curve fit of Equation 5, as reported in Appendix Q, but it does not show the final form of the equation used for the application of the model.

ATM response: The researchers apologize for the concerns caused by the omission of the details of the vertical mixing scheme numerical implementation in the final report if this is considered incomplete. This was done under the general terms of the format of the remainder of the final report in that none of the details of the numerical implementation of any of the model terms was presented. The emphasis in Appendix Q was intended to explore the validity of the relationship in general and to answer specific questions regarding the physical nature of the relationship. When the specific coefficients were requested by the reviewers, the numerical implementation spreadsheet, detailing the entire calculation method was forwarded. A summary of the details of the numerical implementation follows for clarity.

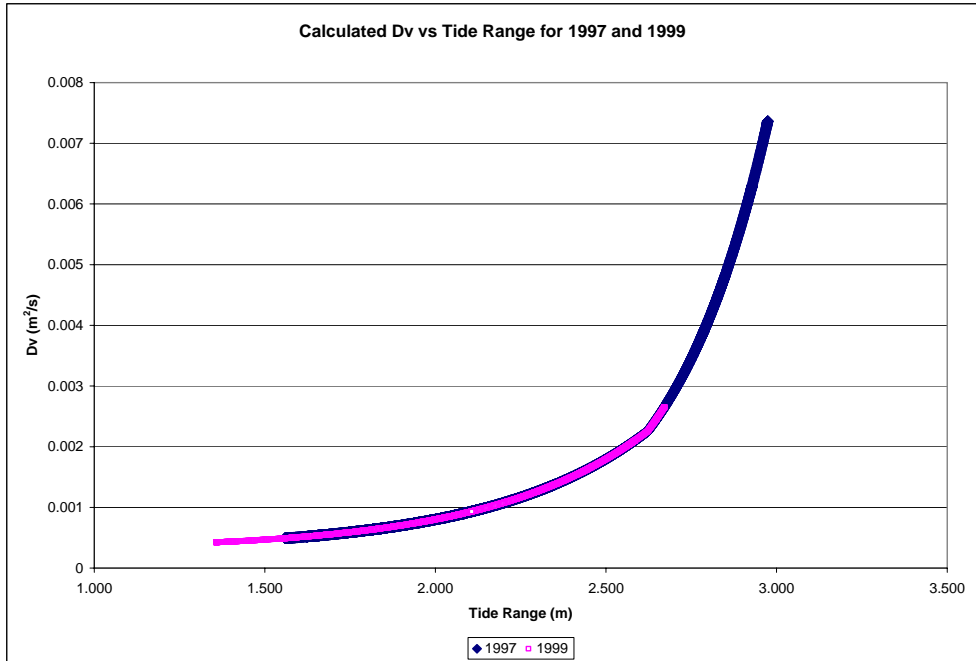
$$Dv = \begin{cases} 10^{(\alpha_2 \Phi + \beta_2) + \gamma_2}, & Dv_1 > Dv_2 \\ 10^{(\alpha_1 \Phi + \beta_1) + \gamma_1}, & Dv_2 > Dv_1 \end{cases}$$

where

$$\begin{aligned} \alpha_1 &= 0.95 \\ \beta_1 &= 5.2 \\ \gamma_1 &= 0.0003 \\ \alpha_2 &= 1.5 \\ \beta_2 &= 6.6 \\ \gamma_2 &= 0.0001 \end{aligned}$$

This final numerical form of the relationship given above was developed for the original hydrodynamic model calibration report delivered in January 1998. The implementation has not been altered since that time as can be seen by reviewing the Dv curve in the final hydrodynamic model calibration report (Jan 2004) and the curve published in the ECM6 conference proceedings in 1999.

The original log law implementation given in the equations above were without alteration to both the 1997 and the 1999 time periods for the final calibration study. A plot relating the calculated Dv to the measured tide range for the two years is shown in the figure below.



It can be seen from the overlay of the two periods that all of the information necessary to calculate the 1999 Dv results was available in the 1997 data set and were indeed used to calculate a relationship that was applicable to, unchanged, 1999 data set.

To reiterate the explanation of the constant offset presented in the general response to the comments, it should be noted for clarity, that the use of the 0.0001 add on, which was done for both the 1997 and 1999 simulations, was done just prior to the submittal of the final report as it provided a slight increase in overall statistics. If this were a zero order model using a constant coefficient, this would be akin to making a slight adjustment to the vertical diffusivity constant. So long as this is done for both years equally, it is within standard modeling practice. ATM is willing to remove the offsets from both years and present the model results without it for both 1997 and 1999 if this would be more acceptable to the reviewers, it does not significantly affect the final results.

Comment #10: Appendix Q does not explain that the Dv was computed using two unique sets of coefficients with the additional constants and that the higher values of the two computations were used for the final value. What is the effect of the additional constant?

ATM response: The first part of this comment was addressed in the response to Comment # 9 above. The purpose of the additional constants (outside of the exponent term) on the relationship is to offset the entire curve, the predominant effect being exerted at the lower end of the mixing range. In some sense it is similar to the Dv-min of other turbulence relationships in that they are intended to limit the minimum Dv attained. It is possible that another set of the primary, log law constants could be found that would diminish or erase the need for the added constants, but the performance of the scheme as

it stands was sufficient that the focus was placed elsewhere. The constants were both developed for the original 1997 data set and used to tune the model response to that data.

Comment #11: Along with the additional constant added to Equation 5, as described in comment 9 and 10, an additional 0.0001 was added to the final time series for both the 1997 and 1999 time series. This constant is not described in the report or appendix.

ATM response: The noted constant should clearly have been discussed in the final report but was omitted through oversight. As we noted above the use of the 0.0001 add on equated to a very small change in the overall performance of the model and was done just prior to the submittal of the final report to provide a slight increase in overall statistics. ATM is willing to remove the offsets from both years and present the model results without it for both 1997 and 1999 if this would be more acceptable to the reviewers, it does not significantly affect the final results.

Comment #12: The Dv time series is described as effectively being an additional boundary condition. As a boundary condition, the computation of the Dv time series needs to be clearly described. If a smooth time series is desired for the input to the model, this needs to be stated. The order in which the time series is smoothed makes a significant difference in the Dv “boundary condition” and the resulting salinity predictions. Smoothing the final time series of Dv (using the same three iterations of a 36-hour running average used for the smoothing of the tidal range) made a significant difference in the Dv time series, especially the first two high mixing periods in 1999. Salinity simulations using the new Dv time series show differences in salinity predictions of approximately 5 to 10 percent at selected stations. Was the sensitivity of the computation of Dv to smoothing order and the running averages evaluated?

ATM response: The relationship was developed relating Dv to tide range. If the calculated Dv time series rather than the tide range is smoothed then the relationship between Dv and range will be altered. The researchers have suggested smoothing the range time series only but have not suggested smoothing the Dv series at any point.

In the interest of clarifying the definition of the tide range to be used, ATM will investigate the use of a standard filtering technique in place of the progressive smoothing used at present. By adoption of a better defined filtering method, the training of new users for the development of the Dv curve may be facilitated.

Comment #13: It appears that the application of Equation 5 is highly calibrated to the four or five stratification/de-stratification cycles in 1997 and 1999 data sets. For the two data sets, there are only seven tidal periods where the computed Dv values are greater than 0.015 m²/s and only three tidal periods are greater than 0.030 m²/s. The 1997 and 1999 data sets cover only a limited set of tidal and streamflow conditions. Before the vertical mixing scheme could be used to a major modification to the system, the

robustness of the scheme needs to be demonstrated on a wider range of hydrologic and geometry conditions.

ATM response: The application of Equation 5 was developed and calibrated for the 1997 data set only and has not been altered since. No attempt was made to modify the implementation for the 1999 data set.

The same mixing scheme has also been applied to a series of additional time periods in response to these comments. The dates and range of conditions tested include simulations for 1992 (pre-deepening), the full year of 1997 (additional flow conditions) and fall 1999 (drought conditions). The results are presented in terms of salinity comparisons at the USGS continuous stations at Port Wentworth, USF&W Dock and Lucknow Canal, as plots and statistics located at the end of this report to maintain continuity in this section. In addition, a statistical analysis of the range of flows experienced over the model simulations is compared to the statistics for observed flows between 1955 and 2001. Similarly, statistical comparison of the percent occurrence of the variations in tide range at Fort Pulaski was made for the simulated periods and observations between 1935 and 2000. The flow and tide range plots and statistics tables follow the salinity comparison plots and tables. The results clearly show that the model is capable of not only of simulating higher flow periods but is also capable of predicting changes due to deepening (or shoaling) of the channel.

Also see the response to Comment #7 from Dr. Kim.

Comment #14: The hind cast analysis presented in Appendix Q was inadequate. The original multivariate analysis (ATM 1996) was described as a “qualitative analysis” and not a quantitative analysis. The relative salinity impacts average over some time period does not address the skill of the mixing routine in capturing the salinity intrusion dynamics in the system prior to the last deepening. Why wasn’t a quantitative analysis performed using the 1993 geometry, flow, and tidal conditions and show time series of performance at the USGS sites?

ATM response: ATM agrees with the reviewers notion of the 1996 work as somewhat qualitative analysis (see detailed discussion in response to Dr. Kim’s comment #10) and agrees that a better idea would be to show different bathymetric simulations, with observations available for comparison, directly. To illustrate this, we have done simulations under the 1992 pre-deepening conditions and the statistics are presented at the end of the document.

Comment #15: The application of BFHYDRO to the Savannah Harbor appears to be a unique application of the model with the empirical mixing scheme. BFHYDRO, developed by ASA, has been peer reviewed in engineering and scientific journals and been applied to many systems around the world. Has BFHYDO-ATM, with the log-law vertical mixing scheme, been applied to other systems and has the mixing routine been peer reviewed?

ATM response: The mixing routine was presented in a paper which was peer reviewed for publication in the proceedings of the 6th Estuarine and Coastal Modeling Specialty Conference. A copy of that paper has been included with the delivery of this document. The BFHYDRO model using the log law for mixing has not yet been applied to other systems.

Comments and Concerns on Vertical Mixing

James Greenfield - USEPA R4

Note Earl Hayter, EPA ORD will get his written comments to me by Monday. I will then forward them on the USACE for distribution.

Comment #1: As I understand the vertical mixing scheme applied in WQMAP GPA is an empirical scheme based on smoothed tidal range that only can be used under the calibration summer time conditions of 1997 and 1999 conditions. Since the scheme is not physics based it has no potential for being applied under other conditions. Also the details describing the mixing scheme in the final report were incomplete and misleading. To clear up these errors and misunderstandings the following issues must be addressed.

ATM response: ATM disagrees with the assertion that the scheme as it stands can only be used for the summer 1997 and 1999 periods. We have now applied it to numerous other periods with varying hydrology and tidal conditions without any variation in the tide range/vertical mixing relationship as presented in response to Paul Conrads' comment #13 and Dr. Kim's comment #7, and maintained similar levels of accuracy. See results presented at the end of this document.

Comment #2: Final report does not explain how the vertical mixing actually works. The final report finally gave us some details but not the actual equations or calculation methods. These methods should be written up and provided in detail.

ATM response: ATM agrees that the final report did not explain all of the details associated with the calculation of the vertical mixing well and apologizes for that omission. The spreadsheet provided to the reviewers during the review period did contain the details of the calculations and a more complete explanation is provided in the response to Paul Conrads' comment #9.

Comment #3: The final report alludes to other vertical mixing formulations were tried but no details on what formulations were evaluated and what were the results of the evaluations. Is the Savannah Harbor hydrodynamics so complicated that "normal" mixing schemes are not applicable. If so why?

ATM response: The formulations that were evaluated include both 1-equation and 2-equation turbulence closure models (Muin PhD, 1993). Although both closure models performed rather well for the barotropic simulations, recreating the tidal harmonics and velocity profiles through the domain, capturing the range of variation in the stratification-destratification process remained a problem. The 1-equation model tended to over mix in

most cases and was not well suited for stratified flow. The 2-equation model tended to over mix in unstratified conditions and under mix in stratified conditions.

There are numerous ways in which models address vertical mixing. There are zero order models such as a constant and Richardson Number formulations, there are first order empirical relationships and there are the second-order closure models. All of these types of schemes appear in the literature and are being used as standard practice and could therefore be termed “normal” mixing schemes. It can also be said however, that not all researchers find the turbulence closure schemes to be the best approach. Closure models have some of their own problems and this is documented in the literature, i.e. while they are good, they have some problems of their own and are not the only option

In a paper entitled “Eulerian-Lagrangian” modeling of 3D baroclinic circulation in a river-dominated estuary and plume” Y. Zhang and A.M. Baptista present a model of the Columbia River. In this application they tested various turbulence closure schemes (Mellor-Yamada, K-epsilon and others) and found that they couldn’t get them to work to the level desired for the system, they write:

“Mixing in Columbia River is a very complex phenomena: wind, spring-neap transition, heat exchange with atmosphere, and river discharge all play some role in this. We are yet to find an optimal choice in this regard. The results presented here were generated using a simple zero-equation closure due to Pacanowski and Philander (1981)”

Comment #4: The empirical formulation is based on tidal range as a surrogate for tidal energy, why was not the tidal prism used which is a better indicator of tidal energy?

ATM response: In earlier comment response, ATM showed the significant correlation between bottom currents (which through shear velocities are well tied to turbulence) and tide range. We also acknowledge that we could recast our formulation to currents which might prove more defensible to the review group. This would be similar to tying to the tidal prism which is also a direct function of currents.

Comment #5: The hindcast method provided in the final report was not a true hindcast. See Dr. Kim’s comments. Why was the model not set up for 1993 with 1993 flows and boundary conditions and the model results compared to 1993 data?

ATM response: See our responses to Dr. Kims comment #10 and Paul Conrads comment #14. We have subsequently done simulations with comparison of the model simulations to the 1992 USGS data (1992 was chosen as it was the year we had used for the pre-deepening in the 1997 data analysis report). The statistics for these runs are presented at the end of this document.

Comment #6: Has external expert review been conducted on the vertical mixing equations in WQMAP GPA? If so please provide.

ATM response: The basic formulation and relationship between tide range and vertical diffusivity was presented in a peer reviewed paper at ECM 6 in 1998. The paper was peer reviewed and accepted. The comments of the reviewers are not typically made available to the authors in the ECM review process. The paper has been included with the delivery of this document.

Comment #7: Has the WQMAP GPA code been applied to other estuaries? If so please provide details.

ATM response: The BFHYDO model within WQMAP has been applied to multiple estuaries. The two changes to the BFHYDRO code that are unique to this application are the vertical mixing formulation and the use of the 1-D component above GPA-17. These two aspects are unique to the Savannah Estuary application.

Comment #8: For the upper end of the harbor, upstream flow impacts the amount of salinity intrusion. How does the backwater calculation for the riverine section impact the timing and velocities of the upstream flows as they are translated down to the upper end of the harbor? No details in the final report were provide on how and if the backwater flow calculation was or is appropriate method for simulating the riverine portion of the model.

ATM response: The upstream calculations are not river model type backwater calculations. The model solves the exact same equations for the upstream as for the estuarine portion of the river. The difference is that the river bed elevation between the area at approximately the mouth of Abercorn Creek and Clyo, begins to increase. In order to handle that in the model, an initialization period is used to ramp in the initial river floe and adjust the sparse matrix coefficients to account for the relatively high upstream water level. In addition, to maintain stability in the upstream portion of the model, the vertical deviations from the vertically averaged velocity, are ignored (i.e. 2-D equation solution). The vertically averaged velocity is however still calculated by the same conservation of water mass and momentum equations as everywhere else in the domain.

Comment on Vertical Mixing in the ATM Savannah Harbor Model

Paul Lamarre and Roy Burke III - Georgia Environmental Protection Division

Comment #1: To successfully address important water quality issues in Savannah Harbor, with respect to channel deepening and TMDLs, the adopted model must be capable of representing ‘vertical mixing’ accurately and defensibly.

ATM response: The mixing scheme calculated vertical eddy diffusivity values are well within literature range for the given conditions. The basis of the model development is on the observed physics of the stratification-destratification in the estuary as well as on the predictive performance of the model system. We have attempted to address all of the concerns raised regarding the mixing scheme as they have arisen in the forgoing comments. Please refer to the comments and responses to the USACOE, USGS and EPA.

Comment #2: Recently, serious questions and concerns have been raised about the suitability of the empirical vertical mixing scheme included in the ATM Savannah Harbor model and the manner in which it is used during computations. These are valid questions and concerns that need to be addressed and resolved.

ATM response: The researchers believe that the majority of the questions and concerns referred to here have been addressed in the preceding responses to comments from the USACOE, USGS and EPA.

Comment #3: To date, the adequacy of this empirical scheme has not been fully demonstrated. The Division therefore has an unacceptable level of discomfort with this approach considering the importance of its intended purposes.

ATM response: The researchers hope that the foregoing discussion has clarified many of the misunderstandings and concerns regarding the mixing scheme and that these points have added to the demonstration of the adequacy of the scheme.

Comments on Vertical Mixing Approach in the Savannah Harbor WQMAP Model

Wade Cantrell, - S.C. Department of Health and Environmental Control

Comment #1: It is our understanding that BFHYDRO has an extensive record of application. However, the vertical mixing component in the model code was modified for application to Savannah Harbor. The model code contains a disclaimer by the original developer, which states the developer accepts no responsibility for contents, use, or results of the modified code. This, by itself, does not make the Savannah application unacceptable, but it invites extra scrutiny of the vertical mixing approach.

ATM response: The disclaimer at the heading of the code is to address the fact that ASA has released the code to a third party developer (ATM) and is therefore no longer in control of that version of the code. The use of a disclaimer for the release of technical, intellectual property is common, if not ubiquitous, in the modeling world.

Comment #2: As a participant in the review process, we are aware that reviewers from the federal agencies have concerns about the vertical mixing approach regarding its ability to give meaningful predictions of the impact of the proposed deepening. To our knowledge, these concerns have not been resolved. Until they are resolved, we consider the vertical mixing approach unacceptable.

ATM response: The researchers have tried to answer the questions and concerns in earlier discussion in the responses to the USACOE, USGS and EPA. In those sections, the issues were covered in some detail attempting to clarify many of the misunderstandings and concerns regarding the mixing scheme and to demonstrate the adequacy of the scheme. We continue to be open to ways that may help make the review group more confident in our mixing approach.

Comment #3: One specific issue that needs to be addressed is the apparent sensitivity of the calculated diffusivity values to the smoothing of the tidal range. Section 5.2 in Appendix Q is not clear on exactly how the smoothing was performed. USGS developed a modified diffusivity input file in which the diffusivity values were calculated without smoothing the tidal range. During the high mixing periods in July 1999 and August 1999, the diffusivity values calculated by USGS were 40 percent and 20 percent higher than the values used by ATM for calibration, respectively. For comparison, the sensitivity test performed by ATM varied the diffusivity by only +/- 10 percent. Based on this test, ATM concluded the model was sensitive to changes in the vertical diffusivity. ATM should provide justification for the calculation of the diffusivity values and re-evaluate the sensitivity of the model results to this input.

ATM response:

The sensitivity to mixing performed by the researcher and the analysis performed by the USGS are quite different and the impact on the results would also be quite different. Using the log law relationship without smoothing does result in some higher values for D_v but also results in equivalently lower values. These lower values would tend to compensate for the higher values in the overall influence of D_v .

The sensitivity analysis performed by the researchers for the final report, added a constant positive percent offset in one case and a constant negative percent offset in the other case, to the entire curve. There would therefore be no compensating values for those conditions and the impacts to the results would be expected to be quite a bit higher.

Savannah Harbor Expansion: Interagency Coordination on Water Quality; Questions to be answered in agency letters

Dr. Jack Blanton – Skidaway Institute of Oceanography

Comment #1: I guess I have been keeping a low profile on the MTRG work. In fact, our travel project with GPA expired more than a year ago so I've not been attending any of the meeting. As I recall, it was quite a hassle to get the travel funds in the first place, so I didn't pursue it.

I have a rather large and heavy box of reports on the ATM model work. While I've quickly looked them over, I have not had the time to really study them. Paul Conrads encouraged me to look at Appendix Q (in the box of reports I have now), and it's on my "to-do" list.

I really have no expertise in water quality modeling, and I've been assuming that the hydrodynamic model that drives it was a "done deal." However, my reservations about the hydrodynamic model remain the same: the turbulence closure scheme is BAZAAR. Efforts to get them to include (or at least examine) more defensible schemes, as requested through Seim's and my earliest involvement a few years ago, have not happened, at least as far as I know. Any attempts to handle the effects of stratification on flow and transport, especially salt intrusion predictions, will always be clouded by the turbulence closure scheme.

Thanks for getting in touch. I'll try hard to take a look at Appendix Q within the next few weeks and get back to you if I can provide comments that would be of any value. In the meantime, I would like to see the Tetra Tech Hydrodynamic Model report.

ATM response: The researchers have tried to answer the questions and concerns noted generally by the reviewer here, in earlier discussion in the responses to the USACOE, USGS and EPA. In those sections, the issues were covered in some detail attempting to clarify many of the misunderstandings and concerns regarding the mixing scheme and to demonstrate the adequacy and defensibility of the scheme. We continue to be open to ways that may help make the review group more confident in our mixing approach.

Additional Model Simulations and Comparison to Salinity Observations

Results of the model predictions for additional time periods, other than the calibration and validation periods follow.

Port Wentworth - 1992

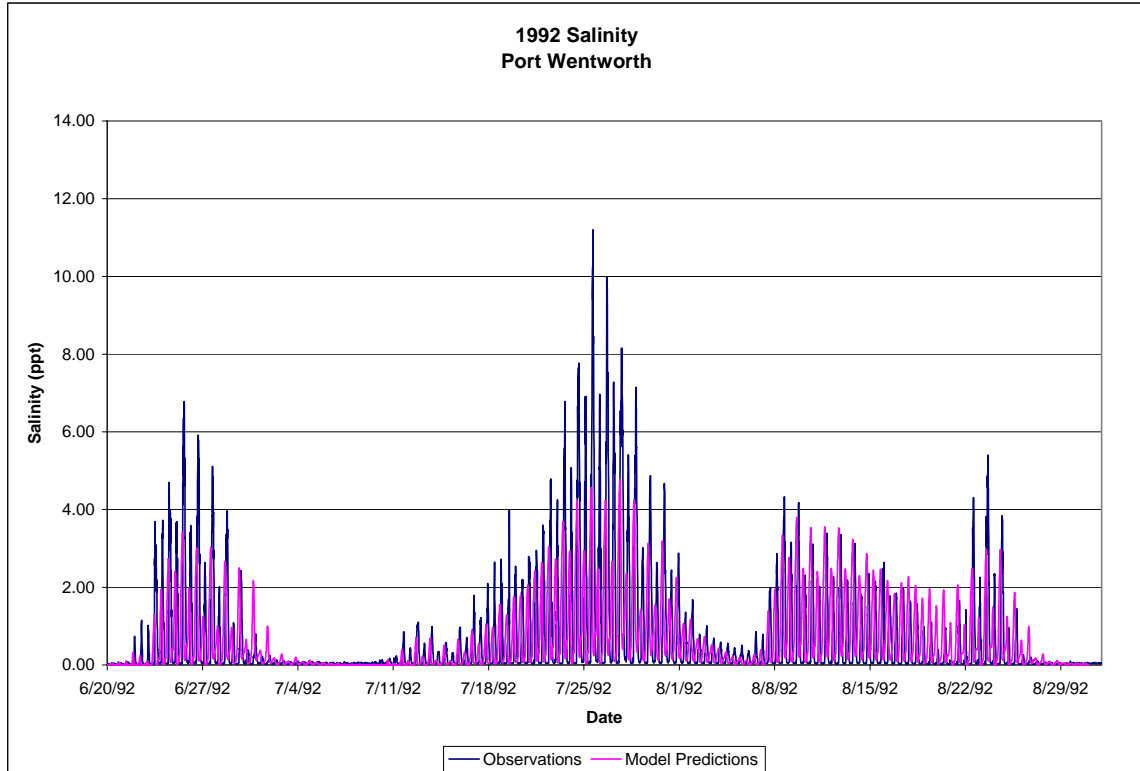


Figure 2 Model predicted versus observed salinity at USGS Port Wentworth station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.03	0.01	-0.02
50% =	0.06	0.16	0.10
90% =	1.98	1.92	-0.06
Mean =	0.60	0.56	-0.04

USF&W Dock - 1992

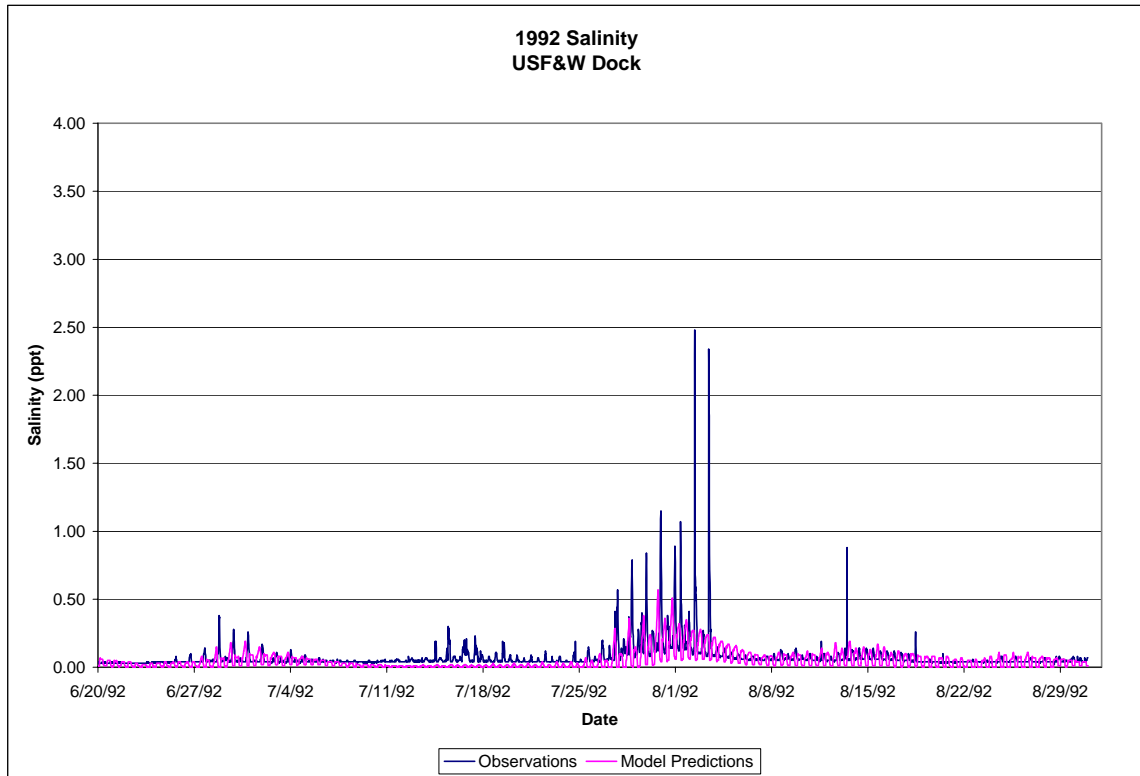


Figure 2 Model predicted versus observed salinity at USGS USF&W Dock station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.03	0.00	-0.03
50% =	0.04	0.03	-0.01
90% =	0.12	0.13	0.01
Mean =	0.07	0.05	-0.02

Lucknow Canal - 1992

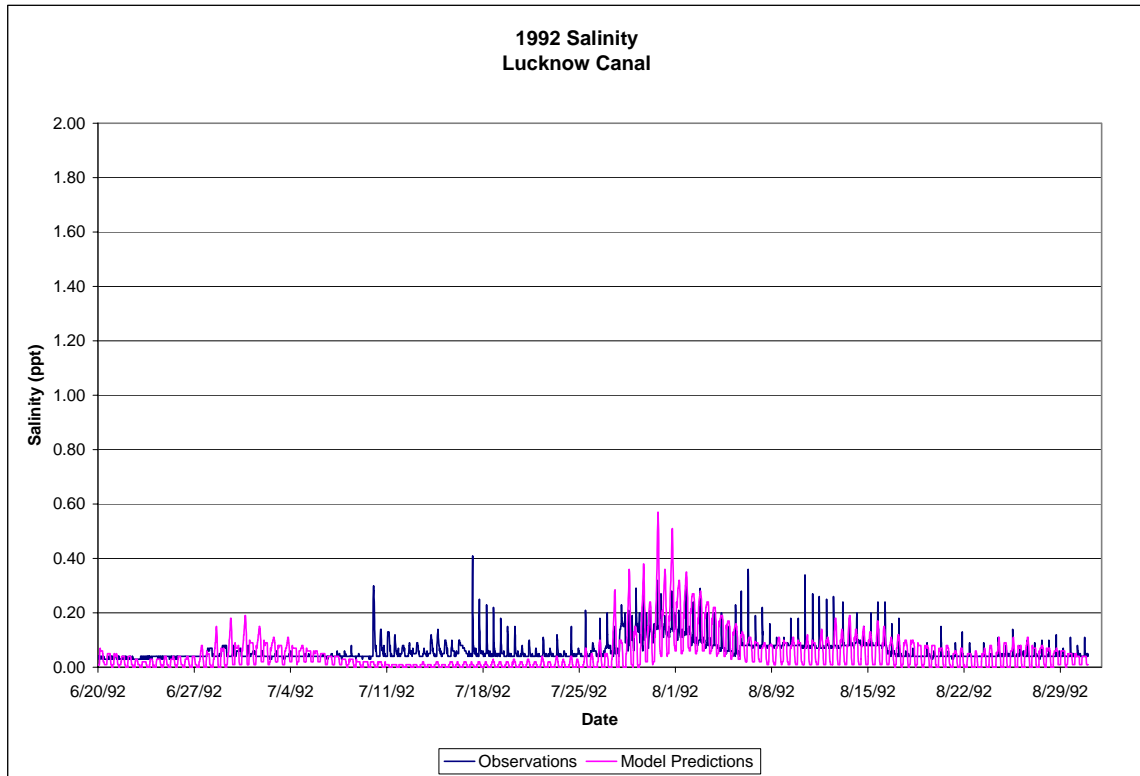


Figure 3 Model predicted versus observed salinity at USGS Lucknow Canal station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.04	0.00	-0.04
50% =	0.04	0.01	-0.03
90% =	0.10	0.03	-0.07
Mean =	0.06	0.01	-0.05

Port Wentworth – 1997

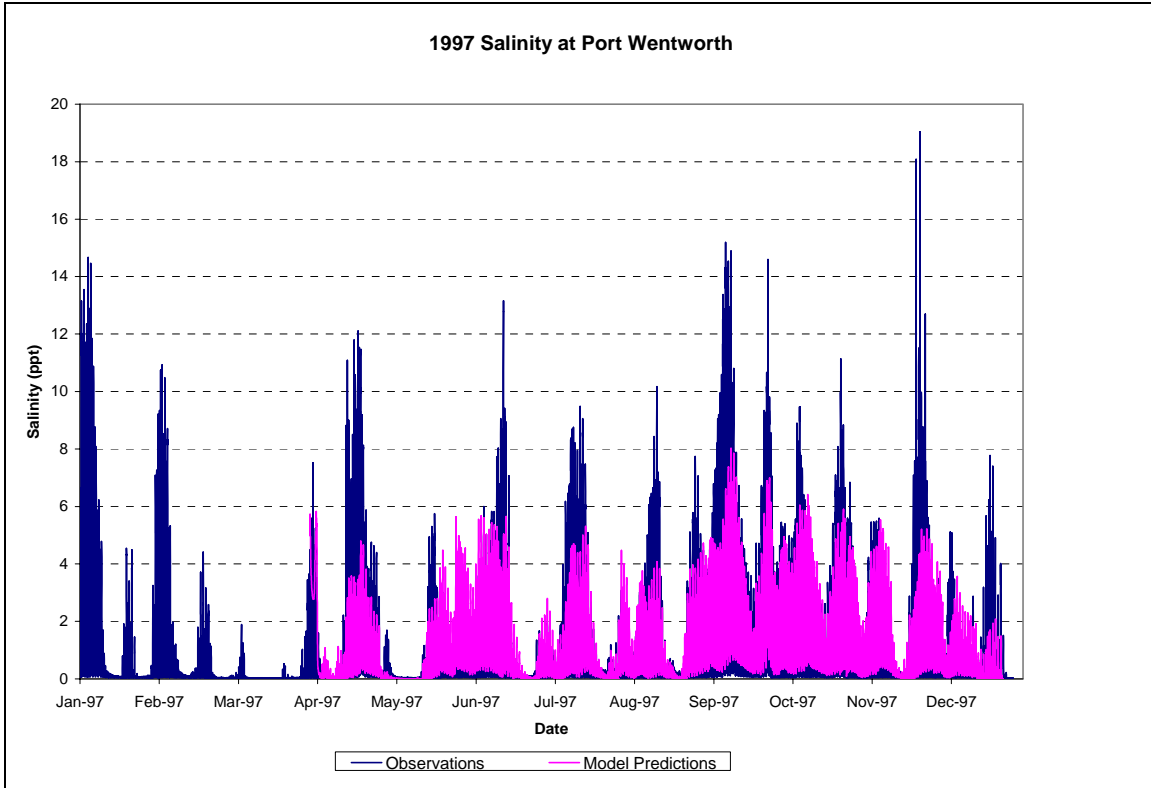


Figure 4 Model predicted versus observed salinity at USGS Port Wentworth station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10%=	0.04	0.03	-0.01
50%=	0.16	0.59	0.43
90%=	4.33	3.59	-0.74
mean=	1.28	1.24	-0.04

USF&W Dock - 1997

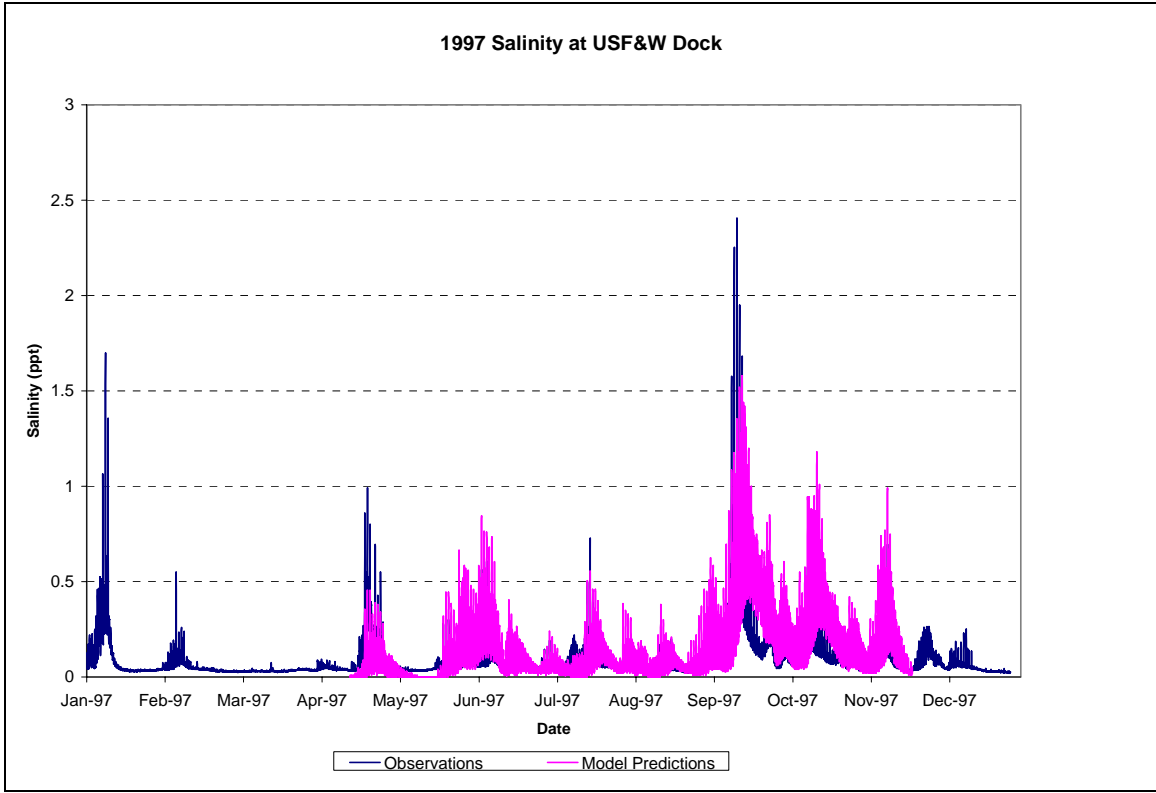


Figure 5 Model predicted versus observed salinity at USGS USF&W Dock station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.04	0.01	-0.03
50% =	0.06	0.10	0.04
90% =	0.20	0.42	0.21
Mean =	0.10	0.17	0.06

Lucknow Canal - 1997

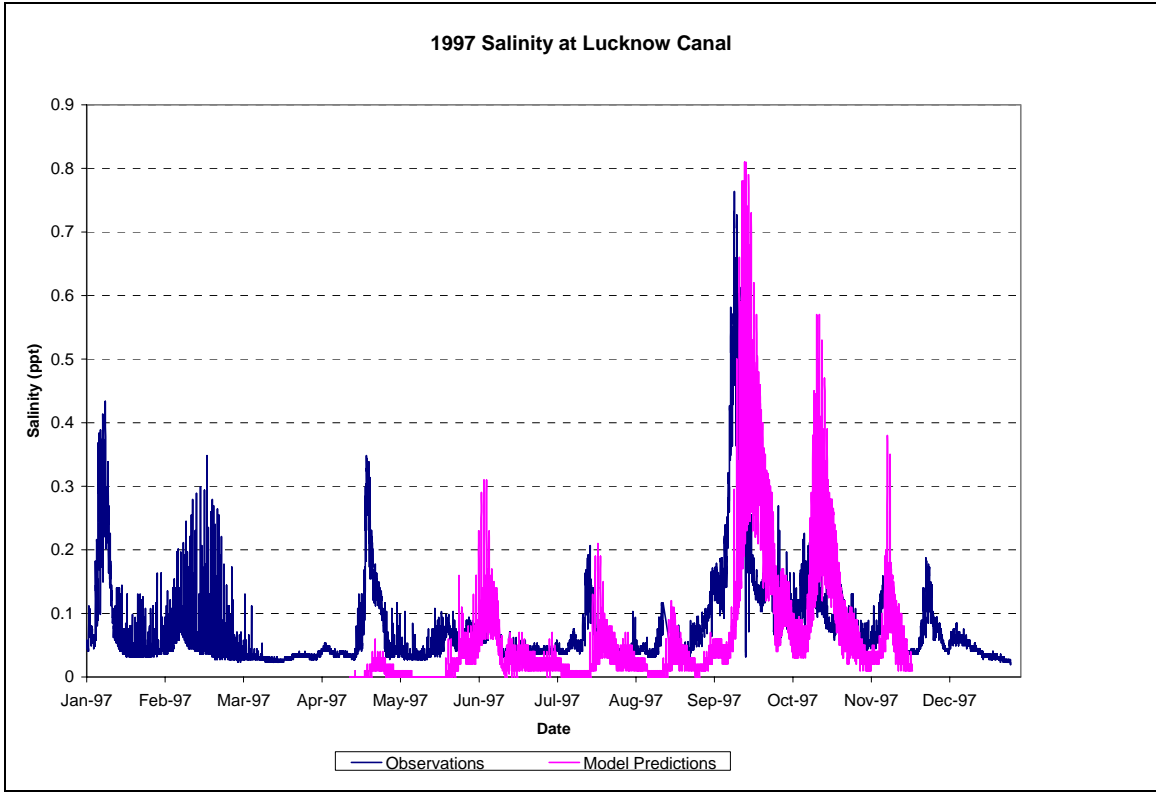


Figure 6 Model predicted versus observed salinity at USGS Lucknow Canal station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.04	0.00	-0.04
50% =	0.05	0.03	-0.02
90% =	0.15	0.15	0.00
Mean =	0.08	0.06	-0.02

Port Wentworth - 1999

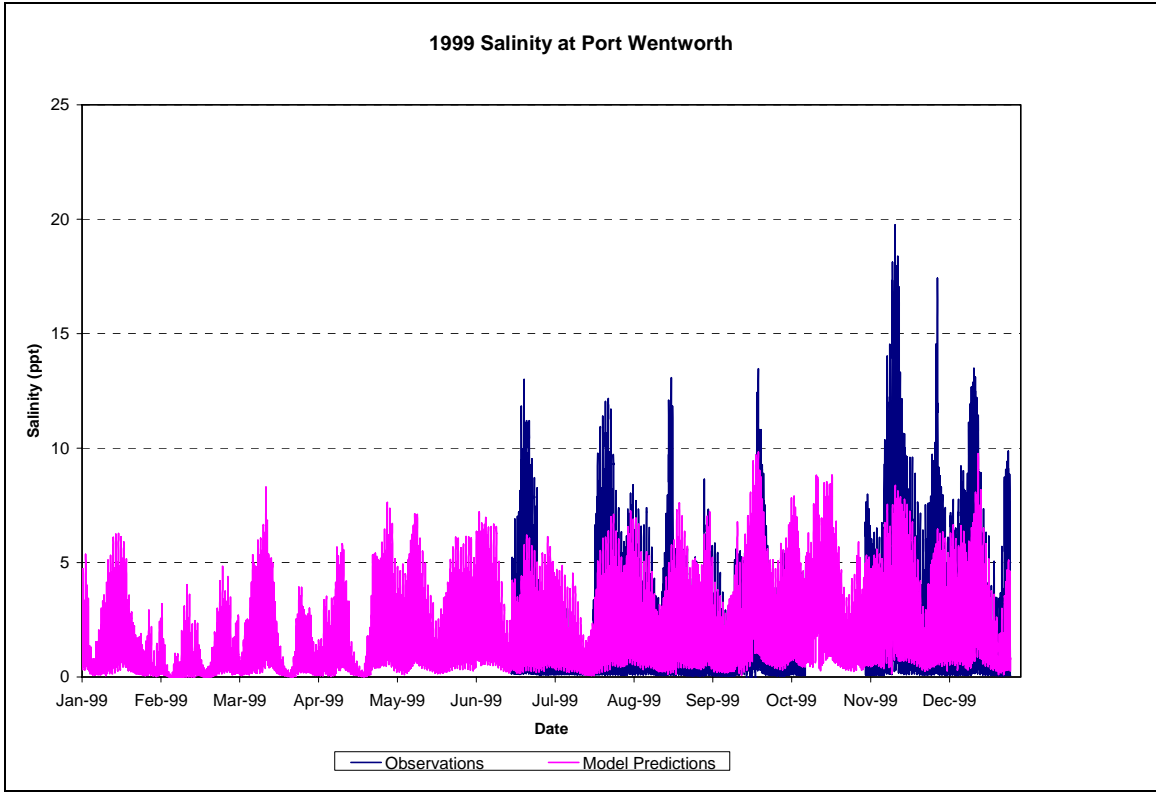


Figure 7 Model predicted versus observed salinity at USGS Port Wentworth station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10%=	0.09	0.48	0.39
50%=	1.30	2.12	0.82
90%=	6.91	5.58	-1.33
mean=	2.63	2.65	0.02

USF&W Dock - 1999

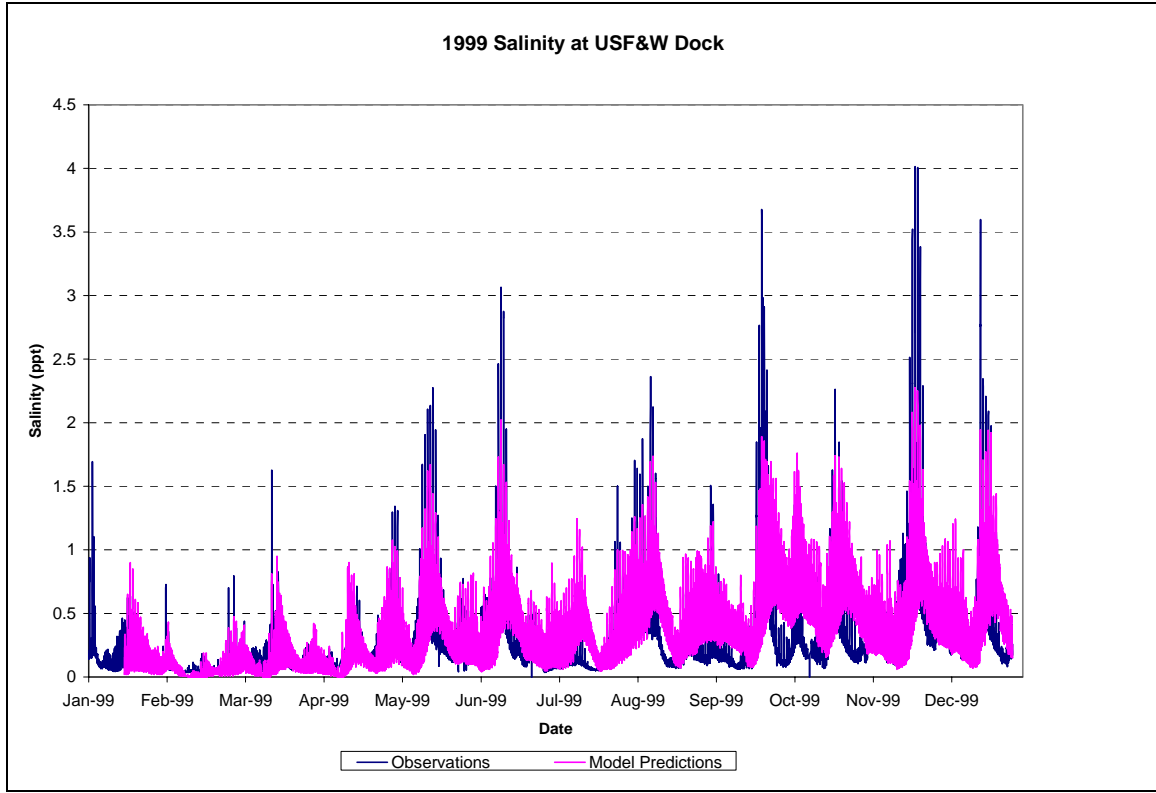


Figure 8 Model predicted versus observed salinity at USGS USF&W Dock station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.07	0.05	-0.02
50% =	0.20	0.35	0.14
90% =	0.55	0.91	0.36
Mean =	0.28	0.42	0.13

Lucknow Canal - 1999

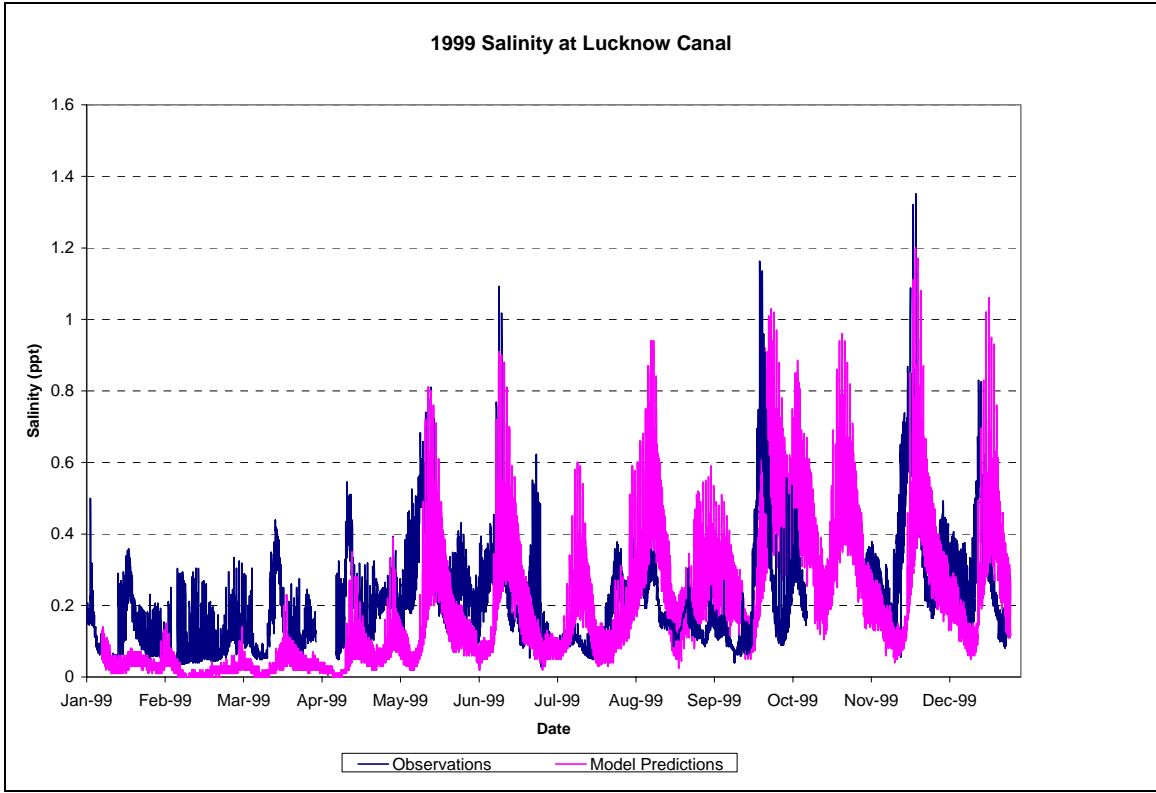


Figure 9 Model predicted versus observed salinity at USGS Lucknow Canal station

	Observations (ppt)	Predictions (ppt)	Difference (ppt)
10% =	0.07	0.02	-0.05
50% =	0.18	0.14	-0.04
90% =	0.38	0.48	0.10
Mean =	0.21	0.20	-0.01

Figure 10. Percent Occurrence of Clio Flows: Observed 1955 to 2001 and Simulation Input (1992, 1997 and 1999)

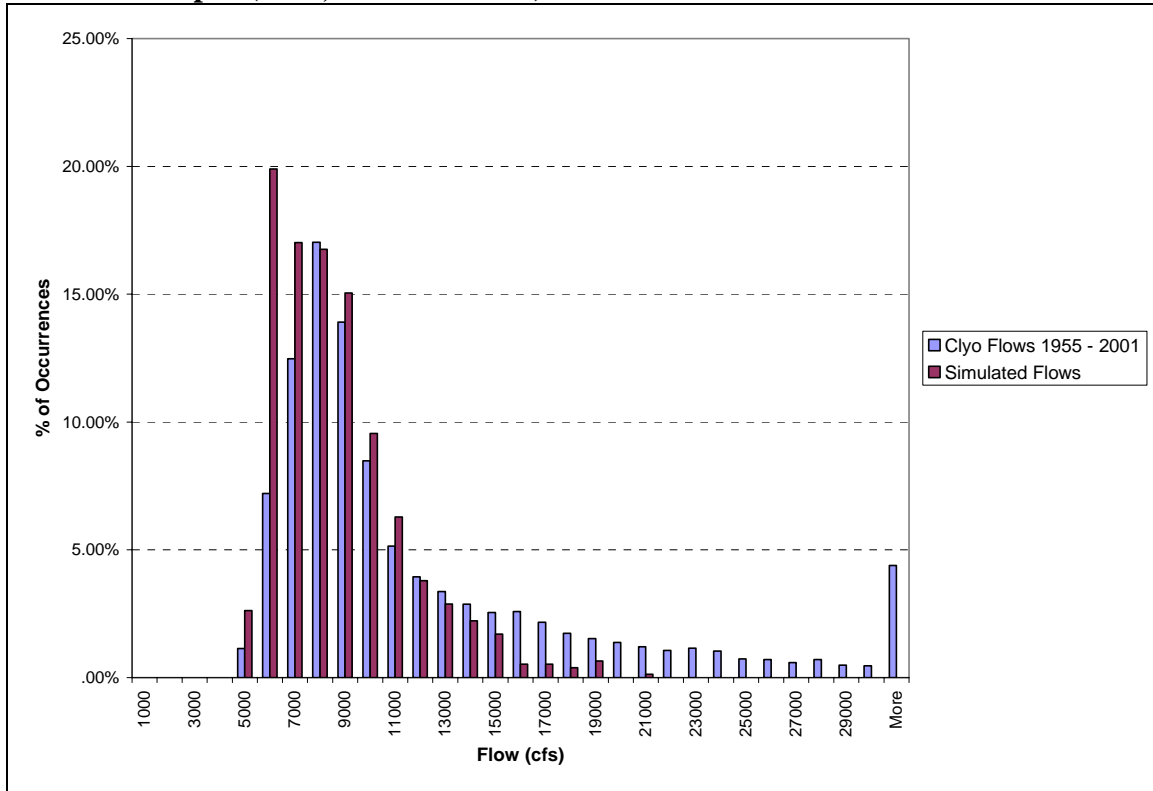


Table 1. Statistics for Observed and Simulation Input Clio Flows

	Observed Flows (cfs) 1955-2001	Simulation Input Flows (cfs) 1992, 1997, 1999
Maximum	83800	20600
Minimum	4400	4790
Average	11935	8157
5th percentile	5670	7660
10th percentile	6160	5440
50th percentile	8860	7650
90th percentile	22300	11500
95th percentile	28700	13320

Figure 11. Cumulative Percent Occurrence of Ft. Pulaski Tidal Range: Observed 1935 to 2000 and Simulation Input (1992, 1997, 1999)

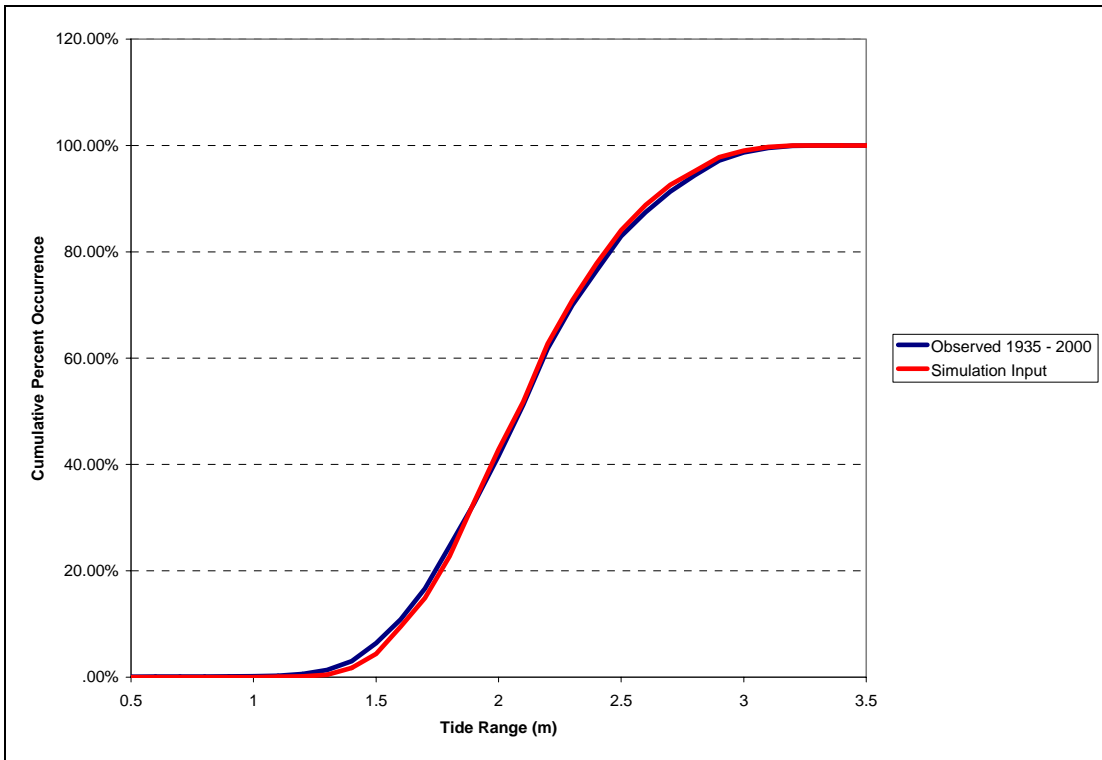


Figure 12. Percent Occurrence of Ft. Pulaski Tidal Range: Observed 1935 to 2000 and Simulation Input (1992, 1997, 1999)

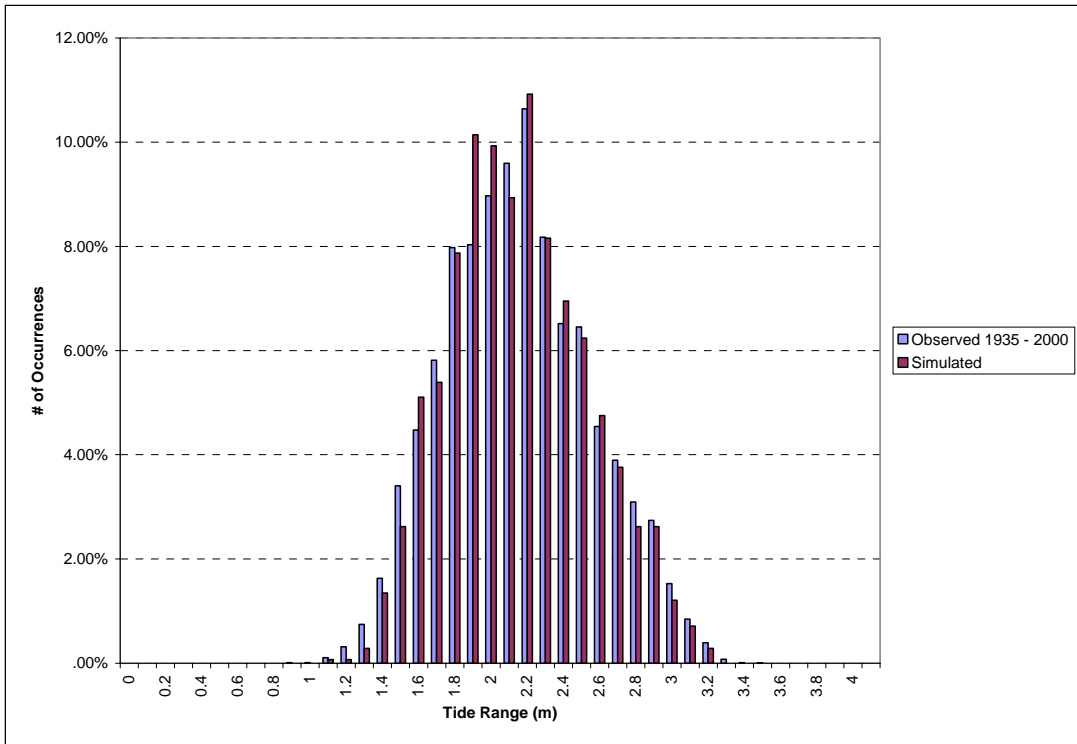


Table 2. Statistics for Observed and Simulation Input Ft Pulaski Tide Ranges

	Observed Range (m) 1935-2000	Simulation Input Range (m) 1992, 1997, 1999
Maximum	3.47	3.16
Mininum	0.82	1.09
Average	2.10	2.10
5th percentile	1.47	1.52
10th percentile	1.59	1.61
50th percentile	2.08	2.08
90th percentile	2.66	2.62
95th percentile	2.81	2.79