APPENDIX

Peer Review Comments and Author's Reponses

A draft copy of the report was sent out for technical review in September, 2005. Over the period of the next three months comments were received from five reviewers:

- Joe Lyons, U.S. Bureau of Reclamation
- Kirk LaGory, Argonne National Laboratory
- Jimmy O'Brien, TetraTech
- Doug Osmundson, U.S. Fish and Wildlife Service
- Melissa Trammell, National Park Service

Comments from Jimmy O'Brien, Doug Osmundson, and Melissa Trammell were supplied to the author as MS-Word documents, and are included in the following pages. Joe Lyons provided a paragraph of comments and a marked-up paper copy of the draft report. Kirk LaGory provided a marked-up copy of the draft report but no detailed written comments.

The comments from all of the reviewers were considered, and in most cases these comments and suggestions were incorporated into the revised report. The ***Response to Reviews Comments*** which follows the text of their documents focuses on the primary concerns of the reviewers. Suggestions for changes in spelling, wording or context were incorporated into the revised report as necessary.

RECOVERY PROGRAM FOR THE ENDANGERED FISHES OF THE UPPER COLORADO

DRAFT REPORT EVALUATION

PROPOSAL TITLE: Colorado River Channel Monitoring

PRINCIPAL INVESTIGATORS: Dr. John Pitlick, Department of Geography, University of Colorado

Rating: Excellent - E Good - G Fair - F Questionable - Q

1) EVALUATION OF THE PROPOSED SCOPE OF WORK

a) Evaluation Factors — Rating

1. Clear, measurable objectives with specific timeframes ..... G
2. Relevance of objectives to recovery ..... E
3. Description of methods or approach, validity of methods, appropriateness of statistical analyses ..... G
4. Integration with existing knowledge / incorporation of relevant scientific literature ..... G
5. Feasibility and probability of achieving the stated objectives ..... G
6. Timeliness of proposed work ..... E

b) Overall Evaluation: (Check one box only)

❑ Excellent X Good ❑ Fair ❑ Questionable

2) BUDGET EVALUATION: Underestimated (-) Appropriate (x) Overestimated (+) Unknown (?)

Personnel (-) Equipment (-) Travel (-) Materials (x) Other (x) TOTAL (-)

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3) EVALUATION OF PRINCIPAL INVESTIGATORS — Rating

1. Knowledge of, and experience in, the proposed area of work ..... ____
2. Past accomplishments ..... _____
3. Significance of previous contributions in this area of work ..... _____
4. Group integration, cohesiveness, and collaboration (if appropriate) ..... _____

PAGE 2

4) DETAILED COMMENTS

Explain the basis for your evaluation. The strengths and weaknesses of the proposed scope of work should be discussed, with specific comments on the potential contribution to recovery of the endangered fish in the upper Colorado River basin and on the background of the investigators in relation to the current application. Please provide any recommendations for improving the study proposal.

General Comments:

Overall this is a very good study and report. The recommendations for target flows, frequency and duration are excellent and within the scope of tenements of the Recovery Program. As shown in Table 3, the recommended higher threshold flows were not experienced during the period from 1998 to 2004. The report states on page i that "(t)he limited availability of water in subsequent years prevented further tests of the geomorphic effects of bypass flows." It is recommended that this study be extended until a period of several successive years of high flows have been monitoring for cobble bed movement and channel morphology changes in the 15 Mile and 18 Mile Reaches. This study is a good argument for having contingency plans to delay or extend studies undertaken during low flow drought periods that require high flow data. Forcing the completion of studies within a time frame during low flows limits the ability of the researcher to meet objectives and perhaps draw definitive conclusions. The researcher may have to rely on extrapolation of low data and study results to meet the objectives requiring a high flow analysis. The comment on page 26, that the frequency of threshold discharges during the period from 1998 to 2004 … "provide an indication of the ability of the Recovery Program to meet the flow recommendations given in previous studies and reports…" exemplifies this problem as well as being a delineator of the success of the program.

Specific Comments:

The following specific comments are relatively minor when considered in relationship to the overall contribution of the study to the Recovery Program.

1. It is noted that the objectives listed on page 5 of the report do not correspond exactly to the objectives in the provided Proposed Scope of Work for Project 85A. The Project 85A objective in the provide Scope of Work "(t)o develop a matrix which can be used by the coordinated reservoirs operations group to tailor reservoir operation to target multiple objectives of habitat maintenance and creation" was not addressed in the report. Storage and seasonal runoff conditions under which coordinated reservoir operations should be evaluated and would be a significant contribution to the Recovery Program. The figures on page 27 which illustrated the reservoir release in 1998 and 1999 are unreadable.

2. The nature of bed material in the 15 Mile and 18 Mile Reaches indicates that the sand sized sediment in this reach is supply controlled. The sediment supply to these reaches is controlled in part by the seasonal storage and delivery from upstream pool reaches. This process of storage and release would account for some of the variability between the peak discharge and peak suspended sediment concentration at the various gages shown in Figures 17 and 18. On page 41, the statement is made that "(i)f sand was moving through this river system as a wave or pulse, then there should be lag in the timing of the peak percentage of sand in the downstream direction. This does not appear to be the case, suggesting again that much of the sand is transport is derived locally, i.e. for the channel bed, rather discrete sources upstream." This is

true if "derived locally" refers to the pool reaches upstream rather than more distant tributary watersheds. A similar pattern of sediment movement has been observed at the Jensen gage on the Green River in response to sediment storage and delivery from the Island Park/Rainbow Park reach. The sand collects and forms into waves in the Island Park reach in a seasonal redistribution. The sand waves store near the lip of Cross Mountain Canyon where it is delivered to the Jensen gage with the start of the spring runoff.

The sand supply to the 15 Mile and 18 Mile Reaches is of critical importance to this study and should be addressed in more detail regarding tributary inflows associated seasonal storms and potential upstream storage reaches. The only comments provided regarding sediment supply is "…surface erosion of sedimentary rocks in areas immediately upstream of the key reaches contributes a large proportion of the sediment carried by the Colorado River…" and "(m)uch of the fine sediment is derived from surface erosion of friable sedimentary rocks underlying Roan Mesa.". This general assessment that does not shed much light on the nature of sand sized sediment movement through the study reaches or its tributary sources.

3. The term "granules" is used several times throughout the report. How does this term relate to the conventional, clay, silt, sand, gravel, cobble, boulder size fractions?

4. It is stated in the report that "(t)he single most important thing that can be done to maintain habitats used by endangered fishes is to assure that sediment supplied to the critical reaches continues to be carried downstream. Sediment that is not carried through will accumulate in the low velocity areas, resulting in further channel simplifications and narrowing." While this intuitively correct, the key factor controlling the rate of channel narrowing is vegetation encroachment in the active channel. The sediment provides the substrate for the vegetation, but until the vegetation is established, sediment remobilization at high flows is always possible. Once vegetation is established in the channel, the potential for removing the vegetation and mobilizing the sediment is significantly reduced. In general, tamarisk plants that attain a three year growth will not be removed by subsequent spring high flows. Bankfull discharge (> 621 cms) with a frequency of one out of every three as recommended in the report is critical to avoid vegetation establishment in the channel. If flows do not exceed bankfull for any consecutive 3 year period, some channel narrowing may occur with vegetation establishment in the active channel that can only be removed in the future by mechanical techniques. On page 57, it is noted, that "…the plants would not have become established if the sediment forming the bed surface had been mobilized to any extent in any of these years." The statement is critically important and it should be emphasized that tamarisk plants need to be removed within the first 2 to 3 years of growth or the majority of the plants cannot be removed by high flows in subsequent years.

5. The sediment traps were an innovative approach to collecting low flow sediment movement over a cobble bed. The conclusion that the Colorado continues to transport fine-medium sand during periods of low flow is justifiable. It should be noted, however, that because of the nature of the trap, the ability of the fine-medium sand to exchange with the bed is limited. Berry (1985) thesis "Bedload Transport Processes in a Cobble Bed Channel," and the 1984 companion reports on the Yampa River Cobble Reach by O'Brien presented the concept from flume studies that sand could be scoured from a cobble bed up to mean one cobble diameter below the bed surface without initiating cobble motion. The exchange of sand between the flow and the substrate will effect the interstices size distributions. In the O'Brien report, the critical shear stress parameter was also assumed to be 0.03, but it is agreed that this value does vary and it is likely to be slightly lower for most cobble bed reach in the range from 0.025 to 0.030.

6. The comment, "…if there is a natural tendency for the grain size distribution of the suspended sediment in the Colorado River to coarsen with time…" is confusing. It was intended that this statement reflect that the suspended load can become more coarse during the recessional limb of

the hydrograph. A coarsening of the suspended load is a physical process that reflects a change in the relationship between the fine sediment moving as wash load and the bed material load. This comment however would infer that the supply of fine sediments decreases as the discharge decreases which is true except that much of the system wide fine sediment loading may be in response to seasonal thunderstorms. In several places in this paragraph, the wording should be changed from "…becoming coarser with time…" to "becoming coarser over the seasonal hydrograph…" or "…becoming more coarse with the recessional limb of the seasonal hydrograph."

7. The last sentence on page 37, "The similarity in percentages suggests that the sand in transport in the Colorado River exchanges in equal proportions with the sand stored in the bed, consistent with the contemporary theories for equilibrium transport in gravel-bed channels (Parker et al, 1982 and Toro-Escobar, 2002)" is not an appropriate interpretation of the concept of equal mobility of gravel in rivers. The concept of equal mobility is that the average size distribution of the gravel portion of the bed load is approximately equal to the gravel distribution of the substrate. A direct comparison of the gravel moving transport in the Colorado River with the channel substrate is necessary to draw this conclusion.

8. Figure 28 is fascinating and it is likely that much more information could be drawn from the measurement of cobbles that moved or didn't move that would support the concept of equal mobility for gravel bed streams. There are a number of particles that were wedged between larger cobbles that were mobilized including a large one at the bottom of the figure. It may be possible to calibrate the critical shear stress parameter from scaling the larger particles missing in the 'after-photograph'. It is interesting to note that similar photographic and paint methods were employed at Mather's Hole on the Yampa River in 1983 and 1984.

9. On page 21, "…undulations in the bed caused by pools and riffles…" should be reworded to "…undulations in bed profile caused by pools and riffles…". It is suggested that the rest of sentence starting with "…producing a net fluid force…" be deleted. Actually, the entire discussion on page 21 and 22 is overly simplistic and poorly worded and should be deleted because it doesn't add anything to the report.

10. Page 28, the comment "(s)ome water was "lost" in the process…" should be expanded. Is this figure of speech or does it refer to actual floodwave attenuation and losses due to diversion, infiltration and evaporation?

11. The cross section resurveys illustrated on pages 29 thru 32 were completed at different times on different years throughout the seasonal hydrograph (May 98, Aug 98, Oct 99 and July 01). It is recommended that the resurveys be conducted at the roughly same time prior to and immediately following the peaks flows (perhaps in April and July) so that channel response to the spring runoff can be quantified.

This review will be considered by Recovery Program staff and the Biology Committee. If you desire anonymity, please do not sign below. The Recovery Program sincerely appreciates your assistance.

NAME: Jimmy S. O'Brien, Ph.D., P.E.  DATE: 10/17/05

Review of Final Draft Report:

'Channel monitoring to evaluate geomorphic changes on the main stem of the Colorado River"

Author John Pitlick; Geography Department, University of Colorado, Boulder

Reviewer: Doug Osmundson, Fish Biologist, USFWS

This report provides summary information on a 1998-2004 geomorphic study of the '15-mile reach' of the upper Colorado River located near Grand Junction, Colorado. Its stated purpose was to asses the effects of coordinated reservoir releases and normal snowmelt flows on geomorphic processes. In addition, conditions in one specific reach were monitored to verify earlier estimates of flow thresholds necessary for various levels of sediment transport.

The report is thorough and well-written. In addition, the author has obviously made an attempt to explain geomorphology concepts in a way understandable to those outside the discipline, while retaining the rigor and documentation required and expected from peers. For me, a non-specialist, this made reading the report both interesting and enjoyable and greatly facilitated my ability to understand and critically review the report.

Much can be said that is positive about this report. However, to aid the author in improving this draft, my comments will focus on questions that I have.

Executive Summary

Page i, line 3: As far as we know the 15- and 18- mile reaches do not provide important habitat for the humpback sucker.

Page i, lines 17-18: The word 'nonetheless' seems out of place because the coordinated reservoir releases were in years other than those described as drought years.

Page iii, line 14: I am a little confused here by the term 'bedload transport'. Based on definitions given elsewhere in the report, I would consider sand moving along the bed, out of suspension, to be included as part of the bedload when transported (defined as "overpassing" on page 18). This evidently occurs during flows less than 4,000 cfs. Or do sand particles, as they fall out of suspension, automatically deposit, getting lodged within the framework particles? I would think much of it would continue moving along the bed. If so, is the 10,000 cfs threshold referred to in line 14 referring instead to framework particles only as the 'bedload'?

Introduction

Page 2, line 4: Are there 25 reservoirs or 24 (see page 9)?

Page 3-4, summarized recommendations: The recommendations provided here are aimed at maintaining the 30-day annual average of ½ bankfull flows and 5-day annual average of bankfull flows as calculated for the 1978-1997 period of record. Some explanation is needed regarding

why maintaining the status quo of a highly depleted, post-development block of years is desirable. Why is this the standard that we should aspire to meet? Have the fish done well under these conditions? Perhaps provide some explanation that these years represent of suite of both low-flow and high-flow years, or something.

Study Area

Page 9, lines 2-5: Much of this repeats what was already described in the Introduction (page 2).

Methods

Page 16, last line: Is a 20-cm coffee can 20 cm in diameter or in depth?

Page 17, line 3: Editorial comment: who does "us" refer to? There is only one author.

Results

Page 23 and elsewhere throughout the report: In some places in the report, discharge reported in cubic meters per second is followed parenthetically by the same discharge in cubic feet per second. This is very helpful to those of us readers accustomed to using the cfs term. However, reporting discharge both ways is not done consistently throughout the report. It would be helpful if it was.

Page 23, bottom paragraph: Its hard to tell from fig. 9 that runoff was equally divided between above- and below-average years prior to 1998 because there is no average line provided on the graph. Also, any calculated 'average' is dependent on the block of years chosen for averaging. Obviously, with a 14-year period of record at Palisade, starting in 1990, coming up with an average prior to 1998 (n = 8 years) is not particularly useful.

Page 25, lines 4-6: This statement downplays the significance of back-to-back low water years. However, only recently (1987-1992 and 2000-2004) have there been 6 and 5 year blocks of consecutive years with below-average flows. Will there be more of these in the future? Probably, as noted by the author. However, it is also worth noting that such blocks did not occur during the period of record prior to water development in the upper Colorado River and should now be considered a cause for concern. Global warming coupled with planned future depletions will likely result in a greater frequency of such extended depleted conditions. At this point in time we do not know all the ecological ramifications of such events. We do know that tamarisk invasion on banks occurs during these periods leading to channel narrowing. It becomes increasingly hard to uproot these in subsequent years, the longer the root systems remain undisturbed.

Page 27, line 2: If coordinated reservoir releases were made in 1997, as well as 1998 and 1999, why are the results from 1997 not reported here?

Page 28, line 6: was this "fraction" released a large or small fraction, i.e., 1/10th or 9/10s?

Page 28, bottom paragraph and elsewhere in report: There should be consistency in reporting river locations in either river kilometers or river miles (or both together). Pages 8 and 12 use RK, whereas RM is used here.

Page 33, line 1: Is the intent here to say '…mostly due to lower flow levels **at the time of photography** in 2000 as compared to 1993..."?

Page 33: How is the change in channel complexity measured or reported in Fig. 14?
It looks like the reduction in percent area of side channels in the 15-mile reach is 0-70% depending on location. Increases in side channels was as high as 100% in another location. From an ecological perspective, loss of habitat in one location is probably okay if it is created or expanded in another location. However, net loss is a concern. It would be good if the net change in total area and net percent change could be reported for each of the 15-mile and 18-mile reaches. As written, whatever net changes there are trivialized by describing them as "small (<20%)". I would say that a 19% loss of side channel area in 7 years (1993-2000) is huge. Was any photographic analysis of backwater area done here, as was done in the earlier studies?

Page 35, first paragraph: The hysteresis effect described here and in previous reports is interesting because it tells when suspended sediment loads and concentrations are highest, i.e., on the rising limb of the spring snowmelt hydrograph. However, it would be good if the author could lend some interpretation to this here. As flows rise in the spring, are deposits of fines stored in the bed being released and carried in suspension until they are depleted from the bed sometime prior to the peak? Or is it because low elevation snow banks melt first and it is these local areas that supply the bulk of the washload at this time. Snow melt may be complete in these areas early and erosion of fines would taper off. The peak usually occurs when warm days and nights finally arrive in the high country and the remaining high-elevation snow comes down at once without a lot of attendant fine sediment. Some such explanation would be welcomed.

Page 37, last sentence: I'm interested in this discussion of sand in the bed. But I am unclear of the implications of the statement here. Does "…sand in transport…" refer only to suspended load, or does it include bedload sand.? What is meant by "…exchanges in equal proportions with the sand stored in the bed…"? I'm picturing sand moving along the bed, filling framework voids, but continuing to move. Deposited sand eroding out and being replaced by more sand coming from upstream existing in dynamic equilibrium. Am I misconstruing the author's meaning here? If so, why would the fact that the suspended load being made up of 20 % sand (the rest being silt and clay) have any relation to the fact that the bulk bed material including rocks etc. (clearly not previously in suspension) is also made up of 20% sand? The connection is unclear.

Page 39, Fig. 17-B: It is pretty hard for me to look at this graph and conclude that sediment concentration in the Gunnison peaks several weeks prior to the discharge peak. It looks like the peaks coincide.

Page 44, last sentences: Most of the sand in the traps must have been sand that came out of suspension or was traveling as bedload. Yet the traps were operated during runoff at flows in

many cases greater than 125 m^3/s; hence, the sand should have stayed in suspension according to Dietrich's relations and the author's calculations. So why did the traps fill with sand?

Page 46: Table 4: What does 'n' refer to? It generally refers to sample size, yet evidently refers to something else here. Some kind of key to the acronyms such as the footnotes for the observed/threshold discharges would be helpful.

Observations

Page 56, first 4 lines: Here initial motion is described as occurring at 50% bankfull. Elsewhere in the report, such as p. 61, initial motion is described as occurring at 2/3 bankfull depth. Am I missing something here?

Also, I was under the impression that initial motion is that discharge at which a few framework rocks somewhere on the bed begin to move in a sporadic, but not widespread, basis. But this definition seems to fit the situation here with the painted rocks where a few were found to move. Is the additional part of the former definition (not explained here) that this threshold is reached only when such sporadic movement occurs at 50% of the sites? Rocks at the painted rock location were beginning to move at flows 30% less than the initial motion discharge. Was this because this location was one of only a few (something less than 50%) where rocks were beginning to move?

Summary

Page 57, Fourth line of Conclusions: Again, the word "nonetheless" seems out of place here (see second comment under executive summary).

Also, why not report here what happened with the 1997 releases?

Page 58, End of middle paragraph: This seems to be one of the more biologically relevant points of discussion offered here. What happens to spawning substrate when sand goes from traveling in suspension to traveling as bed load as the flows drop below 5000 cfs? Pikeminnow spawn during the descending limb of the hydrograph at various temperatures above 16-18 C. Are sites selected for spawning those that will maintain sand in suspension longer than other sites? What happens to sand at spawning riffles versus riffles not selected for spawning? Pikeminnow in the Grand Valley generally spawn in July. Historic mean July flows (1902-1937) in the 15-mile reach were 7,212 cfs. However, during 1954-1989, mean July flows have been 4,341 cfs (Osmundson and Kaeding 1991). Perhaps spawning occurrence in the 15-mile reach is low because sand travels as bedload through potential riffle sites now during most years. Perhaps spawning in the 15-mile reach occurred more frequently in the past because this threshold was met more often. Just a thought…

Another thought: The sand traps were placed in the channel and monitored during runoff. It would be interesting to see how quickly they would fill if monitored during base flow. This might give an idea of the amount of sand traveling through the reach during the fish growing season when invertebrate production is important and may be affected by interstitial filling. Too

late now for this study, but in addition to the spawning issue, would perhaps provide a link between sediment transport and biology.

Recommendations

Page 60, Category A, Duration and Frequency: This recommendation has always been confusing to me. I admit its hard to come up with a better and equally flexible way to prescribe a flow recommendation. But it seems the frequency and duration is somewhat contradictory. The frequency is one of three years and the duration is five days **averaged over several years**. To meet the frequency criteria, you need to meet the threshold flow on average once every three years. In the other two years the threshold is not met (zero days). So when it does occur, it would need to last 15 days for the 5 day per year average to be met. Or does the recommendation mean that the threshold could be met for one day in one year out of six so long is it is met for 29 days in another year out of those six (5 days on average)?

Page 60, Catergory A, Purpose: I would add that fines deposited within framework particles within run habitats need to be periodically removed on a widespread basis to improve invertebrate standing crops. There exists literature that can be cited for support.

Page 61, second to last line in Category B: At 10,000 cfs "much of the bed" is **not** mobile unless I have misunderstood much of this. Would it be more accurate to say 50% of the bed is beginning to become mobile at 10,000 cfs.

Page 61, Category C, Duration: Add a spawning temperature component to the recommendation, such as "…late June to early July *when temperatures exceed 18 C*."

Thanks very much for the opportunity to comment on this excellent report.

D. Osmundson
December 2, 2005

RECOVERY PROGRAM FOR THE ENDANGERED FISHES OF THE UPPER COLORADO

DRAFT REPORT EVALUATION

REPORT TITLE: Channel Monitoring to evaluate geomorphic changes on the main stem of the Colorado River.

AUTHORS: John Pitlick

PROJECT NUMBER: 85

RATING SUMMARY: (check one)

☐ Accept X Accept after minor revision ☐ Reconsider after major revision ☐ Reject

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GUIDELINES FOR REVIEWERS

The attached report has been submitted to the Recovery Program for acceptance as final. The Program asks your assistance in judging this reports technical merit. Please include in your review both general and specific comments on the report=s technical merit, strengths and weaknesses. Either type your comments on the next page or reverse side of this form, or attach your comments to the form. Written comments, if legible, may be placed directly on the manuscript; alternatively, numbers may be placed on the manuscript to correspond to numbered, typed comments.

General Comments:

1. Scientific soundness
2. Degree to which conclusions are supported by the data
3. Organization and clarity
4. Cohesiveness of argument
5. Length relative to amount of information
6. Conciseness and writing style

Specific Comments:

Please support your general comments with specific evidence. Comment on any of the following matters that significantly affected your judgement of the report:

1. Presentation -- Does the report tell a cohesive story? Is a tightly reasoned argument evident throughout? Where does the report wander from this argument? Does the report address the objectives as presented in the scope of work? Do the title, abstract, introduction, and conclusions accurately and consistently reflect the major point(s) of the report? Is the biological significance of the results clearly stated? Are the objectives clearly stated? Is the writing concise, easy to follow, interesting? Are the findings well integrated with existing knowledge?

Presentation is good, writing is all of the above. The objectives were addressed as far as hydrology would allow. However, it is unclear whether the recommended discharges apply to the 15 mile reach, or the 18 mile reach, or both. I can assume they apply only to the 15-mile reach since that would be consistent with earlier reports, but you need to make that clear. However, the study was intended to cover both the 15- and 18-mile reaches, so why no recommendations for the 18-mile reach?

In the SOW, the objectives include:

1. Provide Channel monitoring to evaluate rates of channel change and geomorphic effects of coordinated reservoir releases and normal snowmelt flows.

2. Refine previous estimates of gravel and cobble mobility by verifying thresholds for coarse sediment transport.

3. Define the window of time of peak sediment delivery from unregulated tributaries.

4. Assess problems associated with fine sediment deposition and decreases in interstitial void space.

5. Compare and contrast effects of augmenting flows on the ascending and descending limbs of the annual hydrograph.

6. To develop a matrix which can be used by the coordinated reservoirs operations group to tailor reservoir operation to target multiple objectives of habitat maintenance and creation. The matrix will relate flows to the physical characteristic of the river need for endangered fish recovery an example of a similar matrix developed for the San Juan River.

7. Provide data on thresholds and durations of discharges that perform important geomorphic functions so that biologists can integrate this information with biological information and refine their flow recommendations as appropriate.

In your report, you have addressed 1, 2, and 7. You have partially addressed 3 – you defined the peak time of sediment carried by the main channel, but did not directly link that to peak sediment delivery by unregulated tributaries. On 4, the problems associated with fine sediment deposition were discussed, but not really assessed. It appears that you may have combined objectives 4 and 5 in the report. However, the effects of augmenting flows on the ascending limb were not discussed, only those effects on the descending limb. Is there an accompanying matrix as described in Obj. 6?

2. Length -- What portions of the report should be expanded? Condensed? Combined? Deleted? *Please explain how the study changed from a 4 year study (1998-2001) as specified in the SOW, and the time period when all the work was done, to a 7-year study through 2004, as it is described in the report. The only analysis presented that was done after 2001 was a pair of photos taken in 2000 and 2004 showing growth of tamarisk on a gravel bar. While this is interesting and potentially important information, I'm not sure it justifies calling this a 7 yr*

study. If additional field work was done after 2001, why are those data and analyses not included? Otherwise, length is appropriate.

3. Methods -- Are they appropriate? Current? Described clearly enough so that the work could be repeated by someone else?
Methods fine near as I can tell although I'm not a geomorphologist.

4. Data presentation -- Are the results clearly presented? When results are stated in the report, can you verify them easily by examining tables and figures? Are any of the results counterintuitive? Are tables and figures clearly labeled? Well planned? Too complex? Necessary?

See comments below

5. Statistical design and analyses -- Are they appropriate for the data and correctly applied? Can the reader readily discern which measurements or observations are independent of which other measurements or observations? Are replicates correctly identified? Are significance statements justified?

6. Errors -- Point out any errors in technique, fact, calculation, interpretation, or style.

7. Citations -- Are all (and only) pertinent references cited? Are they provided for all assertions of fact not supported by the data in this report?

8. Recommendations -- Are management implications identified? Are the recommendations technical sound? Are they supported by the results of this and other research? Would implementing the recommendations contribute to recovery?

The first two recommendations are consistent with previous reports. The third is a bit of a departure. Earlier in the report you note that '…there are clear limits on the potential uses of bypass flows…' [to augment peak flows] due to limits in storage on the upper Colorado River. Augmenting post-peak flows as well would then necessarily require more storage? But more storage would likely negatively impact peak flows. So, in a trade-off, which is more important? Could we do both if necessary without additional storage?

COMMENTS: (may be attached on a separate sheet of paper, if desired)

This report is a good follow up to earlier studies. It is unfortunate the hydrology during the study did not support a more thorough evaluation of geomorphic changes due to recommended changes in flow. Would you recommend further monitoring?

Some of my comments are questions needing clarification. Please clarify in the text of the report as well as in a response to comments.

Pg i. Para 1. No hyphens in species names. Cypha should be lower case. I think you should add bonytail to the list as they are being reintroduced in both reaches. And actually, the 18-mile

reach does not provide habitat for humpback chub (Gila cypha). They occur downstream in the Black Rocks reach as you point out in the introduction.

Pg i. Para 2. Delete 'Nonetheless'. Since it happened before the drought, it's not unexpected that it was done. (Also on pg 57).

Pg 1. Same comments as pg i above.

Pg 1. end of page. The parenthetical about razorback sucker using the reaches for similar reasons as the pikeminnow seems to imply that they too are drawn to an abundance of prey fishes; this is not correct since they are not piscivorous. However they may be drawn to higher biomass of invertebrates and other habitat features.

Pg 2. full para 1. last sent. Does the statement that diversions remove 14% to 30% of the native flow apply to both reaches, or just one? Which one?

Figure 2. darker lines are shown for RM 177.3 (A), and RM 162.4 (B). Any significance to that? If so, explain in text and in figure.

Pg 19. end of page. Insert period between 'rate' and 'The'
And top of next page, insert space between Figure and 8.

pg 26. suggest adding average values to table.

Pg 27. Fig 10. Can't read this graph –delete fill color.

Pg 28. I don't think its unfortunate that only half the annual flow of the Colorado River can be stored – presumably you say unfortunate because more storage would allow greater bypass flows in dry years. But that is ironic since the bypass flows are only needed because the natural flow has been diverted and diminished.

Pg 39. last sentence above figure. 'were' should be 'where'

Pg 41. last sentence. does the question '…is there a reason to be concerned about changes in the timing of the peak?' refer to the peak of sand transport, or the peak discharge?

Pg 42. 3rd line. Pikeminnow should be lowercase.

Pg 42-43. Discussion of sediment traps. There is no way to tell from the figure how the sediment in the traps changed over time within a year. You describe in the text how the distribution of sediment size changed over time, but it is not shown in the figure. Perhaps you could color code the lines by date, so all the sediment traps on one date have a specific color.
Figure – need to add a legend, with additional info as mentioned above.

Pg 56. last sentence above figure. 'show' should be 'shown'

Conclusions: last para. You state three major assumptions on which the recommendations are based. But these' assumptions' are more than just that – they are supported by data in yours' and others' studies. I suggest you rephrase this paragraph to emphasize that these statements are supported by data.

This review will be considered by Recovery Program staff and the Biology Committee. If you desire anonymity, please do not sign below. The Recovery Program sincerely appreciates your assistance.

NAME: Melissa Trammell DATE: 10/25/05

Author's response to reviewers' comments:

1. Comments from J. Lyons:

   The majority of comments from this reviewer were editorial in nature (mistakes in spelling, punctuation, etc.); these were noted, and the text was changed accordingly. The most substantive comment from this reviewer was a suggestion to more fully explain the differences in bed elevations shown in Figure 26. This was done (see figure caption).

2. Comments from K. LaGory:

   This reviewer likewise pointed out a number of mistakes in spelling and punctuation; these were noted, and the text was changed accordingly. Additional suggestions from this reviewer were incorporated into the report as follows:

   a) Page 10 now lists specific values of discharge in the 15- and 18-mile reaches at the time of the aerial photography.

   b) Pages 23-24: Graphs depicting changes in discharge due to coordinated reservoir operations have been redrawn (the same request came from all the reviewers)

   c) Pages 31-36: The section discussing seasonal trends in suspended sediment has been modified in response to questions and concerns from several reviewers, including K. LaGory. The response to comments from Jimmy O'Brien provides more detail on this discussion.

   d) Pages 50-53, flow recommendations: The reviewer questioned the basis for assigning specific durations and frequencies to each of the recommended flow levels. To address this concern, a table showing differences in flow frequencies for three separate time periods was added to the recommendation section. A discussion of the relevance of flow frequencies for different periods now precedes the discussion of the recommendations. The durations and frequencies of the recommended flow levels are based on the "recent" flow record from the Cameo gauge, using the period 1978-2000 as the basis for setting target frequencies (see Table 6). Other time periods are considered to be either unrepresentative of contemporary conditions, or undesirable from a geomorphic standpoint. The author recognizes that there are several problems in trying to set targets for flow frequency and flow duration in a managed river. As discussed in previous reports and in this report the magnitudes of various flows are assigned on the basis of specific geomorphic threshold, whereas frequencies and durations are assigned on the basis of contemporary hydrologic conditions.

3. Comments from Jimmy O'Brien:

   This reviewer provided a number of perceptive and thoughtful comments. The responses given below focus on the main points raised in the review:

   a) The reviewer noted that the objectives listed on page 3 do not correspond exactly to the objectives in the Proposed Scope of Work for Project 85A; another reviewer (Melissa Trammell) also commented on this, noting that the specific objective of

developing a "flow matrix" was not met. To correct this, the report now includes a flow matrix, which appears in the Executive Summary (Table ES-1) and in the Recommendations (Table 7).

b) The reviewer comments that sand-sized sediment in this reach is supply controlled, and offers a number of additional comments on the processes of sand transport. The text of the report has been revised to reflect a number of these comments; however, the author does not agree with some of the reviewer's interpretations. For example, there isn't much evidence in the Colorado River that sand is stored in pools upstream of the study area, as appears to be the case in the Green River; it seems just as likely that sand is stored in pools throughout the study area. The text of the report has been changed accordingly. Elsewhere, the reviewer comments that there is not much detail given concerning the source of sand. The author does not understand this comment, since pages 30-36 discuss sand in detail. The data presented in the report show than a non-negligible fraction of the sand carried by the Colorado River is derived from the channel bed (ultimately it is all derived from the watershed, but ~20% of the bed material consists of sand, which can only get there by exchanging with the material in transport). It is important to recognize this, because it affects how you approach the problem of channel-maintenance flows, i.e., whether transport rates are governed by the supply or by the hydraulics of the flow. The distinction between supply-limited and capacity-limited transport depends on the grain size. Nobody would say that the Colorado River is supply limited with respect to gravel; likewise, probably everybody would say that the Colorado River is supply limited with respect to clay (and maybe silt). Sand fits in between, thus there is a danger in generalizing about sand transport in rivers such as this.

c) Comment 4 focuses on the potential impacts of vegetation growth on channel narrow. This point is noted and now discussed in more detail in the Summary and Conclusions. Disturbance of vegetation is also given as one of the intended purposes of flows approaching bankfull.

d) Comment 5 describes the reviewer's own experience and observations of sand/gravel interactions; this appears to be more of a remark than a suggestion or concern.

e) Comment 6: The wording in the discussion of sand transport has been changed to reflect the concerns raised by the reviewer.

f) Comment 7: The reference to equal mobility has been dropped.

g) Comment 8 appears to more of a remark than a concern.

h) Comment 9: The reviewer suggests that the discussion of the equation for gradually varied flow is simplistic and unnecessary. It's not clear where the concern lies, thus the author has retained the discussion of this equation in the report, primarily for the benefit of non-specialists (see comments from Doug Osmundson).

i) Comment 10: Wording was changed.

j) Comment 11: This comment is primarily a suggestion for conducting field surveys. It is not clear that the timing of the survey measurements in this study introduced a significant problem in the analysis and interpretation of the data.

4. Comments from Doug Osmundson:

This reviewer provided a number of perceptive and thoughtful comments. Suggestions for changes in wording or context were incorporated in the report as necessary. The main points listed in the review were addressed as follows:

a) The wording was changed in several places in the report to distinguish between the different components of bed load- the movement of sand versus the movement of "framework" grains (cobble- and gravel- sized sediment).

b) The rationale for using a particular block of years to develop flow recommendations is given later in the report, in the RECOMMENDATIONS.

c) The first 8 comments listed under RESULTS are primarily requests for clarification and rewording; these comments were incorporated into the report as requested.

d) The reviewer asked for some clarification in the discussion of sand transport, similar to some of the comments from J. O'Brien. Several editorial changes have been made in the section discussing sand transport to address the concerns raised by the reviewers. Some of the concerns are open-ended questions; however, in other cases the wording has been changed to try to clarify important points, such as the transition between sand moving in suspension and sand moving as bed load.

e) The wording within the sections discussing differences between peaks in water discharge and peaks in sediment concentration was changed in several places.

f) Comment on accumulation of sand in traps: Sand accumulated in traps at all flows; this might seem unclear, perhaps because the suspended sand is envisioned as a cloud of particles moving along with the flow, never in contact with the bed. In fact, particles move continuously between the bed and the flow; if the system is in steady state, the number of particles being entrained by the flow is balanced by the number of particles being deposited.

g) Column listings for Table 4 were changed.

h) The two comments listed under OBSERVATIONS were addressed; however, it should be noted that there isn't a 1:1 relation between depth and discharge (see Fig. 25). The depth changes rapidly in the range from low to intermediate discharges, and more slowly thereafter; thus a river may indeed reach 2/3 of the bankfull depth at 50% of the bankfull discharge. Also, the definition of 'initial motion' is open to interpretation- there's always a chance that some rock somewhere on the bed will move, even at low flows. The criteria for initial motion used here is based on a specific value of the dimensionless shear stress, $\tau^* = 0.025$, which represents the point at which some of the framework grains on the bed are beginning to move.

i) The comment referring to page 58 of the SUMMARY appears to be more of a remark than a suggestion for clarification. The potential ecological effects of sand dropping out of suspension on the receding limb of the hydrograph are discussed earlier in the report, and listed as a recommendation for future work.

j) The RECOMMENDATIONS have been reworded to address the concerns raised by the reviewer.

5. Comments from Melissa Trammell:

Again, the review comments provided here were extremely thorough and helpful. Individual comments on wording/spelling were incorporated into the report as necessary. Other more detailed comments were addressed as follows:

a) The reviewer suggested that the report should include flow recommendations for not just the 15-mile reach, but also the 18-mile reach. This was done.

b) The reviewer suggests that some of the objectives listed in the scope of work were not addressed completely. This is a matter of interpretation, or perhaps a difference in how the objectives of the report were phrased. The only element missing from the draft report was the flow matrix. The flow matrix is now included in the final report.

c) Comment on the duration of the study: The reviewer questions why the report includes results beyond the period specified in the scope of work. It is not clear to the author why this presents a problem. The report is admittedly late, but there is no reason to restrict the information in the report to a specific time period, especially if it helps guide recovery program efforts.

d) Among the detailed comments, most all of the suggestions for changes in wording and/or clarification in figures were taken into account, and the text and/or figures were modified accordingly. Discussion of species location and use of habitats has been clarified; Figure 2 has been changed; average values were added to Table 3; figures showing changes in discharge as a result of coordinated reservoir operations have been redrawn; wording regarding reservoir storage and bypass flows has been changed (see p. 24); and the discussion of sand transport and potential ecological effects of changes in the transport mode (suspension vs. bed load) has be edited. The figure showing grain size distributions of the sediment caught in the traps is already quite complex; adding color lines w/ a legend for each line would make this an exceedingly busy figure, therefore, this suggestion was not incorporated into the report. Finally, the wording leading up to the recommendations has been changed.