

SCIENCE ADVISORS REVIEW OF THE GCDAMP 2013/2014 BIENNIAL WORK PLAN AND BUDGET

GCDAMP SCIENCE ADVISORS

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JUNE, 2012

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JUNE 18, 2012

INTRODUCTION

This review document is released from the Science Advisors to both GCMRC and the Technical Work Group to accommodate TWG member needs to evaluate its findings prior to the formal TWG meeting on June 20-21, 2012. A power point of the findings will be presented at the meeting as well as the final document.

The review incorporates review input of the Science Advisors as well as other reviewers selected by the SA Executive Coordinator. As in all SA reviews there are sections devoted to general review comments, specific review comments and recommendations.

The review is structured to provide SA specific recommendations to the GCMRC at the request of the new Chief. These include general and specific recommendations proposed by the SAs, specificity regarding why a particular issue is raised, concerns over a science or technical direction taken by GCMRC, and SA proposed recommendations for revision of the plan.

BACKGROUND

Science Advisor reviews have always responded to three types of input to AMP science and management plans and proposals; general comments, specific comments and recommendations. General comments are focused on overall concerns regarding the use of adaptive management processes, application of ecosystem management, research integration, science and management collaboration, general science program approaches etc., whereas, specific comments address specific approaches in a project or study, questions on sampling designs, hypothesis being tested, conclusions reached, etc. Recommendations can address general and specific comments and normally are directed at approaches to improve the science or management processes.

GENERAL RECOMMENDATIONS

- Sediment elements of the plan, most specifically Project B, are generally effective with respect to clarity of stakeholder input in defining program goals, objectives, key uncertainties, and information needs, as well as how these have resulted in short-and long-term strategic and operation level science direction. Within these broad projects, individual research projects mostly demonstrate critical knowledge of resource interactions and the inherent need to integrate research and monitoring projects from sample design to analysis. Similarly, individual project research designs are for the most part well articulated.

- A significant challenge exists in describing numerous individual research projects within a document of limited length, so selected sections could benefit from a brief expansion of information provided. This would assist managers and policy makers less familiar with the physical dynamics of water and sediment movement in the study area.
- The GCMRC physical resources program has an excellent record of responding to stakeholder and peer review comments in terms of continuing to integrate data gathering for individual physical resources research projects, integrate physical resources research with biological and cultural resources research projects, and focus physical resources research on addressing specific management needs. Review of referenced literature demonstrates consistent extremely high productivity in terms of peer-review publications, and effectively provides the physical science component necessary for adaptive management in the large and complex CRe within Grand Canyon.
- There is not an explicit science strategy for a comprehensive ecosystem science approach for the program in this report. Although individual project descriptions included to varying degrees some mention of coordination and integration with other projects and other resource programs within GCMRC, the report does not include any statement of overall responsibility for integrating within resource programs and between resource programs. Who will take charge of integrative interpretation and reporting for the numerous individual projects within the physical resources program (projects A-C), for example or among the physical, biological and cultural resources programs? Does GCMRC still have an ecosystem modeler on staff? It is clear that individual research projects will provide results useful and accessible to managers and stakeholders. What remains unclear is how effectively the individual project results will be integrated and synthesized, and who specifically will be responsible for this integration and synthesis, including the identification of continuing, over-arching research needs.
- Clarification is lacking of a strategic basis for this AMP proposal of science and management activities. A comprehensive Work Plan such as this must create in the first chapter a strong strategic basis for its overall direction, including stakeholder direction and policy direction, management and science approach and overall science design. The basis for overall management and science direction exists in the twelve articulated AMP Goals developed by stakeholders for implementation of the program (EIS 1996, USGS 2007). These have physical, biological and social resource orientation including fish, water quality, power, recreation, adaptive management, etc. As characterized then and in the GCMRC Strategic Science Plan of 2007, science and management direction is strategically tracked through objectives, information needs and science questions developed by stakeholder groups in collaboration with scientists and managers using an adaptive management paradigm (USGS 2007). That is, the AMP has defined the basis for program as pursuit of stakeholder developed goals, objectives, information needs, desired future conditions, etc., with the application of experimental policies and science. Although the document does not provide an introductory chapter that provides overall guidance on the system development of science and monitoring programs, some projects such as the sediment and water quality section do provide some of this tracking. The water quality section, Project B, provides the tracking that with an introductory chapter would be effective. It is not

clear if new GCMRC developed science questions such as in fish ecology are a new basis for direction of the science and monitoring programs apart from stakeholder direction. For example in the overview of the science program direction the following statement related to new research in fish ecology does not seem to give strong basis to stakeholder developed direction, “Although not listed explicitly in these project descriptions, consideration was also given to past recommendations and guidance that were part of the development of Strategic Science Questions, Research Information Needs, Core Monitoring Information Needs, and Desired Future Conditions.” If the GCMRC intends to utilize new questions in fish ecology and other programs as the primary basis for some of the science and monitoring direction this should be articulated in the beginning of this document. Section 3 of some project areas address how specific areas of study respond to past information needs and science questions (Project B specifically) but without referencing the overall strategic direction developed by stakeholders, policy makers and scientists, linkages get lost and studies seem disaggregated. In short this document should provide in a first chapter the basis for this program, i.e. stakeholder goals, objectives, information needs as well as the policy directions regarding changing program priorities.

- The paradigm of Adaptive Management has also been articulated well in many management and science documents as the approach for addressing the management and science needs of this program and other similar programs (Walters 1986, Runge 2011). Adaptive Management is not a minor commitment or a simple variant on management or science programming. It is a significant commitment to use aggressive management policy experiments to focus on the best opportunities to resolve stakeholder goals and objectives through assessments of the outcomes of management actions and applied science. The paradigm was selected because of the extensive uncertainty and complexity faced by this program. Clarifying this critical commitment and how it reflects on key science and management directions in this program over time should be part of in an introductory section.
- Just as little clarification is provided for the stakeholder and adaptive management basis for this proposed program of work the proposal to conduct science programs utilizing ecosystem science approaches is not addressed. In the introduction section reference is made to research and monitoring themes that have been introduced in the many documents cited, but there is no reference to an ecosystem science process that guides the current program now and through the next decade. The overall ecosystem science design is missing from the document. When one reads the specifics of various program areas such as water quality and fish ecology it becomes clearer that science integration is occurring and scientists are evaluating several parts of the ecosystem simultaneously in project designs. But, this program must address solutions across multiple decades with multiple sets of projects. That critical part of the overall ecosystem science design is not presented.
- In the sediment section reference is made in the 7 questions for Project A to determining pre and post sediment deposits. A “geochemical signature project element” is mentioned on pg. 25, first paragraph. In review of the project A.5 for using geochemical elements for accomplishing the differentiation of pre and post-dam deposits it is noted that preliminary work and proposed

new work is directed at determining whether the method can be used to resolve the relative proportions of pre- and post-dam sediment in a deposit that contains a mixture of these two sources. The project justification is that, "This research addresses what is perhaps one of the most important questions regarding sediment in the ecosystem: Is pre-dam sand still being excavated and exported downriver under present operations of the dam?" This question has surfaced before by stakeholders and scientists and it does have importance when managing at least in part a non-renewable resource. That is the river has lost 90% of its annual fine sediment input and uncertainty exists as to whether appropriate flow management can use the 10% remaining inputs to sustain CRe bio-physical and socioeconomic resources at a satisfying level for society. A primary approach to date has been to focus on developing effective assessments of long reach fine sediment mass balance in the system, that is determine the above and below water line volumes of fine sediment in the system as well as annual inputs and outputs, with refinement assessments in shorter reaches. Significant variance and uncertainties still exist in data and assessments in shorter reaches and river sites such as eddy. To date this approach has not pursued what portion of the transported sediment is of pre- or post-dam origin. The argument that we assume is being made is that there is a reasonable question as to whether the 10% input now available can maintain a net positive mass balance of fine sediment under any alternative flow regimes evaluated to date. If not and the data shows increasing percentages of fine sediment is of pre-dam origin then we are mining a non-renewable resource that has a limited life. Knowing the decay function of the available pre-dam sediments would be of value, but how much value? That is, if the current science approach can determine the total fine sediments available in the system is it critical to know what percent exists of each? To date the two resources have been treated as though they are interchangeable in their support role to other physical, biotic, cultural, social, etc. resources. If that is in fact the case and other programs are reduced to accomplish this project what is the critical gain to the AMP. A second question is the capability to make the determination sought and the related cost. This plan highlights in many sections the inherent variability in sediment data sets especially in shorter reaches and local sites. Given these issues regarding this project are the stakeholders in support of this question being," one of the most important questions regarding sediment in the ecosystem."

- The Project B presentation is most effective in linking the science and monitoring activities to stakeholder objectives, developing data for assessments to support management actions such as the mass balance work, providing the ecosystem linkages of this project to other projects and justifying the monitoring and research activities and expenditures including the progressive use of advanced technology such as laser-diffraction and acoustic technologies. Just as important the Project has focused on developing assessments and models and developing web based information access through the USGS Center for Integrated Data Analytics. Perhaps this project outline and documentation could be repeated in other projects to provide linkage to stakeholder needs.
- Project C, Lake Powell Water Quality Program, although seemingly rich in data, has not produced in-depth interpretive assessments that are common to other GCMRC sediment and biology projects. Modifications in monitoring approaches, improved data base development,

and greater accomplishment in analysis, assessments and publication were part of the recommendations of the 2001 cited Program Evaluation Panel. The Lake Powell and CRe water quality monitoring programs were also selected as one of the first resource monitoring programs for development and approval in 2011/12. Some improved data recovery methods have been employed, use of data for improvements to the CE-QUAL-W2 has occurred, and development and inclusion of more data into GCMRCs DMS has occurred. However, except for publications on the existing data minor accomplishment has occurred in data analysis and interpretation. Although Dr. Robertson was released from the Science Advisors program in 2009 to assist this GCMRC activity, that work is not proposed for completion until November, 2013. Accomplishment of this effort led by Dr. Robertson and others is proposed as the primary interpretive science effort for the long term Lake Powell program. It proposes a complete evaluation and interpretation of all physical, chemical and biological characteristics of the lakes water as compared to climate factors and GCD operations. Further it prescribes to verify and improve CE-QUAL-W2 for use in interpreting past changes in Lake Powell, and predicting future changes related to climate change. When one reviews the budget allocation for this program in 2012/2013 it is increased from \$182K to \$236K or approximately \$50K. Based on data given approximately \$25K of additional salary funds are allocated for this additional work. On the surface the increase in funding seems insufficient for the task expectation. It is not clarified if other funds are available.

- Significant concern exists with the level of documentation in Knowledge Assessments for this program. Originally Knowledge Assessments and SCORE reports at five year intervals were approved by the AMP as critical to track science and management accomplishments toward AMP goals, information needs, DFCs etc. The 2006 Knowledge Assessment was prepared but the draft was never finalized. The originally approved 2011/2012 KA was to be a formal USGS report, but to date the SAs have not received this report. This plan references science reporting in 2011/2012 KA workshops which included scores of power point presentations. Apart from diverse power points not being an acceptable surrogate for an agreed to formal KA report, it is impossible to use them for referencing current status.
- Native fish research in Project D.2.1. And D.2.2. Specify sampling of native fish for otoliths (natal origin) and egg maturation using various procedures. Due to fish ESA status the sample sizes are very small. No estimates of expected variance are given on the proposed procedures. Can the effectiveness of these procedures be determined from these proposed sample sizes in 2013? The issue is raised because of the relatively high costs of sampling. If expected high variances are associated with the procedures would requests for additional take of the species to increase sample size be more appropriate.
- There is significant variance associated with efforts to quantify native fish growth, age, survival and population parameters for all aggregations of adult fish in the CRe as a system as well as for adult fish and juveniles in discrete aggregations. The years and effort spent on improving adult population estimates in the system has obviously yielded significant knowledge, albeit significant uncertainty exists. Approaches outlined to incorporate all aggregations to reduce bias on population estimates appear prudent. Back projections with ASMR to gain inference on

early HBC life stages are failing to gain the specific learning needed. The new integrated research efforts on food base, juveniles, habitat, etc. seem necessary to gain greater interpretation of early life stage response to differing policy experiments, management actions and natural events. However, we are four years into these new efforts and rapidly discovering multiple tangents that must be pursued to make adequate progress in reducing uncertainty. And, overarching AMP programmatic questions exist that are rarely addressed in any programs pursued, and they relate to the AM paradigm to pursue those policy experiments and management actions that will create the greatest chance of gaining a positive management resolve and hopefully in an early time frame. The scientists have noted expected outputs of reports, thesis and publications from the presented research but there are no predictions made on when one can expect a resolve or answer to the stakeholder goal, objective or information need. This omission is not specific to the fish program, but it is more pronounced here because the program does not clearly specify the management or stakeholder objectives it is trying to resolve. The proposed program does define a series of research questions and hypothesis for reducing uncertainty, and they are definitive in 2013/14 for both addressing life stage and food base issues. Is it possible for the scientists to define more clearly over a five to ten year time frame the series of studies, replications and expected years necessary to determine the relative importance of three differing impacts to juvenile fish, i.e. main-stem temperature, food base and predation.

- The proposed fish science direction is emphatic regarding approaches to determining the trophic basis for production of fish, i.e. quantitative gut content analysis and stable isotope analysis. The two differing methods represent differing time commitments, costs and success probabilities. In the case of stable isotopes the scientists determine them to be infeasible generally but potentially possible in the LCR work. However, managers need more clarification of risks, uncertainties, costs, time frames, etc. for differing options. If this were possible, policy makers and managers could potentially develop probabilities and expected values for differing competing management and research directions. These decisions are very difficult with the current information.
- The overview of Project F.1. Does specify its relationship to Project D for both the sampling approaches and inference related to modeling. As stated previously it seems implicit that an ecosystem research design must be followed over 5-10 years to address the native/non-native interactions regarding habitat competition, food base needs, predation, etc. Yet, it is not provided. It should be provided in general terms in an introductory chapter for this plan and in more specific terms in the fish biology section.
- In all projects and subprojects for both physical and ecological resources the issues of high variability in data and assessments are mentioned as a critical factor affecting interpretation of analyses. In a few cases sampling and analysis errors are referenced, but for most projects they are not. No discussion is provided as to how high system variability is effecting time and costs to obtain the outcomes desired and their expected reliability and utility. Provided statements on learning, i.e. thesis, reports, publications, are important but the end goal is resolving defined

stakeholder objectives and implementing revised management actions to improve resources. This is not addressed adequately.

- Research in Project G on laboratory experiments project significant accomplishment such as follows. “This new capability will allow researchers to quickly and cost-effectively quantify relative impacts of rainbow and brown trout competition and predation on humpback chub”. Yet, in reality this research would seem to be used primarily in developing hypothesis that would take years to assess in the Colorado River environment due to high variability. Again it points to a need for ecosystem science designs that can more realistically project outcomes and expected benefit through a 5-10 year time frame. This could also help in refining science integration and direction and avoiding duplicity in the many projects.
- In Project I vegetation site specific surveys at random sites provide evaluations of vegetation change to the 45,000 CFS level, whereas the proposed corridor-wide database will evaluate vegetation to the old high water mark, i.e. 250,000 CFS. The comprehensive river corridor assessments are certainly important but no specific requirements or need is specified by the AMP for the larger data set to 250,000 CFS except possibly for consistency with NPS data sets. Is that the rationale?
- Project I highlights potential effects of tamarisk decline on the protected willow flycatcher and drift food base. Although integration with Project F is cited as a need for higher resolution data to extend the remotely sensed data, more detail is necessary to determine the full need for the more costly site surveys.
- The background information and justification for Project I effectively place the work in the context of current scientific understanding of riparian vegetation response to flow regulation. The fact that techniques use elsewhere along the Colorado River drainage network will now be applied in Grand Canyon is a plus. The overlap in sampling areas with the sandbar monitoring project is also good. As in Project A the combination of detailed ground surveys and system-wide remote sensing surveys is applauded.
- For Project I in addition to stratifying sampling sites with respect to geomorphic responsiveness, it could be useful to also characterize sites with respect to magnitude and frequency of recreational use (the GC River Guides Association should be above to provide information on sandbars that are consistently campsites for large commercial river trips). Campers and hikers do modify the riparian vegetation to some extent, despite the NPS prohibitions against doing so.
- Some confusion is created by the proposed two part cultural resources program in Project J. This program proposes first through research activities to refine the appropriate level of precision, accuracy, and spatial scope required to monitor the condition of archeological resources in the CRe. Second it proposes to develop site field measurements, remote sensing and geomorphic modeling to determine the long-term stability and erosion vulnerability of sand deposits defined as protecting the archeological resources. Over the past two decades the cited work by Hereford et. al. (1991, 1993, 1996) isolated and assessed the potential effects of operations on gully erosion and Draut and Rubin (2005, 2006, 2008), and Hazel and Others (2010) addressed operation impacts of changing sand bars to aeolian sand cover on select archeological sites. However, in these tests of hypothesis issues of total impact or expected

benefit from management actions were not evaluated. Based on this and other research, efforts were extended by both research and the AMP to develop a programmatic program for mitigation including specific sites and proposed activities. This program was apparently agreed to by the AMP since it was initiated and funded. Funding appeared to have lapsed but was to be reinstated. But it appears that now a new program of research and monitoring is being proposed to replace the previously supported program. No reference exists in this plan to the above work or the TWG and AMWG in implementing the above noted research and monitoring program. The program that is proposed appears to specify appropriate methodologies. However, some explanation needs to be provided on why the AMPs long term investment in and implementation of the above noted program at a very high cost is no longer referenced. Has it been terminated?

- The background information for Project J effectively summarizes the importance of monitoring erosional change at archeological sites; reviews work to date in the study area, and identify information needs that will be addressed by the proposed research. As with other projects, the combination of more detailed monitoring at limited sites using ground-based LiDAR and other techniques, and system-wide monitoring based on remote data is likely to be effective in addressing research questions. The sequence of events through time described for gully affecting an archeological site represents the type of channel adjustment described in channel evolution models (Schumm et al., 1984; Simon and Castro, 2003; Simon and Rinaldi, 2006). These models have been used to predict channel incision and subsequent adjustment, typically following base level fall, and application of the models to gullies in the Colorado River system might provide some useful insights for management.
- In Project J it is surprising that there is no mention of how the archeological sites to be monitored do or do not overlap with sites to be monitored with respect to sandbars and riparian vegetation. Given the high likelihood that changes in base level, sediment supply, and sediment stabilization by riparian vegetation influence stability of archeological sites, it would be useful to understand whether sites chosen for their archeological value have any geographic overlap with monitoring sites chosen for other reasons, and whether the results of sediment and vegetation monitoring can be directly related to specific archeological sites. Project J.3., in which supply of aeolian sand is considered only in relation to valley geometry and not in relation to riparian vegetation extent is an example of the surprising lack of coordination.
- Project K does not provide any program information but cites the need for an economist to address programs specified by AMWG, TWG and various committees. The SAs prepared a response to the proposed economics program in the 2011/12 Biannual Plan. In that response the SAs agreed GCMRC expertise is needed in economics to respond to AMPs socioeconomic program requests. The SAs proposed this expertise be gained through short term consulting contracts. An economics specialist on a term appointment could accomplish the same objectives. However, although extensive efforts over the last 4 years have been dedicated to specifying a socioeconomic program, none is presented. One would expect some description of program requirements to proceed hiring of a program specialist.

- There is a lot of overlap in sampling efforts of the fisheries program and to a lesser degree in the conceptual basis for this effort. In my opinion some of the sampling programs could be combined in creative ways to give the same answers with less sampling (\$) and less handling of the fish.
- Importance of an integrative approach. Any large and complex project such as the work plan reviewed here must inevitably subdivide into components. Hydrology, fish biology, and primary production are examples of such components in the Glen Canyon Dam Project. But success in accomplishing the goals of the larger project also requires substantial effort to integrate the work plans and analytical efforts through a coordinated conceptual framework that ties together the disparate projects. No single pathway will always work to accomplish this goal. But any program without a concerted effort to develop that framework and then weave the parts into an integrative whole is likely to fail. It could accomplish its goal, if they are lucky, but probably with glacial speed. Perhaps an effort has been made to weave the disparate programs together but that is not obvious from the document reviewed here.
- The need for a conceptual model of the study system. A crucial component of that weaving process is development of a conceptual model of the system as a whole, including explicit discussion and framing of the nature of relationships among the components of the program. It is easy to say that the hydrological models will be important to understanding of the biological dynamics studied by the biology groups. But if those groups are not communicating from the start, it is unlikely that the output of the first component (hydrology) will provide the most appropriate information to ensure the success of biological studies and analyses. What is the most important hydrological information to understand biology? In recent studies of the effect of flow on stream biota in Puget Sound lowlands, it was found that the usual hydrological parameters are not very relevant to understanding the effects of landscape change on stream biology (Morley and Karr 2002, Booth et al. 2004). Standard hydrological measures of peak flow, for example, were not well correlated with biological condition; in contrast, the fraction of the year that daily mean discharge exceeded the annual mean discharge ($T_{Q_{mean}}$) was strongly correlated with biological condition. We are not suggesting this measure is likely to be important for the Colorado River. But we are suggesting that creative thought about biologically important hydrological measures might deserve more exploration in an effort to create a conceptual model and define relevant parameters for measurement that will help to integrate across the project components. Recently a National Research Council panel evaluating the St. Johns River Water Supply Impact Study offered comments that may apply here. These comments to the St. Johns River Water Management District (Florida) are in line with the two points just made. That panel issued four reports with the following messages.
- Carefully plan study design from concept to data collection and analysis. The nature of this kind of work plan often means not enough information is presented for the reader to understand the study design, sampling program, and analytical approaches to be used in extracting insight from the collected data. As a result, one cannot tell whether or not those planning steps have been taken to ensure a robust and properly focused study program as well as an approach to analysis that will be both effective and timely. Results delayed for years preclude mid-course corrections

for far too long. And mid-course corrections are crucial to any effective adaptive management program for management or for research designed to provide results that will provide insights required to improve management over the short term. In terms of adaptive management employ a simplistically framed four step process: plan, do, check, adapt. It may not be practical to have all the details here, but at the very least a report section should sketch the nature of the collaborative planning effort that led to the work plan outlined in the document.

- What quantity of data is needed to make robust decisions about management situations? Any long-term study should explicitly include in the effort a plan to determine how many samples are needed, what seasons, what habitats, what taxa, and so on. Scientists tend to collect massive amounts of data and too rarely ask what proportion of those data is actually needed to understand patterns and make informed management decisions. A couple of the comments below deal with this issue.
- This is a very large project that has been going on for years. The proliferation of acronyms and the abundance of cited reports, publications, work plans, and so on make it very difficult for a person not familiar with all those documents to truly understand what has come before. And, of course, all that came before cannot be repeated here. That said, enough should be in this work plan to understand the four issues just mentioned above and how the program has handled and will continue to handle these important topics. I am not convinced the current plan meets the minimum standard to show that these four issues have been adequately dealt with.
- As a new reviewer of GCDAMP, a perspective can be quite limited. As a result, an overview of the GCDAMP plan is critical for providing an integrative view of the a) motivations, b) approaches, and c) desired outcomes of the plan. The overview lacked a clear structure for presenting these three key elements, though most of the information was there. It might benefit GCDAMP to revisit these major structural issues in light of efforts and achievements since the inception of the research program, as the tone of the overview is more like a progress report for a program assured of high-level funding than a statement of a vision for what could be achieved with \$10M.
- It was striking that an experimental approach to adaptive management was not highlighted in the overview. Yet that approach should be central to GCDAMP. Instead, the overview reads much like an agency monitoring plan for any large river. We have great respect for the value (and cost) of basic monitoring, but the expected use of that information to resolve pressing management and research questions should be stated more clearly. We suggest reorganizing the overview around the opportunities provided by high-flow experiments, the information needs of stakeholders, and the ways that monitoring and research results can guide management actions in the key areas identified. As it stands, the problem statements are scattered, creating a fuzzy sense that the work is relevant but little reason for enthusiasm that any of the challenges can be overcome. We see this largely as a problem of presentation, not content per se. The coverage of the history and guiding themes at the outset was succinct and effective, but then the overview became less coherent after that point.
- A chief concern about the outline of the 10 projects is that there is so much focus on sand transport and just two species of fishes. Though sediment transport is clearly an essential

concern in the GC ecosystem, and the two focal species of fish are integrators of high conservation and utilization value, that level of focus still feels limiting for such a large and complex system. It is striking that only one riparian project is being pursued, the cultural/archaeological project is basically an extension of the hydrology/sedimentology section, and the combined budgets for riparian and cultural/archaeological work are a pittance compared to fish and sediment transport. Being new to GCDAMP, one is surprised by that ordering of priorities relative to the scope for corrective interventions. Sediment transport and fish populations are notoriously difficult to manage in large ecosystems with profound physical alteration, while riparian vegetation and archaeological sites seem more practical targets.

- The overview failed to link the proposed research projects to an adaptive management policy experiment framework, leaving uncertainty as to whether that core objective is being met. Instead, it described routine monitoring of a program. This suggests that some unique opportunities are being missed in GCDAMP, and that the research effort lacks a vision of solutions for the major problems facing the ecosystem.

SPECIFIC COMMENTS

- Page 12, first full (and long) paragraph: Many general points are made in this paragraph. **We** assume they will be expanded in subsequent material. Some questions arise in view of the comments here. How will knowledge of production be used to interpret fish population dynamics? Does higher production of algae and invertebrates lead researchers to assume that fish populations should follow the same pattern? Why or why not.
- Page 12, end of page: This integration of information across aquatic and terrestrial; plant, invertebrate, and vertebrate components; and among spatial scales is important. Also very important that some kind of overarching model be developed to establish some expectations that lead to hypotheses that are then explored. After reading the report, I do not recall seeing any careful formulation and framing of those relationships through an integrative model at verbal or quantitative levels.
- The very large spatial and temporal variability in sediment dynamics along the Colorado River, combined with the very large size of the region, and difficulty of physically accessing sites, creates a need for monitoring at multiple spatial and temporal scales. The report generally is quite effective at providing background information based on previous results and the importance of continuing work. It would be useful to very briefly reiterate at the start of Project A the idea that fine sediment can be stored along the submerged portion of the channel and then brought into suspension and deposited along the channel margins during high flows, although we have limited knowledge of the amount of sediment stored along the bed and the conditions under which that sediment is brought into suspension.
- The report effectively identifies and differentiates core monitoring and research information needs addressed by the projects.
- The multi-level approach of a subset of sandbars with annual surveys and repeat photos, plus aerial overviews of the system every 4 years, and channel-wide surveys of river segments on a rotating basis of 3-10 years appears efficient and effective. Are the photos and annual surveys

conducted at the same sites? Why or why not? Why have 50 ground survey sites and only 30 repeat photo sites (presumably cameras are cheaper than ground surveys, so why not have more cameras)? It would be useful to justify the location of the 50 sites-do they represent simply a continuation of existing data (to create longer datasets), or has the distribution of sites been re-evaluated in light of increasing understanding of sediment dynamics along the river?

- In Project A.1.1. Are these the same sites that have always been monitored? What about re-evaluating whether these are the most appropriate or feasible subset within the canyon? If the length of continuous record at these 50 sites is a prime criterion for their selection, note this explicitly.
- For Project A.1.3. the description is not as clear as those for other projects. It is not clear why the existing campable area metric is not sufficient, for example, and thus why a new project is needed. Why can't this be part of A.1.1. or even A.1.4.? Perhaps there is sufficient justification, but if so it needs to be better explained in the report.
- In Project A.2.1. it would be useful to explain the rationale for greater detail to RM 87 and lesser detail downstream. The proposal for annual data collection regardless of HFEs seems more appropriate.
- Project A.3. describes a statistical analysis of observed eddy and sand bar characteristics to predict sand bar response to changing discharge. It is questionable that this will work because presumably inflowing sediment concentration and flow duration are also very important in controlling sand bar dynamics but is not clear that these will be included in the analyses (the text mentions using metrics derived from computational hydrodynamic models, but this is very vague). Even though existing attempts to numerically simulate dynamics have not been successful (cited Logan et al., 2010 report), this empirical approach should be combined with a modeling approach. Project scientists should explore collaborations with other modelers(for example, those involved in computational fluid dynamics) beyond Nelson's group.
- Project A.4. does not include a very convincing description, partly because the site and methods are left open-ended. It would be more effective to briefly summarize observed relations (e.g., coarser sediment=less suspension?) and propose a conceptual model that explains why area immediately (2-4 km) upstream from a gage site influences suspended sediment characteristics. The stated hypotheses are a little vague in that they postulate relations but not the direction of those relations.
- Project A.5. is quite important to understanding sediment dynamics with the study area, but the project description in the report is as vague as to be largely useless. What exactly will be done? How many sample sites? Hypotheses? The brief description provided is completely unclear and needs much more detail.
- The sediment program in project A has two major thrusts. One is to determine over long time frames the most favorable flow regimes to support sediment and other multiple often conflicting resource desired future conditions. To monitor and assess the effectiveness of the first challenge GCMRC has chosen to develop assessments and models of mass balance of fine sediments for the entire system from extensive water quality monitoring data in Project B. Knowledge created has determined definable variance in sediment mass balance as well as

other resources when the system is subjected to differing flow regimes. A second major thrust is to assess impacts of the various flow regimes on the interacting dynamics of water and sediment and other resources in short reaches and under different river geomorphic conditions to aid in modifying the regimes. Both programs have significant accomplishments over the last two decades. In 2013-14 the program and its costs are expanded. Has a cost effectiveness assessment been accomplished recently of the programs to determine if management and stakeholders require this level of monitoring and science findings or if there is another level of accomplishment that could fully meet management needs with cost savings.

- An issue is identified from a Knowledge Assessment workshop related to vegetation and sediment interactions, and specifically loss of camping beach area to vegetation and changes in tamarisk vegetation due to the introduced beetle. This issue is pursued in two of seven presented sediment questions (pg 24). Project A.1.1., in part, is designed to provide necessary assessments. However, insufficient information is provided in A.1.1. to evaluate the expected accuracy of outputs or benefit from the remotely sensed data, except that it will differentiate vegetation from other land forms. For this approach to be effective it would have to differentiate tamarisk, both with and without leaf cover from other vegetation types. Has this level of capability been demonstrated in pilot studies? Will there be an integrated analysis on sites with traditional ground based surveys that would permit assessment of remote sensing effectiveness. It is eluded that ground based surveys would be used for calibrating remotely sensed data models.
- Project B is one of the best articulated programs in the plan, appropriately tracking stakeholder needs to science direction, addressing integration, etc. The development and continuing refinement of a suspended sediment budget for the CRE, of which this project represents a continuation, has been one of the most important and successful components of the physical resources research program within GCMRC. The introduction to this project effectively identifies the core monitoring and research information needs that the project is designed to address, as well as the integration of project results with other resource monitoring programs of GCMRC. The first paragraph of section 4.1 would be an appropriate place to better justify Project A.4.
- The Project B proposal for developing user-interactive web tools for the data is applauded. In addition to making the results more accessible and understandable to stakeholders within GCDAMP, the results will be accessible to the public at large, which will be excellent educational tools for university classes.
- Extending the existing 1d sand routing model to downstream river reaches, and enhancing the number of comparisons between the model and field measurements is an excellent component of Project B. These extensions are very important to being able to understand when sufficient tributary sediment inputs have occurred to justify the occurrence of an HFE.
- Although the background information and justification for Project C is generally good, it would be useful to specifically identify core monitoring and research information needs addressed by the project, as was accomplished in B. Past productivity appears satisfactory.

- Page 22, first full paragraph: From experience in small streams, hydrologists often speak of various issues (USP, pools vs. riffles, etc.) and how they change the dynamics of erosion and deposition as a function of flow (low vs. high flow). Is that set of ideas not relevant to this system? And is there equivalent theory that might apply here?
- Page 22, next paragraph: The key point here is that there are models but they do not deal very effectively with sandbar formation. What kinds of things should be done to fix or overcome that problem?
- Page 23, First sentence of section 3.2: Key sentence here. Not sure the next section of text really plays out the plans of what will be done in the future to change that reality.
- Page 23, Second sentence of section 3.2: How will managers know when they have achieved that goal? What benchmarks or index values can be established to recognize when progress is made or when the task is completed?
- Page 23, end of second paragraph of section 3.2: Key assumption seems to be that the tributaries must supply all the sediment needed to meet the needs of the system. Can the tribs be expected to yield what is needed from their current more or less undisturbed state? Or will disturbance have to be generated in those tribs in order to ramp up the sediment supply to keep the sand bars active and present? This seems very important as a mechanism to understand what is needed and if replacement is possible.
- Page 24, first sentence of second paragraph in section 4.1: The dichotomy between above and below water sandbars/sediment is intuitively appealing but I wonder if it leads to simplification that is not as good as it should be. The dichotomy may be important to humans but many other creatures and the hydrology itself may be more intimately tied to the details of below water level distribution of "sandbars" that alter flow at levels below the water surface. That is, the dichotomy may be more appropriately considered as a continuum, especially with the great diversity of areas below water surface. Has this been considered?
- Page 25, paragraph below the table: In the case of eddy modeling, how far and up and downstream of the eddy is considered in efforts to model the eddy itself? Hard vs. soft banks, curves in channels, depth of areas, angles of flow, and so on some distance from the eddy defined narrowly seem like they would be important to understand something of the diversity of eddy behaviors. Is this being considered? Could it help to focus on important eddy contexts for modeling beyond local eddy shape or size? Perhaps this is already being done but it is not clear.
- Page 29, second from bottom paragraph: Good but very brief summary of the study approaches. One might conclude that none are great, all are equally bad. Where does one go from here? How is the future work helped by what has come before? Can something more be said about the lessons of this for the future?
- Page 31, next to last paragraph: Only two dimensions of biology (aquatic primary productivity and the very vague "aquatic habitat") are included in this conception. What about taxonomic or other diversity? Are we to conclude that higher productivity is better? What components of habitat are considered? Why those and not others? Aquatic habitat is so vague. Habitat for whom? What dimensions of habitat (physical, chemical, biological)? And so on? Too often

habitat is used as a catch-all phrase without careful thought of what is really meant. It too often means everything and thus can mean almost anything from nothing to everything, from well conceived and defined to not at all defined. What is the situation here?

- Page 32, first paragraph: last couple sentences in the paragraph: And what hypotheses about the array of factors that may be responsible have been developed? How has this thinking helped to define what is included as of likely importance as a measurable variable? It seems a long time has passed before the level of thought suggested in the last sentence was initiated? Is it being attempted now in a rigorous and systematic fashion? Or is it a fishing expedition? Perhaps it has been systematic but one can't tell from the text provided here.
- Page 32, second paragraph, last sentence: The phrasing here "what aspect of channel geometry" leads the reader to assume only one factor is being sought. Why not "aspects?" Seems very unlikely that there is a single "aspect." Isn't it more likely the result of multiple factors acting in complex ways? The task is to understand the set of factors and how and where their relative influences vary with other things such as the comments above re eddy dynamics.
- Page 32, next to last paragraph: Some of the obvious examples of relevant factors are mentioned here. But it is still not systematic and the effort to show how those things will be used is not very comprehensive. Other things that come to mind include hardness of banks and bottoms, angles of channel flow along the thalweg, and so many more. Have these and others been considered in a systematic and comprehensive way as opposed to a laundry list of possible important factors. How will they be systematically investigated? How will the selection of sites and design of sampling be formulated to increase the likelihood of success? Which of the many may not be attacked effectively in a study of the systems here, given the configuration of eddy situations, time and money available, and so on? In short, what has advanced planning done to improve the probability of project success?
- Page 34, bold statements near bottom: Very important question. Again, how will this be determined? What kind of experimental or sampling design will be employed to ensure solid results?
- Page 67, first paragraph: Many things proposed here but not enough detail about exactly what will be done, where, when, and why to really provide any useful judgment about the merit of the decisions. I realize this is just the project summary, but as in other places, I have not found very much detail in the subsequent discussion of a project to really understand the underpinnings and components of the study design and analysis that is planned for the effort.
- Page 67, first paragraph of background: What are the sites, how were they selected, what is the design context, and so on for these multiple levels of sampling. If this isn't explored and explained in the next sections, it should be. This is the standard set of parameters measured in these kinds of situation. Much of the money expended in these efforts may bear little fruit that is useful over the long term. If that is not true, it should be possible to show with historical data that the current suite of things planned for measurement actually will provide useful insight. Perhaps most important, here and elsewhere from hydrology to water quality to primary production to age structure of fish populations, the suggestion is that all of these studies fit together to lead managers and stakeholders to cumulative wisdom for management in the

future. Yet, little in this report demonstrates how these various studies are being fitted together (integrated) in useful conceptual ways to reinforce and aid the work of other projects.

- Page 80, second paragraph on “synthesis of existing aggregation data”: When will this analysis be completed? How important is that to the careful design and planning of work expected to continue or be revised here?
- Project D: Well presented, good science, nice range of approaches, relevant questions.
- Table 3: Have these data really been mined fully? There is a lot here to work with. Comparing the 1993 abundance data (Table 1) to the mark-recap data (Table 3), there is great variation among sites in the ratio of mark-recaps to abundance, suggesting that some populations have been fully tagged while others are scratching the surface. Can these disparities be used to estimate population sizes.
- The mark-recap data clearly show two centers of distribution, also agreeing with Table 1. It would seem then that identifying the dispersal pathways between LCR and HGG should be a priority--we know they are connected based on mark-recap, so what are the key stopover points, and what can be done to ensure that these sites remain available? Focusing non-aggregation sampling efforts on the LCR-HGG connection points would address this issue.
- These aggregations are intriguing--what physical or biological characteristics predict their location? Have they been profiled carefully, especially the sites not associated with LCR? The mark-recap data strongly suggest that many “aggregations” (e.g. LCH) are merely pass-throughs, not stable population centers.
- p83 below Table 3: Need to explain how two nights of sampling allows BOTH closed population estimates AND capture probability estimation. The first is obvious; the second is not if it is to be a parameter independent of the first. The rationale is intuitive, but the statistical approach and its rigor need to be outlined.
- p83: “...colonize new habitats...”: This is key, and great that non-aggregation sites will be surveyed. What is the level of confidence that there are not and have never been fish present beyond the aggregations? Given the movement patterns documented by mark-recapture, can we gain any insight into dispersal pathways e.g. rate of exchange is highest between LCR and HGG sites, so there must be a corridor connecting them that is more easily traversed than other such corridors.
- p83 bottom: Odd that estimation of HC abundance at non-aggregation sites is not listed as a product. Even if the fishes are simply moving through, it is important to know where else they can be captured.
- p84 - This section is begging for a long-term record of water temperature, to evaluate how often the 16C threshold is reached at each of the aggregations. That must be available?
- p84 - Are there no observations of gravid female HCs during previous sampling? Have museum specimens been examined to assess phenotypic indicators of spawning condition? Results of Kaeding et al 1990 TAFS 119: 135-144 suggest that breeding in Upper Colorado occurs in mid-late June, so why not do some surveys during that time window (or the equivalent thermal time below the dam)? Kaeding et al found expressible gametes in both males and females, so this should be an easy approach to resolve the question.

Also, use implantable temperature loggers (perhaps as an ultrasonic tracking tag) to ascertain the actual thermal regime experienced by these fish. They are surely good at finding the right thermal habitats, if they exist.

- D2.1 - Excellent choice of method, but some pilot data should be shown to prove the stated ease of differentiation, and capacity to resolve multiple different tribes. Water $\delta^{13}C$ and $\delta^{18}O$ can vary widely through time, particularly in arid landscapes where groundwater vs. runoff mix varies seasonally. So robust inferences will require some seasonal work. Nice use of surrogate species to establish system baseline comparisons, and that will also allow evaluation of seasonality.
- Why not also use trace elements. Would be a worthwhile investment to assess whether additional resolving power could be achieved, as they can be more definitive than C and O isotopes in some settings. Be sure to keep otoliths from any incidental mortality of adult HC.
- D2.2 - This doesn't seem like a compelling first step. Why not sample HC at the right times of year, use thermal monitoring, etc. instead of focusing on manipulations and lab comparisons that are of dubious relevance to field circumstances. The ultrasound approach is appealing, but better as a secondary rather than primary approach to addressing the data need.
- Project E: Early life history: Overall assessment: Poorly presented, not convincing that any element other than the first will be particularly useful, annoying level of repetition, suspected weak return on investment
- P92 – Regarding back-calculated estimates, needs to be clearly stated whether there is a signature in the adult pop age structure of the low recruitment during those years? If not, it suggests that YOY survival is not the key bottleneck alleged here, unless it can be shown that the imprecision in back-calculation of survival is as high as to swamp interannual variation in recruitment (seems unlikely if recruitment failure is observed in some years).
- p94, H1: Are we sure that adults are spawning in these low recruitment years? That seems a reasonable first hypothesis, given the known variation in thermal regimes across years, and temperature-dependence of reproduction
- p94, H4: Flood years may also just dilute the population or otherwise reduce sampling efficiency, giving the appearance of low recruitment/few juveniles. Again, is there clear cohort evidence that these were really bust years?
- E.1: Are hoop nets an efficient way to capture these small individuals? would seem the mesh size would not be appropriate if the nets are designed to catch adults and minimize drag in river flow
- p96 : Mass emigration from LCR hypothesis: do the bulk catch rates support the inference that lots of juveniles are there in July, but few in fall? Or is it just the low-N mark-recap effort?
- P96: Temperature and trout are presumable directly correlated, yes? Please clarify how the two would be disentangled, as implied by the phrasing here.
- E.2: This element is all over the place. Really needs to be restructured to clarify what issues are being addressed and why the proposed methods are worthwhile. The toxicological component was the only one that was at all convincing and only because it constitutes a survey of a fundamental pattern rather than an attempt to draw big-picture inferences from shaky data. All

other aspects of this section are valuable for basic understanding, but the case was not compelling that they would allow any real questions to be resolved. In particular, the diversity of hypotheses for controls on juvenile performance is only indirectly assessed (at best) by this diverse suite of methods. Bottom line: it is hard to have confidence that these data can be stitched to tell the story that the applicants are interested in.

- P97: Components of energy flow estimation: this set of shaky estimates will yield an even shakier overall estimate of energy flow. The authors must at least recognize the spatiotemporal variation in most of these factors, and the difficulty of constraining all of them well enough to have a meaningful estimate of production potential in the end
- P97: Extrapolating fish consumption rates: but aren't good population size estimates for these fish species very hard to obtain (thus the motivation for much of the GCDAMP work)?
- P97: Contaminants: Why start with fishes instead of their food resources? Will contaminants be used as a tracer of trophic pathways, or as a stressor in their own right?
- P98: Isotopes: N isotopes will not be informative of prey N content for carnivores. How will the C and especially S isotopes be used?
- P98: Comparisons to other species: How about just doing some simple modeling of how food resource changes vs. competition changes could affect juvenile performance in HC? That might be a better starting point.
- P100 top: The food quality case is weakly developed, at best
- P100 toxic metals: It is not clear that the applicants realize how much work it will be to conduct these analyses well, and draw inferences about bioaccumulation pathways. Not all of these elements are likely to bioaccumulate at the same rate, but that is not mentioned. Is there any evidence of levels of these metals that are toxic to fishes themselves? Has Hg and other metals never been analyzed by state authorities regarding the major trout fishery?
- E.3: This project component is poorly specified; more detail is needed on the age and spatial states that will be modeled, and the robustness of the approach for estimating different life history parameters. The specification of the 'deterministic models' is even less well developed. Overall, this approach seems useful, but is poorly presented.
- Project F: Native and non-native fishes: Overall: All components are reasonable, basic monitoring that should be continued. A few more details would have been nice. It would be very helpful to show some of the historical data. There is a major risk with such monitoring efforts that no one ever circles back to evaluate the patterns carefully after collecting all that data. In this case, it would help to justify continued efforts if we were shown time series of past results under each section, rather than just stating the year range of monitoring. Costs seem pretty high.
- P106 summary: Summary includes no statement of what will actually be done!
- F.6: Natal origins are difficult to assess with mark-recap since the fish have been alive for quite a while before they can be marked by most standard methods. Perhaps apply the otolith microchemistry method from D2.1?
- F.7.2: This is a really great idea. Have the guides already signed on?

- F.7.3: Primary production will be analyzed by what method? Presumably sondes measuring DO fluctuations, but it should be stated clearly. There is no statement of methods to integrate the 5 spatial estimates, either. That can be very complicated to do properly for a large river.
- Project G: Trout interactions: Overall: This is a nicely written project proposal, but I remain very skeptical of the value of the lab assays for understanding the patterns in the natural river. However, the trout removal opens all kinds of exciting possibilities to gain field-based insights.
- G.1: The value of this lab approach for understanding predation and competition in the field is dubious at best. The species substitution is just fine, but the conditions in these lab experiments can't come close to capturing the actual controls on the interactions in the field. This must be acknowledged, and there is need for an explanation of why targeted sampling of trout in the field could not be used to assess their actual foraging patterns with respect to experimental flows or natural fluctuations from the monsoon. That would be far more relevant.
- G.2: How about sampling juvenile fishes and inverts in the drift? 20 d of trout removal with 25% efficiency on first pass should be enough to see effects on drift of inverts and little fish pretty quickly. That would be an informative way to leverage this huge effort. It is essential to interface well with the other GCDAMP teams--this removal represents a huge opportunity for integrative research relevant to management as well as fundamental ecosystem science
- Page 116, end of second paragraph: This paragraph deals with some issues at the heart of very important questions for scientists, especially biologists: how to decide how much sampling is needed. Is this really practical and even necessary to sample all habitat types? I agree that the decision to only sample the most productive ones can mislead us. Wouldn't it be just as useful to have focal sampling on a smaller range of microenvironments but with greater precision of parameter estimates? That leaves the question of how to select the focal habitat/microenvironments. The project leaders here have decided that it is not necessary to sample in all seasons, a wise decision that too many biologists are reluctant to make. Here the reasons are practical and financial. But the same kind of process could be used to decide that not all habitats need to be sampled, or can be sampled at an intensity that gives enough information to have confidence in the results and their interpretation for ALL habitats. Can one, for example, get enough information from all habitats to make strong conclusions from each habitat or would it be better to sample a selected set of habitats with greater intensity to narrow the bounds on estimates of characteristics of the biology in those microenvironments. These are important questions that must be explored eventually to get the strongest results with the greatest fiscal efficiency. Another issue is the collection of so much data that the lab analysis of field samples can't be done in a timely manner to guide work for the next year. See comment below re page 160 for an example of this kind of effort to determine how many samples are needed to have robust results. Also see a report on sampling invertebrates from streams by Leska Fore on the same issue available at her website: <http://www.seanet.com/~leska/index.html> See, for example, the following papers available on that site:

Fore, L. S. 2003. Developing Biological Indicators: Lessons Learned from Mid-Atlantic Streams. EPA 903/R-003/003. U.S. EPA/OEI and MAIA Program, Region 3, Ft. Meade, MD. Fore, L. S. 2003. Biological assessment of mining disturbance on stream invertebrates in mineralized areas of Colorado. Pp. 347-370 in T. P. Simon (Ed.). Biological Response Signatures: Patterns in Biological Integrity for Assessment of Freshwater Aquatic Assemblages. CRC Press LLC, Boca Raton, FL. Another report, perhaps available through the Florida Dept. of Environmental Protection website, is as follows: Fore, L. S., R. Frydenborg, D. Miller, T. Frick, D. Whiting, J. Espy, and L. Wolfe. 2007. Development and testing of biomonitoring tools for macroinvertebrates in Florida streams (stream condition index and biorecon). Florida Department of Environmental Protection, Tallahassee, Florida. This leads to the larger question: How long does data analysis for a year's data take? Is the task complete so that this year's data can be used to guide decisions about sampling strategies for the next year? If it takes 2, 3 or more years to count and identify creatures and do analysis, it is impossible to have results in a timely manner to influence study design and the value of results in limited time periods.

- Page 122, end of four questions: What is known about the microhabitat distribution of the predators (both trout species) and the chub that are moving through the area? Does this provide any guidance about the places to search for and remove trout? Or will removal be just a broad scale effort across all habitats? What is known about the size range of trout that feed on the chub and what sizes of chubs? Seems like these kinds of things must be important and some thinking along these lines could be used to guide the sampling and removal strategy? Or has that been done already and it is just not displayed here?
- Page 125, first two lines on page: I didn't see anything on the merit of tracking the size and condition of the fish removed. Are certain size classes (e.g., very large fish) missed with this method relative to other methods such as netting or fishing? How does that interact with what is known about the size of fish preying on chub?
- Project H: Large trout: Overall: This is an interesting set of elements that are presented fairly well, but without enough detail to evaluate fully. Also essential to be sure that the large-trout interests are not studied independent of the natives. These interests were not being merged very well in this project, when there is good opportunity to do so.
- p129: Natal Origins Project D.2 says nothing about trout--it is chubs only
- p133: Project motivations: Though helping natives is cited here, all of what follows is geared purely toward a trophy trout fishery with only incidental consideration of what would help natives. More integration is needed--look for the win-win scenarios.
- H.1: The lab growth experiment will be of margin value without direct comparison to other strains raised under the same conditions. And since these fish never see ad lib trout chow in the wild, the results would be dubious anyway. Transferability issues should be addressed.
- H.2.1: This primary production work will expand understanding the ecosystem dynamics, but it is hard to see how it is central to dam or fish management, which are the overall stated objectives. Would this model or measurements actually affect management practices? If so, sketching out that scenario in more detail would be helpful. Also need to clarify the coordination with other Projects under GCDAMP.

- H.3: The bioenergetics approach will be very valuable. The biggest challenge will be parameterizing the relative availability of different prey items. Nice that H.2 will give good prey availability data.
- H.4: This is also a great idea, though the budget seems out of proportion to the work required.
- H.5: Need to clarify what exactly would be done, and how it would yield concrete insights. The contingency nature of this request, but more detail is needed to be sure that it would be a good investment.
- Page 133, end of first paragraph: How about the hypothesis that a fishery that concentrates on catching and harvesting large fish culls the fast growing genes from the population, yielding a fish population that grows slowly and matures at smaller sizes, never reaching the large size of the more desired but rapidly harvested large fish. This is a well-known phenomenon in sport fishes and in many hunted terrestrial vertebrates. In effect, selective harvest of large fish truncates the gene pool of the resident breeding population, leaving a population that does not grow large. Nothing is said here about the intensity of fishing pressure on this population but it must be fairly high. Was fishing pressure enough in this system to have this kind of influence? If one of the other hypotheses is the causal factor, would we not expect that issue to have altered the growth rates and population dynamics of other species in the system? Is there any evidence of such changes that might be tied to a non-harvest situation as envisioned by the four hypotheses outlined in the text of the document?
- Page 133, End of second paragraph, just before section 4: These ideas very compelling. The presence of large numbers of small fish would certainly attract the attention of large rainbows, fish that often take small fish in their normal feeding activity. What specific habitat condition do you have in mind here? Habitat condition as a general explanation is just too vague to be very useful in understanding pattern or in defining specific management actions that are likely to resolve the problem.
- Page 134, Top two lines: Extrapolating this simple experiment to wild situations seems a dangerous path to me. There are so many complications that are not considered by this approach. At least those complications should be acknowledged here and a rationale developed to show how the more ominous consequences will be avoided.
- Page 137, First short paragraph: Seems simplistic to look at prey size without also considering the abundance of prey as a crucial determinant of growth rates. Many trout populations subsist on very small but very abundant prey
- Page 160, Ground dwelling arthropod section: See results of similar work in shrub steppe in WA and ID for ground inverts captured with pitfall traps. Consider number of traps, duration of trapping. This is an example of a study that looked at methods to determine the minimum number of traps needed to give enough data to make robust inferences about the condition of places. See also references and comments above for page 116. Two references from the shrub steppe studies are as follows: Kimberling, D. N., J. R. Karr, and L. S. Fore. 2001. Measuring human disturbance using terrestrial invertebrates in the shrub-steppe of eastern Washington (USA). *Ecological Indicators* 1:63–81.

Karr, J. R., and D. N. Kimberling. 2003. A terrestrial arthropod index of biological integrity for shrub-steppe landscapes. *Northwest Science* 77:202–213. Two citations—one using fish data, the other invertebrate data from streams—deal with aspects of proper level of sampling to give robust results without wasting time and money: Fore, L. A., J. R. Karr, and L. L. Conquest. 1994. Statistical properties of an index of biological integrity used to evaluate water resources. *Canadian Journal Fisheries and Aquatic Sciences*. 51:1077–1087. Doberstein, C. P., J. R. Karr, and L. L. Conquest. 2000. The effect of fixed-count sub-sampling on macro-invertebrate bio-monitoring in small streams. *Freshwater Biology* 44:355–371.

- When one reads the individual native and non-native fish projects it is implicit that they represent an integrated approach. However, what is not provided is a science design of the linkages of individual science components and linkages to the stakeholders' goals, objectives, DFCs, etc. These goals, objectives, DFCs, etc. are the basis for science programs for the AMP.
- There is some overlap in Projects D and E as relates to food base, growth and survival work. We assume part will be used for validation of findings while pursuing somewhat different questions. However, can duplicity be reduced?
- Project D [monitoring and abundance estimation in humpback chub aggregations in main-stem]: This project description was not well written and did not give precise details in some cases about the number of fish being handled and tagged. Two issues need to be evaluated. The concept of a classic meta-population applies here because of the network organization and directionality of flow in this network of subpopulations (see papers by Bill Fagan in Ecology on Aravaipa Creek). It is more likely that a source-sink paradigm applies (Pulliam 1988). Specifically, the aggregation at the LCR and in the LCR are the populations responsible for production and export of larvae downstream with some occasional straying upstream of adults or sub-adults. The important questions to answer here are: 1) do YOY move downstream and how far and is this related to floods during snowmelt or monsoon in LCR (as in Project E)? 2) What is the survival of fish in downstream aggregations (is it so low that these populations are not self sustaining)? And c) How much do the downstream populations serve as a lifeboat for the LCR population when it is waning in abundance? I.e. is there more than negligible movement upstream that would offset bad years when downstream production is high (if production downstream occurs at all). The second large comment for this project is the statistical analysis. One of the goals is to provide a robust estimate of capture probabilities by sampling more frequently. There should be some proof of concept here. A quick and dirty simulation showing how additional sampling might lead to lower CI bounds and more rigors in obtaining the capture probability. Also, it would be nice if this unit's statisticians could develop a method for combining estimates from electro fishing and hoop and trammel nets into something that gives a robust total population count.
- Project E [HBC early life history]: This one is mostly rock solid. There is some overlap with Project D in sampling (especially of the important LCR aggregations). Three comments: 1) Light traps sample a very biased subset of aerial invertebrates, typically those that are photophilic like moths and caddisflies. Moths may overwhelm everything else at some sites and you won't get mayflies, some or most chironomids and of course gammarus and other important inverts that don't emerge. The citizen scientist part of this is tremendous though. 2) Don't deploy sticky

traps for more than 5-7 days. After 6 weeks they will be significantly decayed and collect debris making the bugs impossible to ID below order and increasing the handling time on them. One more suggestion here is that the lab considers using a baby oil solvent to release the inverts from tanglefoot on the traps. Then they can reuse the plexiglass and look at the sample under a dissecting scope. 3) Some redundancy in H5-7—competition arises because of food limitation so the two are inextricably tied to each other. See some discussion in Sabo and Pauley 1997 CJFAS for ideas about native salmonids (cutthroat trout) and their competitive ability with transplanted migratory salmon (coho). For years various agencies in Canada have been transplanting (seeding) salmon above falls, there may be some papers out there that reveal whether this protocol has been successful (see Gordon Glova's work in particular).

- Project F [monitoring]: This section is a little mundane and lacking in detail. Nevertheless this work is core and essential. Focus the monitoring in a way that moves towards capability for estimating abundance and combining gear types in a rigorous way such that we know how relative abundance (not CPUE) of all members and life history stages of the community are changing over time.
- Aggregation and Metapopulation: We're still in the search for ways to expand understanding of how HBC define important habitat. This may help. But, the reality is that the LCR continues to be the focus of juvenile recruitment and adult abundance. Focus there!
- Hopes for undiscovered alternatives seem dim! There's high uncertainty elsewhere. That's no surprise given that there are damn few fish elsewhere! And, trammel nets continue to be a problem. Temperature is an issue, but that can only change if dam releases include a direct effort through multi-million dollar dam intake modification toward fostering gonad development through higher temperatures and/or if thermocline waters are entrained by lower water levels in Powell. The latter can be anticipated (by water level) and, as seen in the recent past, use this prospect to test for changes in distribution. We can't sample tissue maturation, but the indirect outcome of changes in distribution might help.
- Natal origins of HBC: The otolith component of this has good prospects. Those involved (Limberg) is a proven performer. The integrated outcome may be very informative about where HBC's spend their time. LCR should offer an importantly unique signature about duration and frequency of repeat visits. Again, everything we know about HBC's says that managing the LCR habitat is most essential.
- The ultrasound and Ovaprim prospect are interesting, but it seems that assessments of hatching success, etc., are a secondary issue. Constraints to spawning and recruitment may be temperature sensitive, but the more parsimonious explanations lie in ecological interactions in the LCR and the adjacent CR habitats.
- HBC Early Life History: This project offers important insight from recent work re. growth above vs. below Chute Falls and summer vs. fall seasonal growth patterns. Both of those results are "surprises". That leads to questions about how management might best proceed. The NSE effort has been very valuable in narrowing the domain of uncertainties. The events between juvenile and adult recruitment continues to be a major set of unknowns. That, unfortunately, is a too-familiar curse for fishery sciences.

- NSE methods and results now open the questions of correlates, cause and effects. Yes, multiple hypotheses are helpful. Some can be tested with existing data and upcoming sampling. Some require longer-term monitoring efforts and, one can imagine new ideas that derive from recent results. The above/below Chute Falls food web project is highly likely to produce important lessons of contrast and their explanatory value. Adding contaminants as a tracer may produce another round of important surprises in carbon flux (noisy) vs. tracers. Have a look at Jim Peterson's bioenergetics model for HBC's (and other prospects like Paukert and Peterson). That's a tool worth trying because you do know growth rates, temperature and diet.
- A concern is that there is temptation to provide "exact and precise" explanations about the mysteries of available prey and competition. Do not be overly expectant of resolving those. In fact, the HBC issues are a major driver for what GCMRC can and should do. This project has brought major advances. Support for its future offers strong prospects for additional and growing insights. We cannot expect a perfect fit to some stock/recruit outcome. Mother Nature is not very cooperative for this place and its fortunes. We can expect some important reduction in uncertainties. Pour support on this one.
- Monitoring native and non-native fish populations: This set of efforts is essential. Monitoring of juvenile HBC is essential to understanding population dynamics. The Chute Falls work is an essential program. The new PIT tag detection system provides important data that enhances ongoing field programs. Re-evaluation of the ASMR model is worthwhile. Using HFE's to control rainbow recruitment works and requires monitoring. Appearance and abundance of non-natives is discovered from monitoring. Management of the recreational fishery requires monitoring and is augmented by the natal origins project. Food base, drift and fish food habits are essential to painting the food web picture(s).
- The proposed "Citizen Science" project using river guides is novel. Potential benefits include the improved sampling program, the citizen contribution and the basic science that can be gleaned from better knowledge of stochastic insect emergences.
- Benthic algae and invertebrate work provides the monitoring background required to evaluate both spatial and temporal components that drive the food web. It's costly, but necessary as the foundational component. In summary, this is an expensive but proven and essential part of the monitoring efforts required for effective AMP. Therefore, strong support is essential.
- Interactions between Native Fish and Nonnative Trout: This, too, is an essential component of food web assessments. Both rainbow and brown trout are known predators of HBC juveniles (and suckers), but in different river reaches. The Bright Angel removal of brown trout complements previous removal efforts of rainbow above and below the LCR. Competitive interactions are also known, but less well defined in quantitative estimations. The proposed laboratory assessment may offer some insights with regard to vulnerability for size-based evaluations and temperature effects. Use of a surrogate for HBC is both practical and possibly informative.
- Factors Limiting Growth of Rainbow Trout in Glen and Marble Canyons: Rainbow trout fisheries are among the major ecosystem services offered through the GCE. This set of efforts seeks support to expanding the substantial but incomplete understanding of the component

ecological processes. In other words: “we know a lot, but not enough” about how this set of interactions work and, therefore, how to better manage them. This project stems from and complements other ongoing and relevant projects. Simply stated, there are too many small trout sometimes and rarely any large trout like there were in the past. That’s both a management and ecological dilemma.

- We encourage the development of bioenergetics and functional response modeling. That can serve as guidance to how much we really need to concentrate on primary production (PPR). Great mysteries reside in that cause-effect connection. Intensive study of PPR and drift may be educational but scenarios derived from modeling and for experimental results would probably help narrow the scope of search processes. In general, PPR is a not very revealing predictor of how fish grow and flourish.
- PPR offers advice, but not empirical guidance for management. Is there a fishery anywhere that is specifically managed by using PPR as the quantitative and predictive regulator. Yes, PPR happens and high vs. low make sense, but there’s a huge magnitude of unpredicted and unexplained variance by the time one gets several trophic steps up in the food web and, therefore, to the expectation of quantitative fisheries management practices. Perhaps we should deliberately change PPR by 10X through dam management (e.g. HFE’s), then see how that fits in a systems model and pay attention to what the confidence intervals do! A more interesting quantitative approach might be to regress estimates of PPR as the predictor of fishery harvest data. Those could be interesting because, after all, that’s what you’re trying to use to guide management.
- Fish density, predator-avoidance, diets, temperature and observed growth are the essential elements. Drift, primary production, predation risk, and competition are the operative components. All of that can be mechanistic and derives in the field. Lab studies of ad libitum outcomes may only provide boundaries substantially in excess of anything observed to date. RBT has been thoroughly studied many times and many places. It’s the ecological context that matters most. Build better models and test those for effects within the realm of the observed. Go forward from there.
- The proposed review of dam operations effects elsewhere may be helpful in bounding the likely controls available to managers and experimental design for protection of native species and desired enhancement of recreational fisheries. Pay a lot of attention to outcomes from the HFE work done so far. That narrows the prospects for all the local idiosyncrasies that happen below Glen Canyon dam. The most recent ECOSIM workshop done by Walters in Florida contained some revelations about how things work downstream.
- There have been a series of major advances in research efforts and consequent developmental changes in monitoring programs by the AMP. A concern does exist that after fifteen years of work, major plans such as this still do not provide an ecosystem science design and core monitoring plans are yet beyond our grasp. Work on the HBC in LCR is among the leaders on that. Work on trout follows closely in advances. Food web work is beginning to clear the clouds of opinion created more than a few decades ago. Modeling at any scale can be helpful in creating boundary conditions and helping guide the “art of the possible.” The Chute Falls and

NSE projects have made major contributions. Many unknowns and uncertainties persist. That's a reality for an ecosystem context subject to major management effects and only vaguely familiar to the evolutionary histories of its major players. Invaders increase that challenge. GCMRC folks have done a remarkable amount of good work in dealing with both the logistics challenges of working in the Canyon and under the watchful (and wishful) eyes of a complex bureaucracy while within the limitations of budget constraints. All in all, this is a commendable and capable set of projects. The criticisms are targeted on things that are either absent or may or may not produce desired outcomes. Some of those are of secondary utility. Some of those are promoted from hope more than evidence.

RECOMMENDATIONS

Many recommendations are provided throughout the general and specific comments. However, some deserve second mention for emphasis.

- Although some of the projects provide tracking back through stakeholder science questions, desired future resource conditions, information needs stakeholder goals/objectives, most do not. This program has its basis in stakeholders identification of problems, issues and opportunities. Much effort has gone into specification of multi-resource goals, objectives, information needs, desired future resource conditions, etc. through collaborative processes with managers, scientists and policy makers to gain resource improvements. Adaptive management processes were selected to define and implement critical policy experiments, management actions and necessary science to reduce the uncertainties associated with resolution of the many complex issues. It is important that this program basis be presented as the first chapter of this document to clarify the conflicting setting in which the management and science is being accomplished and how this imparts even greater complexities to balancing resource tradeoffs and conducting effective management and science.
- The 2007 Strategic Monitoring and Research Plan presents a science strategy that recognizes the conflicting needs of the stakeholders, complex resource interactions in the Colorado River ecosystem and significant uncertainties faced by policy makers, managers and scientists to gain necessary resolves. Many of the programs described in this plan were implemented or being considered in the Strategic Science Plan. Yet overall discussion of this plan as an ecosystem science approach is not prominent and often not apparent. Development of ecosystem science approaches has been apparent in the work on food base and aquatic ecology. Yet, as has been learned from experimenting with HFEs in differing seasons ecosystem forces between physical and biotic resources are significant and great uncertainty still exists. At least the fundamental concepts of the CRe drivers and an ecosystem science design should be presented in an introductory chapter.
- The General Core Monitoring Plan (GCMRC 2011) identified the Lake Powell and downstream water quality monitoring programs as at an acceptable level of development for their final review and revision for approval as core monitoring programs. Originally this development process for core monitoring plans was to begin in 2012. The SAs recommend that the proposed

review process begin in the first quarter of 2013. If funds are available perhaps the review could be initiated in the last quarter of 2012.

- This document references the current Knowledge Assessment as being based in information presented at multiple workshops in 2011/12. This does not appear to be an acceptable format for this critical information. The SAs recommend the AMP develop in 2012/13 a formal USGS report documenting the current level of science/management knowledge, i.e. a Knowledge Assessment.
- Extensive sampling is occurring in most projects. It is not clear if and how the scientists are evaluating the power of tests which use the created data, especially regards interactions within and among resources. In most projects the continued vagaries of high variance prevents firm conclusions. These issues have been discussed in the TWG and in various GCMRC workshops and in some Protocol Evaluation Panels. The SAs recommend an assessment and workshop led by statisticians/biometricians in 2013 be dedicated to an evaluation of existing sampling regimes (both research and monitoring studies) and a report developed assessing the need for changed and/or improved procedures. Specific areas of modeling analysis such as ASMR and sampling such as HBC growth rates above Chute Falls and food base assessments could be evaluated.
- There are questions about the value of elements of several projects including Projects C, E, G, H, J and I which are referenced in general and specific comments. For example the presentation of Project E was so scattered that I would recommend sending it back to the applicants for complete reworking.
- It is somewhat disappointing to see that, despite this being a long-term adaptive management-oriented program, few of the proposals really address explicitly long-term objectives and needs of stakeholders and managers. The links seem implicit for many of the project elements, but most projects failed to make this case.

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SCIENCE ADVISORS REVIEW OF THE GCDAMP 2013/14 BIANNUAL WORK PLAN AND BUDGET

TWG Summer Mtg.
Phoenix, Ariz.
June 20-21, 2012

Plan Reviewers

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Charge To Science Advisors

- ▣ Conduct Review 6/4-6/15.
- ▣ Evaluate linkage of Science Programs to Stakeholder/Managers Direction
- ▣ Assess Application of AM
- ▣ Evaluate Systems Design Linking Program and Monitoring and Research Projects to Objectives
- ▣ Evaluate Project Integration , Sampling Designs, Project Methods
- ▣ Assess Project suitability for efficient and effective inference, assessments, and resolves regards Stakeholder/Manager Objectives

COMMENTS & QUESTIONS

- ▣ The plan in general does not establish the program basis and emphasize stakeholder goals, objectives, DFCs, needs as direction for monitoring/research projects.
- ▣ AM paradigm not emphasized, i.e. policy experiments/management actions/science to reduce uncertainty in key areas to more rapidly gain stakeholder objectives.
- ▣ Ecosystem Science Design and overall science strategy not presented to clarify most appropriate paths (efficient, effective) to resolve program objectives.

COMMENTS & QUESTIONS

- ▣ Current Knowledge Assessment not developed and presented effectively as science basis for program/project direction.
- ▣ Development proposal for core monitoring programs not presented; yet is necessary.
- ▣ The issues of high variance noted for sampling, analysis and modeling could be assisted by statistic/biometric workshops, especially interaction effects.
- ▣ The presentation of Project B if combined with introductory chapter on stakeholder objectives, AM and ecosystem science design would provide a good format for other projects

Comments & QUESTIONS

- ▣ The Lake Powell program and water quality program seem at a stage of development to design and implement core monitoring programs. Is that planned for 2013/14?
- ▣ Several planned programs especially in monitoring would benefit from projected 5 year cost/benefit/risk assessment of alternative levels of science investment

COMMENTS & QUESTIONS

- ▣ Would the physical sciences program pursued over 5 years permit managers to effectively project system and large reach levels of sediment mass balance and beach structures under current dam operations agreements?
- ▣ Can system wide remotely sensed vegetation survey efforts provide needed information assessments for management in five years with the proposed site surveys?

COMMENTS AND QUESTIONS

- ▣ Will the proposed fish ecology programs pursued for 5 years permit managers to determine if RBT and BT predation is the most significant threat to HBC persistence under current dam operations?
- ▣ What is the expected time period for the proposed Cultural Resources Program to define management mitigation and protection actions for arch sites?

RECOMMENDATIONS

- ▣ Add a first chapter that defines stakeholder goals, objectives, DFCs etc., AM processes, and ecosystem science design
- ▣ Provide linkages to new chapter as in Project B and 2007 Strategic Science Plan
- ▣ Complete a USGS KA report

RECOMMENDATIONS

- ▣ Conduct program level statistic/biometrics assessment for sampling, analysis, modeling
- ▣ Conduct a Probabilistic Risk Assessment and Benefit/Cost Assessment of the overall program