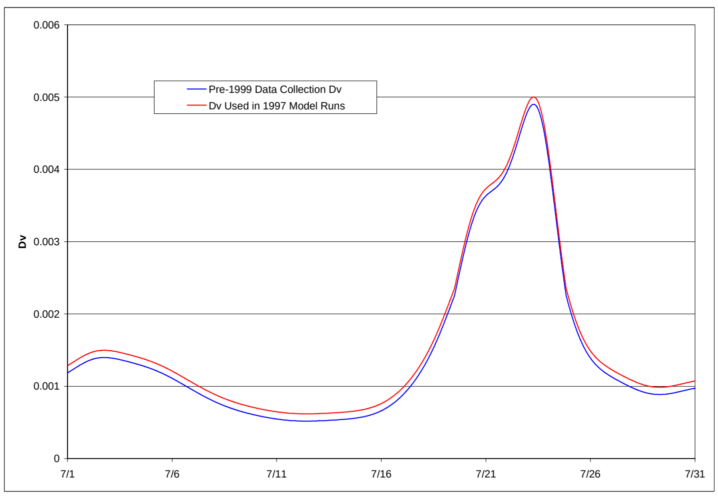
ATM Response to Comments, Questions, and Concerns of the Technical Approach to Vertical Mixing in the Savannah Hydrodynamics and Salinity Model

Part I: General Response to Issues

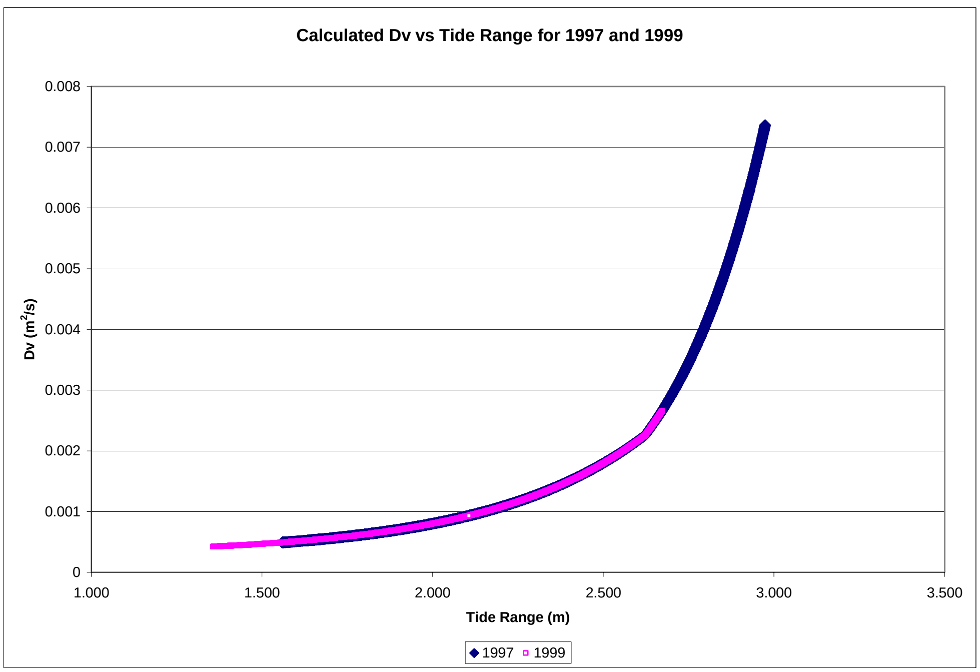
It was clear from the comments and questions raised by multiple reviewers, that there was still significant confusion on the development and application of the vertical turbulence relationship, i.e. the progression of the development, what was finally used in the model, and what data sets were used in the development of the relationship. This was based upon the write up in Appendix Q, and subsequent evaluation of the vertical mixing spreadsheet provided to the reviewers by ATM. Therefore, we have included a detailed write up of the progression of the vertical turbulence relationship for clarity prior to answering the individual questions. Hopefully this will clear up any misunderstandings. Additionally, some general issues relative to the use of empirical or more simplified relationships is discussed herein, as well as some clarification on the limitations of the hindcasting.

Development of the Vertical Turbulence Relationship

The general log-law relationship used in the calculation of the vertical mixing coefficients for the model was first developed for the Tier 1 EIS modeling effort. This effort used the 1997 data set and was completed before the 1999 data was collected. The modeling results from that effort were presented in 1998 and the vertical turbulence formulation presented within a peer reviewed paper at the 1999 Estuarine and Coastal Modeling Conference. Some of the comments made indicated that the coefficients used in the log-law equation were developed from both the 1997 and 1999 data sets. This is not the case. We have included the paper published in the ECM conference that show the graph of the vertical mixing used in the 1997 application. We also recovered a spreadsheet (dated prior to the 1999 data collection) that had the values of the vertical mixing for the month of July 1997. The attached Figure shows the 1997 values from the spreadsheet dated prior to the 1999 data collection effort, and the time series values from the 1997 application used in the model (and provided with the 2004 model deliverable). The one difference between the two plots is a 0.0001 offset that has (as seen from the figure) relatively minimal effect on the values.



The spreadsheet originally provided to the reviewers was confusing in relation to the text provided in Appendix Q. ATM apologizes for that confusion and for clarification outlines directly how the vertical turbulence relationship was developed and applied in the model (additional detail also provided in response to Paul Conrads Comment #9). It should be stated that ATM understands that the vertical turbulence relationship is an empirical one, and was developed to capture the 1997 conditions. The final relationship plotted in the attached plot shows the original vertical eddy diffusivity versus the tide range relationship as developed without the 0.0001 add in. The relationship is the result of combining two log-law equation relationships (see additional details in Paul Conrads Comment #9 response). The plot presents the two curves developed using the Log-Law relationship, and the combined curve that was ultimately used. The joining of the two log-law curves was in essence "a tuning" to capture the 1997 salinity stratification conditions, i.e. the relationship between tides and mixing that gave the best results was used, but then it was applied directly, with no alteration to the 1999 dataset.



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, 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. 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.

Confusion in Calibration versus Validation

ATM realizes that the way calibration and validation are presented relative to the development of the vertical mixing coefficients is confusing. The definition of which data set was to be the "calibration" set and which was to be the "validation" set came from early on MTRG meetings where it was decided that because 1999 was a more complete data set than 1997 it should be used for the calibration. Subsequently when the vertical mixing model was initially applied to the 1999 data set the results were such that no modification was needed in the vertical mixing relationship, and because it was developed to capture the 1997 conditions it wouldn't be appropriate to make modifications. This provided a test of the scheme.

ATM agrees that this is not a typical "calibration/verification" scenario, but it should be recognized that all of the coefficients (i.e. vertical turbulence formulation, bottom friction, and horizontal diffusion) are implemented consistently between the two years.

In order to help further support the models applicability outside of our time periods, ATM has run other time periods and provided comparisons to USGS data for salinity, and WSE at available stations. These results are presented in the detailed comment responses.

Use of Simplified or Empirical Relationships for Vertical Mixing versus Turbulence Closure Schemes

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 are being used, however not all researchers are finding the turbulence closure schemes to be the best approach.

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)"

The use of empirical relationships for turbulence is not unique in model development and a relationship such as was developed by ATM (i.e. tide range to vertical mixing) is also not unique. In a text entitled "Coastal and Estuarine Studies: Mixing in Estuaries and Coastal Seas" published in 1996 by the American Geophysical Union, Z.Z. Ibrahim presents "A Spring-Neap Flushing Box Model". In that report he presents a parameterization for mixing with tidal range for use in systems with incomplete mixing. The paper states relating to a previous assumption presented of constant vertical mixing for a two layer box model:

The major weakness of this [PREVIOUS PRESENTED CONSTANT MODEL IN TEXT] is in the assumption of complete mixing. We may, however, compensate for this assumption by parameterizing a mixing factor, m, as a function of the tidal range. For example, we may chose an arbitrary linear relationship such that

$$M = aR + b$$

where m *– mixing factor*
R *– Tidal Range*
a,b *– constants*

giving increased mixing with increasing tidal range. A baseline mixing due to the river flow is allowed for by the constant b. When m=1, mixing is complete (100%) and when m=0, no mixing occurs. For the tide conditions used here, the constants a and b may be selected to give a range of mixing factors. The extent of the salinity stratification within the estuary at various tidal ranges, for example, may be used to set limits to this expression. We select a mixing factor due to the river flow only, with no tide, of 0.05 or 5% mixing. Therefore if we select a mixing extent of 90% for a maximum expected tidal range of 5 meters we may calculate the constant a to be 0.17. In this exercise a linear function is selected, but any non-linear function of tidal range my serve the purpose as well.

While this method is simplified it demonstrates that use of such empirical relationships is not outside of the realm of modeling practice.

ATM is not stating that the use of turbulence closure schemes is not a good way to approach problems of this nature, it is. What we are stating is that there are a number of ways to tackle the problem and other researchers have found problems with the use of higher order closure schemes and have gone to the use of simpler empirical schemes to provide for a solution. We wish to see that an open mind be kept for the use of the simpler schemes.

With this in mind, it is also clear that many of the reviewers are not comfortable with aspects of our scheme. Recognizing this ATM has had discussion through this review process on methods of recasting our formulation in terms that are based upon existing literature methods (rather than an empirical formulation developed by ATM) while maintaining the spirit and accuracy of the solution. Presently we feel the model shows very good performance (and as will be seen by plots of performance during other periods from our calibration/validation periods presented in the response to detailed comments). This would not be a difficult task and does reflect some of the suggestions made by reviewers herein.