# E-1

# POKREFKE

## 2ND DRAFT COMMENTS
## (W/ GAINES EMAIL REPLY)

**Comments on Micro-Model Comparison Studies**
**Draft Number 2**
Tom Pokrefke

1. As requested, I have reviewed the subject draft. I probably did not review it to the depth that I reviewed Andy Gaines dissertation, but much of this document came from his dissertation. I gave my comments on the dissertation directly to Andy this past summer in St. Louis, so any comments that I made on that document, which were not addressed by Andy, would still be applicable. I have some *general/typo-type* comments and then some specific comments on the Draft Number 2.

2. I have read the comments that Charles Nickles has provided to Andy, and I concur with what he has provided. In an effort to be as brief as possible and to make addressing all of the comments easier for the researchers, I will not repeat concerns/issues raised by Charles.

General/Typo-Comments

3. Comments are:
   a) **Page 1-4, Line 3**. Remove the first "and." done
   b) **Page 2-1, Line 1**. The word "arbitrary" was used. I believe LWRP is anything but arbitrary and is inappropriate to use, at least in the case of WES models. LWRP represents the 97% duration water surface profile (flowline that is equalled or exceeded 97% of the time). The decision to use the 97% duration WSP was made many years ago, but the decision was arbitrary because other WSP could have been used with equal effect. For instance, recent studies on the White River, AR used a 95% duration WSP to define a LWRP. Agree that it was not arbitrary. A low flow was intentionally selected just as in AR to address available navigation depths over most of the year. strike word Arbitrary
   c) **Page 2-16, presented equation**. Up until this point "H" was used for depth, but starting here "D" was used. Need some consistency throughout the report and study. Change D to H in equation
   d) **Page 2-16, first three lines under Section 2.2.2**. Line 1 – remove "of individual," Line 2 – remove one of the "of" after the word "calculation," and Line 3 – believe "an" should be "and." Required typographical changes noted.

Specific Comments

4. Comments are:
   a) **Page 1-7, last paragraph**. States that the various parameters "…offer insight into the two-dimensional character of the flow." Since we are dealing with a riverine system and open channel flow, as a researcher I want insight into the three-dimensional character of the flow. If all I can get is two-dimensions, I can't really address the problems I am trying to solve. This paragraph discusses cross-section area, depth and width/depth ratio, which are two dimensional in nature.

Therefore, these parameters alone only offer insight into the two dimensional character of the flow. Other factors/parameters must be incorporated to gain an understanding of the three dimensional flow characteristics.

b) **Page 2-14, Section 2.1.5.2.** Text spent a far amount of time talking about weighted values, but I saw no evidence where "volume" was ever used in the analysis. The volume was used in calculation of reach weighted parameter values shown in Tables 3-6 and 3-7.

c) **Page 2-17, Section 2.2.2.** I personally have a problem with making an issue of the prototype variability. No doubt it exists and no doubt that most river engineers and riverine modelers know that it exists, but it is virtually impossible to qualify, forget about quantifying. The number of prototype surveys available to any researcher limits them, and they may still miss much of the "story" for any particular reach. This discussion included because some (river engineers and modelers included) do not realize that prototype variability exists nor do they realize the importance of prototype variability. The statement "Therefore, only the spatial variability can be considered" is correct in the MM but not in a model that attempts to replicate a prototype hydrograph. As I stated in C Nickles review, this needs to be addressed.

**Page 2-21, Table 2-4.** I had trouble with this table in Andy's dissertation and continue to have it here. Completely ignoring any model data for the moment, what this Table tells me is that for the 1975 prototype conditions, the area (I'll only focus on one parameter) from a 1:8,000 or 1:16,000 scale model map is about 27% greater than from a 1:300 scale model map. This continues to dumb-fond me that there is such a huge difference when all we are really looking at here is **prototype data**. Part of it may be averaging 28 ranges on the 1:300 scale verses more than 70 ranges on the 1:8,000 and 1:16,000 scales, but that just seems like an unreasonable discrepancy that needs to be addressed. I have no definitive answer why the PROTOTYPE data for the WES model study are (considerably??) different than PROTOTYPE data from the MM study. However, there are three major differences between the sets of numbers: **1)** The WES **model** (786.5-796.0) extended over a shorter reach than did the MM (785.0 -798.0) and the additional lengths (785-786.5 and 796-798) had greater channel depths which would tend to increase areas when averaged over the model length; **2)** there were a significantly greater number of ranges included in the MM than used in the WES model analysis and including a greater number of sections in the deeper segments of the prototype would tend to increase the cross section areas when averaged over the model length, and **3)** Bathymetry from the WES study were converted to a digital format using a slightly different procedure than used with the MM. The WES study PROTOTYPE data were converted to digital by scanning and digitizing the map LWRP elevations for the 1975 and 1976 surveys. Data used in the MM analysis followed a similar approach except that absolute elevations were digitized and then the respective hydrographic sheets (~3-4 miles per sheet) were converted to LWRP elevations using a single LWRP conversion for each sheet which produced a stepped low water reference plane as opposed to a non-linear LWRP throughout the reach. The scale of the models has no impact on the calculated PROTOTYPE values. **NOTE FOLLOWING AFTER FURTHER INVESTIGATION:**

**Bottom line on this: Based on Charles Nickles analysis of the WES models, there were only 28 ranges used in the WES 1:300 model -- Ranges 14-41. These ranges extended from about RM 789 to 794, which is the shallowest part of the reach (See figures 3-2 and 3-5 and 3-6). The micromodel extended over a much larger longer reach as outlined (RM 785.0-798.0) and included ranges having much greater depths than found between RM 789 to 794. This is the reason for the seeming disparity between the prototype data sets shown for the 1:300 model and the two micromodels.**

**These differences need to be highlighted in the report.** — clarified pg 2-19

d) **Page 2-22, Section 2.2.3.** You appear to spend this whole section making an argument for using +20-ft LWRP as a reference for the computations, and then in the end use 0-ft LWRP anyway.The emphasis in this section is on the need to consider several elevations in difference calculations. The last para. on pg 2-24 states the reason for using 0 LWRP.

e) **Page 2-26, Table 2-4.** Are the values here percentages or what? Average differences and mean squared error. Table annotated to make clearer.

f) **Page 2-25, Figure 2-7.** Just a comment about this figure. I hope someone other than Andy understands it, because I sure don't. Figure 2-7 demonstrates that the water surface elevation selected for the basis of calculations directly influences the calculated values of cross-section area, top width, hydraulic depth and width to depth ratio. Figure 2-7 illustrates the relationship of selected water surface elevation (in LWRP elevations) and cross section area to the difference calculation for Ranges 25 through 59 in the Kate-Aubrey reach of the Mississippi River. Prototype conditions are from a 2001 hydrographic survey. Model data are from the 1:8,000 micromodel. Figure 2-7 indicates that larger differences result when the selected water surface elevation is lowest. As the water surface elevation used for calculating cross-section area increases, the difference between model and prototype reduces until approximately the ±20 percent level of difference. This is consistent with observations of the physical response of both the model and prototype channels where the greatest changes occur in the low flow channel.

g) **Pages 3-20 through 3-24 and all similar figures to these throughout the report.** These figures are the most enlightening to me and helpful in evaluating model performance. Noted.

h) **Page 3-26, Section 3.1.4.** Although I have heard Andy say this throughout my involvement on this evaluation effort, this really was not "predictive" in that the micro-model was operated with structures constructed in 1999 and results compared to a 1998 prototype survey. There appears to be a disconnect here. Constructin in 1999 for the model reach consisted only of 300 LF of minor repairs to KA dike 1U. There were only 1000 LF of minor repairs to KA Dike 3T in 1998. Prior to 1998, the last repair work was in 1995 and the last major dike construction was in 1983. The last revetment construction within the model reach was in 1997. Therefore, the model represented dikes constructed through 1999. Even if the micro-model results would be compared to the 2001 prototype survey (which is included in Table 3-6 on page 3-37) this looks like nothing more than a recalibration or check of the initial calibration. The predictive, or plan, run was to compare the model's ability to assess prototype response to a given change. In other words, could the calibrated model "predict" what would actually occur in the prototype? In essence, this is like a check on the model calibration.

i) **Page 3-36, Section 3.2**. In my opinion, repeatability in any type model can be approached in two ways – (1) by initiating testing from the same starting conditions and seeing if the same ending conditions over the same number of hydrographs occur (with some "natural scatter"), or (2) by starting "the clock" after the model has reached **stability** (based on the researchers definition) and running a fixed, but meaningful (whatever that is) number of hydrographs and determine how "stable" the model was over the entire testing time. The results presented from the Kate Aubrey 1:16,000-scale micro-model in Section 3.2.1 follow the 2nd approach. In my opinion, the resulting overall variance presented on page 3-44 does not provide a high degree of confidence. The Jefferson Barracks micro-model study presented in Section 3.2.2 also appeared to follow the 2nd approach; however, the tests were conducted with a constant discharge, which significantly (in my opinion) reduces the variables in the testing. Regardless of this fact, the variance presented on page 3-49 is quite **impressive**. The most meaningful comparison (at least relative to large-scale WES-type models and micro-models) is presented in Section 3.2.3. It was stated that Rob had addressed multiple base test runs in his thesis, but I was unable to find that discussion. However, my point here would be that in the results of the Dogtooth Bend Reach presented on page 3-50 of the report, it is entirely possible that the comparison of the WES model is from the 1st approach above and the micro-model is from the 2nd approach. Such a comparison is not valid because the WES model was working toward "stability," and the micro-model had reached "stability." Rob's Thesis (Davinroy, 1994) states that "depths were collected at the same relative cross section locations from 7 individual base test runs of the Dogtooth Bend model at WES." My understanding of the WES base test was that the model was operated to stability in order to form the base test bathymetry. This would be in the 2nd approach described in this comment and consistent with the approach used for the micromodel. I will await Tom Pos response to this but I would like to confirm that 7 individual base tests were run on Dogtooth bend.

5. I looked through the remainder of the subject report, but did not do an in-depth analysis of the results. I made a sincere effort to try and understand what was being said about "truncation" in Section 2.2.4 and Appendix A, but I was unable to see the value in it. Figures like A-5 through A-10 just did not provide me with any meaningful information. Noted.

6. I applaud Andy for Appendices B and C. There is some good data and information there that shed a lot of light on both WES models and micro-models, and it is great to see it in one location and not lost forever. To that end, I came to the realization that the method classically used at WES and with micro-models by comparing prototype and model surveys to determine the verification of WES models and calibration/base test of

micro-models is probably the best procedure. Quantification of parameters can be done, but when all is said and done, I believe leaving it to the modeler to determine the fact as to the verification or calibration of the model or micro-model, respectively, puts the control and reasoning in the correct place. However, I think that the quantifications, as presented, would be of value to the modeler in evaluating limitations and departures from prototype conditions that could be helpful in evaluating and reporting predictive tests. Noted. Concur. I think this would be a far better report if it only contained app B and C, just enough text to describe what is found in them, Kate Aubrey plates/reach plots, and repeatability data. Although I was a strong proponent of the reach averaging of parameters, I now find them misleading, particularly when comparing 5 miles of 1975-1976 to 13 miles of 1973, 1975-1976.

Thomas J. Pokrefke, Jr., PE
Hydraulic Engineer
Coastal and Hydraulics Laboratory