Review of Glen Canyon Dam Adaptive Management Program Triennial Budget and Work Plan – Fiscal Years 2015-2017

Prepared by

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INTRODUCTION

This report summarizes reviews and comments on the Glen Canyon Dam Adaptive Management Program (GCDAMP or AMP) proposed budget and work plan for the period 2015-2017. The plan was prepared by staff of the Bureau of Reclamation and Grand Canyon Monitoring and Research Center (GCMRC), along with input and consultation from other groups of the GCDAMP. Aggressive collaborative work on the plan was initiated in February 2014 when staff from GCMRC presented a series of science findings from the previous year(s) programs to the AMP. This led to the draft plan being prepared in June 2014.

The plan presents a three year set of research and monitoring activities that will be reviewed by all groups of GCDAMP annually to assure continued effectiveness and efficiency. The Science Advisor Executive Coordinator and Science Advisors play active roles in the review and consultation process.

The procedure for Science Advisor input to the GCMRC 2015-17 monitoring and science planning process has permitted the following:

- Involvement of the SA Executive Coordinator to provide Science Advisor input in two discussions of the TWG, four discussions of the BAHG, multiple exchanges of information with TWG members including the Chair and Vice-chair, and with the GCMRC Chief during April, May and June, 2014.
- Request in May to the Science Advisor EC from the GCMRC Chief and TWG Chair and Vice-Chair to provide written comments on a Draft Prospectus of potential science plan projects, and follow-up participation in collaboration of the TWG and GCMRC on the most desirable projects to pursue.
- Participation of the Science Advisors in review of the Draft 2015-17 Monitoring and Research Tri-Annual Plan provided June 6, 2014, and development of this review report.

The Science Advisors appreciate the request for their involvement and participation in this plan development process. We believe the above noted interactions have permitted important exchange of information and collaboration for the TWG and GCMRC, and input of the Science Advisors.

PROCESS

While the collaborative process for development of the three year plan occurred over a five-month period, this review was done over two weeks in June. Once the plan was in draft form in early June, the SAs were asked to evaluate both the presented monitoring and science projects and their budgets for potential effectiveness and efficiency in meeting GCDAMP goals and desired future conditions for resources of concern. Each of the science specialists listed above as authors provided written comments which were collated into this report. The structure of this report is organized in three sections. The first section presents general comments that address the entire report in context of the overall adaptive management programs and processes. The second section provides comments on each of the enumerated projects. Finally, the report provides a set of recommendations for the Secretary, AMWG, TWG, and GCMRC to consider in their continued assessment of the most appropriate management and science programs to pursue in 2015-2017.

GENERAL COMMENTS ON TRIENNIAL WORK PLAN

Overall this is a well thought out research plan, and certainly the most thoroughly prepared plan we have ever reviewed. In particular, we like the context for the proposed research projects. The authors of the plan have responded to a variety of factors, such as the Assistant Secretary's direction (as provided in two memos), the Grand Canyon Protection Act, Strategic Science Questions (SSQ), desired future resource conditions (DFCs) Core Monitoring Information Needs (CMIN) and Research Information Needs (RIN). While not the only factors that should be influencing the direction of research for the next three years, they are certainly important factors. In addition, the research teams assembled have excellent track records of collaboration and research output in their areas of expertise.

All plans, like this one, present opportunities for improvements and the following comments by the Science Advisors should be taken in that vein. Each advisor takes this task seriously and looks deeply for areas where we can offer assistance in improving the plan. As will be borne out in our many review comments, GCMRCs leadership and staff have presented projects and proposed procedures that are extremely well designed and presented. As such the SAs find few significant faults with the plan and endorse all projects presented with changes proposed. Even then, we encourage the GCDAMP to consider all our comments as opportunities to improve the plan.

Our general comments are described more fully in the following paragraphs with these general headings.

- Program and budget plans.
- Budget suitability.
- Adaptive Management and collaborative processes.
- Systems assessment and systems models.

- Integration of science projects and integration of science and management.
- Revision of selected program administration.

Program and Budget Plans: The Glen Canyon Dam Adaptive Management Program, established in1996 by the Glen Canyon Dam EIS, was originally administratively structured around an annual science program and budget review process by stakeholders, agencies, and a research centers staff. The Adaptive Management Work Group (AMWG) led by the Secretary's Designee and with its Technical Work Group (TWG) for support, was responsible for collaboration with the Grand Canyon Monitoring and Research Center (GCMRC) in recommending to the Secretary management and science direction and required budgets. The first 5--7 years required extensive collaboration of stakeholders and scientists in development and refining the new adaptive management and science program direction required in the EIS. To provide longer term planning, a strategic science plan was developed and updated by GCMRC (1997/2007) and a management strategy was developed by the AMWG (2004). However, over this entire period the intense annual budget and project development process dominated planning and appeared to limit AMWG, TWG and GCMRC time to develop more focus on needed long-term science and management strategies. As noted both GCMRC and AMWG have developed strategic planning documents, but they are not living documents that are evaluated by management or science on even 5-7 year intervals. They are not used for continued reference to determine where the annual programs are in reference to where the Secretary would like them to be in 5-7 years. The new three year plan proposed for this review could be the opportunity for the AMP to accomplish improved short term (1-3 years) and longer term (5-7 years) planning. Three years provides sufficient time to implement a multi-year tiered program and gain some assessment of both its potential success and needs over a longer term. However, we note that bringing major lines of research to completion generally requires more than 3 years, hence there is an important role for both program review on a 3-year cycle and continued focus and funding on a set of core topics over multiple 3-year funding cycles. This proposed plan taken in two cycles could provide possibly an effective format for long term planning. However, to accomplish that end the AMP should consider several improvements and changes to the plan in the current cycle (2015-2017), including: improved integrated science planning; improved inclusion of BOR, other federal, state and tribal agency management and science programs explicitly connected to this program; incorporating and approving the complete elements of a core monitoring plan directly into this plan; and incorporating strategic management and science guidelines directly into this plan. Such a process, of course, is fully dependent on the policy direction to be laid out in the EIS/LTEMP. Once that policy direction is in place, perhaps AMWG/TWG/GCMRC could expand this instrument in 2016 or 2017 to include the above-discussed changes. It would be more effective as a short-term plan reviewed every three years if it had these components built into it. And, it could service as a long-term strategic plan that could be reviewed at six-year intervals. This approach would seem to be more effective and efficient than production of separate long-term plans that are not working documents.

An example of significant programs deserving more inclusion is cultural resources. They are not mentioned in some of the projects even though justification for data collection includes benefits for managing these resources. Projects that collect important time-series data, such as Project

3, should explicitly include some areas that are archaeological so that the very intensive data collection being done could also be used to benefit management and research of cultural resources. And, related to this is reviews. Independent research oversight panels that are convened should include cultural resource experts to help to ensure the integration of cultural and natural resources in both management and research goals.

A second example are is that the compartmentalization and possible redundancy of data collection for individual projects could be easily seen as needing improvement with a more systematic program strategy. More collaboration among the scientists to see what data collection strategies could be used across projects needs to be done. Many of the same data collection techniques will be used across projects and there could be more explicit discussion of collaborative methods (e.g., citizen science, remote sensing, LiDAR). There also could be more discussion of how research and management goals intersect across projects, not just within each project.

The organization and layout of the plan could be improved for communication to the GCDAMP stakeholders and a wider audience. It is a large document, just short of 500 pages. Its complexity and importance calls for a well designed and written executive summary that can stand as an independent document. As such all elements of this plan would be addressed in the executive summary. This would greatly extend its utility. The plan itself would also benefit from periodic guides in the text so that the reader would know what to expect and to provide logic for the overall report. Because of its complexity, there is limited indication to the uneducated as to how and why all projects fit together. Also, the level of detail varied across budget items. The overall budget never appears in the document, only as an appendix. As such, there was continued emphasis on outlining activities and costs for each program or project, rather than as a whole.

Budget suitability: In this plan, budgets are presented for program areas and projects, and the overall budget is placed as an appendix. A budget summary is needed in the introduction. Even though some new programs are costly, they may represent changes of less than 5% in overall activity, and none represent major departures from the general program direction. Budget in individual projects are addressed in a later section. Even then, the overall proposed budget allocations are somewhat similar to the approved 2013-14 program direction, which were determined satisfactory for maintaining science, and management direction toward the Secretary and stakeholder goals, lines of research, and questions being addressed. Regarding budget directed toward major science questions of concern to the AMP, several significant accomplishments regards long-term learning have occurred in the past decade along with launches of several new programs with significant associated findings. Clearly, some of the long-term projects have accomplished several of their objectives and, therefore, have been able to reduce the frequency and intensity of sampling effort. This also demonstrates budget suitability in maintaining progress toward important goals. Nonetheless, some concerns may exist regarding longer-term budget needs and potential commitments. With reference to the final five years of the GCES programs and the first five years of GCMRC programs, it would appear that GCMRC today at best has stable short term budgets. It is apparent that AMP scientists and managers may be required to address more challenging problems and questions in the next five years. GCMRC facility cost increases alone will challenge the ability of managers and scientists to maintain learning necessary to resolve issues in physical, biotic, and socio-cultural programs. And, although much has been learned about the biology in this system and its implications to

HBC survival and improvement, much uncertainty still exists regards impacts of food base, predation and habitat. The fish is improving but the science cannot clearly say why. Water temperature is considered a potential factor that could have negative effects in all of the above areas but, and even though a fund is maintained to at least respond to predation it may not be sufficient. SAs recommendations for development of capability (selective withdrawal device SWD) to cool downstream waters if future warm water projections occur is still under study. The Upper Colorado River Commission allocates over $2 million annually to non-native control compared to an annual average of less than $.3 million in the AMP. Many national and regional projections of climate change characterize the Southwest and Colorado River Basin to have more change relative to other U.S regions. Those changes reflect increases in ambient temperatures as well as water temperatures, reductions in precipitation, greater variance in weather patterns, and potentially more intense weather events. This system (GCD) currently has no management capability such as a SWD to mitigate increasing water temperature although it has been studied. Without this capability threats to HBC would seem to increase significantly. Regarding this three year plan, it would appear that the above noted issues and projections are of sufficient significance to this system to warrant a proposal for a new multifaceted science/management thrust in the GCMRC program and the associated required budget. This would be a strategic activity at this time and perhaps it is planned to be addressed in a separate strategic plan. However, as noted above, a strategic plan is not proposed. Even if that were the case it would seem that some proposed program activities and necessary funding would at least be evident in 2017 programs to address additional science and management needs. Some needed activities would include: closer formal collaboration of managers/scientists across the entire Colorado River Basin, development of a systematic and linked basin-wide approach to model, monitor and assess change in and impacts to key indicators, closer evaluation of a SWD, and at least projections of potential program and budget needs for these efforts. Since there is currently no other AM program on the Colorado River with the capabilities to monitor large riverine areas for effects of climate change on water temperature physical/biotic/social resources, one would assume such activities would occur in the AMP and be addressed in this plan. Their omission is a major oversight. It is our assumption that it does not appear here because of its development in the EIS/LTEMP, and that it will be incorporated in this document in 2016. That would in fact be more appropriate, and perhaps that could be noted in this document. Chapter 2 lists the criteria for development of the budget and these criteria lay a good foundation for understanding that process. However, there is no mention here of recommendations from the various Protocol Evaluation Panels (PEPs) that have been performed in the last few years, some of which include climate change impacts. Including these would be beneficial to ensure that all identified research themes and related budget needs have been incorporated. We suggest that a summary of overall budgets be included as part of the introduction, with the detailed document retained as an appendix**.** It is not clear why there is a need for two separate facilitator. We agree that there could be a healthy role for such a position, but it could surely be shared between AMWG and TWG and even then might not require full-time staff. It is not clear why overnight mail costs are considered for these meetings; it is far more cost-effective to email documents prior to the meeting, then provide hard copies on request as contributors arrive**.** NPS permitting costs for permitting the proposed projects seem excessive given that almost all project elements have 3-year time frames and hence should require labor-intensive permitting only in FY2014 or 2015. Given that the stated goal of the Native Fish Conservation Contingency Fund is non-native fish control, it is not clear why major spending commitments regard native fish studies (364k of 824k). If the balance of 460k is sufficient to cover all non-native fish control efforts, why is the budget so large in the first place?

We appreciate the need to have contingency funds available to act quickly if exotic species expand their range unexpectedly or trout densities rise enough to trigger removal efforts. However, it would be cleaner to keep these separate, so that ongoing research projects on native fishes would not be jeopardized by events that require non-native control, and likewise promises for native fish research do not have to compete with appropriate response to changes in non-native species status when allocation decisions must be made. Finally, the rationale for the enormous ramp-up of these funds for FY15-17 is needed. Proposing to spend that much money on an oral history seems like money that could be better spent on other efforts. There are a tremendous number of documents on AMWG, GCDAMP, etc. that are available on the web since the inception of these efforts, so the incremental gain from an oral history seems minimal compared to other opportunities available to the Public Outreach Program. The Web page development process could receive more support. It is a program that can provide real-time access to all the AMP completed and planned program activities for the general public.

Adaptive Management and Collaborative Processes: In reading the plan, several general program activities stand out to bolster ones confidence that active adaptive management processes are in place. Obvious improvements are occurring in research efforts directed toward implementing experimental manipulations, monitoring impacts, and revising management actions such as high flow events, non-native fish control, and HBC translocations. These examples of the adaptive management paradigm are commendable, as is enhancing the role of citizen science. Moreover, it is clear that collaboration between GCMRC and BOR is active in management actions and science with regard to high flow program implementation, modeling applications in Lake Powell, and funding critical fish ecology projects. We were also pleased to see that collaboration amongst GCMRC scientists and agency managers is expanding relative to the 2013-14 proposals, ranging from brown trout and RBT control to HBC translocations and assessments. It is difficult to determine how much adaptive management accomplishments are needed for the AM process to be considered successful in the AMP. One must first look at the evidence of AM processes being in place and also being actively pursued. A second is to determine if real outcomes are a product of using the AM processes. Although this is not a specific review of AM application in the AMP we believe that generally there is evidence that AM processes are being pursued, and that important accomplishment is and will continue to result from that pursuit. The GCDAMP has been in existence since 1996 and it has demonstrated accomplishment in the AM paradigm. As noted, it has implemented several complex adaptive management actions, including high flow releases, endangered fish translocation, non-native species control, etc., and modified these strategies through progressive science and monitoring learning processes. We also believe that any management direction such as AM has continued opportunity for improvement. The Asst. Sec. and AMWG determined in 2014 that revised management actions and programs could be important to the cultural resources program and the 2015-17 Plan proposes improvements in emphasis, funding, collaboration, monitoring, and research. We see these actions as important for improving AM effectiveness. Without an in depth assessment of AM application in the GCDAMP, it is difficult to determine if major changes are required. It might be important to actually make AM processes and accomplishments a formal part of the Tri-Annual Plan review process every three years. That is, AMWG and TWG and the AM processes they implement are critical elements to the AMP, but these elements are not reviewed and directly evaluated for accomplishment. After 18 years, perhaps the AM process itself needs revision in the AMP. Referred to “double loop learning” in application of AM processes, some programs have changed after formal assessments. We recognize that further assessment efforts would impose further costs on the overall GCDAMP process, and would

distract the scientific team from its core mission. Perhaps a brief lower cost review might be considered for direction by the SAs in 2015 in concert with the brief review of the 2016 program.

System Assessments and Systems Models: Arguments have been made consistently over the last two decades in multiple reviews that the complexity of the ecosystem under study and the science and management programs applied demand some type of ecosystem model to help guide most appropriate paths for management and science. Along with others, the SAs have proposed this direction in several reviews. Development of systems models and sub-systems models have occurred and several are being utilized to guide both science and management in this program. Both ecological system models to guide science and management system models to evaluate policies have been discussed. The SAs have strongly endorsed that an ecosystem science model be developed and used to guide strategic planning. GCMRCs past work led by Walters should be reviewed in this regard. The SAs have also encouraged that an overall systems model for the Colorado River Basin should also be developed. It would have to be an open model that at least permitted evaluation of potential impacts that are exogenous to the currently defined CRE for the GCDAMP, especially potential perturbations due to changes in climate and direct environmental alterations arising from new policy decisions. System models are very expensive. Should the stakeholders support development of a basin wide system model, the AMWG should define specifically what it must accomplish. Given future climate implications to the Colorado River as a whole, would it be best for AMWG to collaborate on a basin wide system model that would have greater focus on refined assessment of policy and management actions in the system and how they might affect critical habitat requirements, water availability for desired recreation and water development, energy production, etc.? That is, should the AMP be a partner in a basin wide model with other Colorado River programs? The approach could have greater focus on learning regarding impacts of basin wide impacts of management actions and natural phenomenon on general regional physical, biotic, and social resource needs rather than trying to refine the model to predict the specific impacts of marginal habitat changes on biotic species of concern and local social issues. GCMRCs and other basin programs science projects and associated ecosystem models could be tasked with this learning need. However, the SAs also recognize that such efforts can substantially dilute efforts away from specific needs of the ecosystem and social systems responding to Glen Canyon dam, and hence should receive significant planning.

Integration of science projects and integration of management and science: The AMP overall program has had one review since inception by the National Academy of Sciences that stressed the need for system-based science approaches, linkages and integration of differing resource science and monitoring projects and approaches, and greater integration of management and science approaches. The Science Advisors have stressed the importance of these approaches in several reviews including the 2013-14 Plan review, and the TWG and AMWG have also expressed the need for these efforts. This being said, there have been significant accomplishments in the last five years in all these areas as noted in this review, and the 2015-17 plan is demonstrating improved approaches and accomplishments in all areas. In reality, because of the complexity of this science and management program, and the detail that must

be provided at the project level, it is difficult to actually interpret all the integration that is occurring. Often plans of this complexity will devote an entire first chapter that presents a clear picture of the overarching aims of the program, its long term target outcomes, adaptive management and science methodologies to ensure critically needed science and management integration including system models and approaches for conflict resolution, application of results, addressing policy changes, etc. Such an approach could improve this plan. We believe that Project Element 13.3 Decision Support System (DSS) that is proposed is an important step in that direction. The development of the Decision Support System should be able to utilize information from numerous Project elements proposed in this Triennial Work Plan and point out any research gaps that need to be filled in the next Triennial Science Plan to enhance those linkages. We also applaud the fact that Project 10—a new initiative—is synthetic in its approach.

Revisions in selected program administration: Concerns by USGS over any potential perceived conflict of their administering the Science Advisor Program budget has resulted in transfer of this program administration to BOR. The Science Advisors do not see this as conflicting with the stated purpose of the program if its tasks and activities continue to be specified by the AMWG and independent unbiased reviews by a group of science specialists are permitted as prescribed in the operating procedures for the Science Advisor Program. It seems reasonable that BOR could administer the SA Program contract in an objective manner. However, for it to be effective as an independent review and service program, its services and outcomes should be available to all stakeholders and GCMRC as specified annually by the AMWG. It should also be permitted to provide some level of review and service input to AMP entities as specified in the SA Operating Procedures even if the level of budget support must be reduced due to budget constraints. These outputs should be provided independently by the SAs' representative(s) to the AMWG/TWG/GCMRC in open forum so as to prevent potential bias from any individual party in the AMP. Based on past requirements and expected future SA program requirements the proposed budget of $70-$80 K would seem to require AMWG to review the SA Operating Procedure and prescribe selected reductions in service/review activities for the necessary RFP. We are concerned that due to the timing of the program transfer and time requirements for contracting, the AMWG/TWG/GCMRC will not have access to an independent science group for an undetermined amount of time in FY 2015.

It is proposed that the Lake Powell program administration is also to be moved to BOR. The SAs have proposed in past reviews that if it is moved to another agency the general activities of this program should be retained. It also has been proposed in past SA reviews that this program needs to have an AMWG and agency review regarding how it can best contribute to specified AMP or agency goals, DFCs, critical questions, etc. in the future. For example, are the types of data collected and the intensity of sampling necessary to respond to agency needs? Should the analysis work continue on all the biological samples? Could an assessment be made of a subset of samples that have already been analyzed to determine if this data offers potential for learning beyond what would be expected given current existing knowledge from other western reservoirs? Certainly, the modeling capabilities cooperatively developed by BOR and GCMRC (CE-QUAL- W2) seem to be important current tools for agencies and would seem important to a basin wide management policy modeling approach should it ever be pursued.

COMMENTS ON SPECIFIC PORTIONS OF THE OVERALL TRIENNIAL WORK PLAN.

This Triennial Science Plan presented is the most comprehensive and complex plan ever presented to the AMP. It is a very professional presentation. A complement to both GCMRC and the TWG is that one has difficulty dismissing any project as not of important value to this program. The SAs find all projects to be worthy of pursuit and encourage their inclusion as possible with recommendations for change provided. In addition, the proposing scientists, program administrators, and reviewers alike would benefit from creating a more consistent template for each proposal element. We were impressed with the structure introduced in Project 5, where background information included key graphics summarizing existing data, and each element included coverage of both the scientific rationale and management implications.

Project 1, Lake Powell: The Lake Powell program and its importance to the AMP direction has been an [LG1]ongoing discussion since inception of the AMP. The AMWG In-and-Out Committee and the CRe definition of AMP boundaries downstream of GCD has created extensive discussions of how this particular program could best serve the AMP. It is now recommended in this plan that this program be shifted administratively to the BOR. How and where this program is administered is the purview of the Secretary and we believe it would not effect how its outcomes could support basin programs like the AMP. The SAs in past reviews have noted that a program in Lake Powell is critical to continued learning of how natural perturbations (climate) and human interventions (policy and management) will effect water quality and quantity variances in the CRe. As such the SAs have supported continuation of these programs. That being said the SA reviews have been critical of the fact that the program's budgets and productivity have been allocated to production of data without clear science plans as to how that data will be used to advance management and learning related to critical goals and resource questions of the AMP. Early reviews found that even data development was not properly automated, verified for accuracy, and timely reported. Although this has been corrected regarding physical parameters, extensive biological data assessments are still backlogged. Another primary criticism was the absence of effective plans for data analysis and interpretation, which appears in this plan to be in process. A very positive outcome has been the collaborative GCMRC/BOR effort on the CE-QUAL model, and this may in part provide good basis for transfer of administration. With recent issues of shortages, increased water demands in the basin, changing use and management policies, and the potential impacts of climate change, Lake Powell management and science programs will become more critical to all managers and users in the basin. We believe, therefore, that an effective Lake Powell program will be more critical to the AMP and basin in the future. There are many critical questions that will need to be addressed. What are the expected future water temperature changes in the CRe? We do not see the evidence of strong pursuit in this plan. What are the science analyses necessary for us to best predict temperature and other water quality changes, including biological parameters? Efforts to date on modeling are to determine short-term (1-2 years) outcomes of expected values. But climate change is now forecasted with greater certainty for the Southwest. Are not the climate change prospects and water level, volume, and water temperature changes critical to long-term management and science issues? Will not water released at the dam be drawn from somewhere closer to the thermocline? Will that not mean that water temperature will increase in the main-stem, as occurred last decade, with high likelihood of significant impacts to aquatic biology? Since a management tool is not in place to mitigate, is it or can it be planned.

And, are not GCD and Lake Powell critical water management tools in this basin? It would appear that the answer to most of these statements is yes. If so, the question raised in the general comments is then raised again here. The GCDAMP is currently the most capable program in the Colorado River Basin to plan and implement a program to address these issues, and seemingly a Lake Powell based collaborative initiative on systems analysis of the basin and mitigative management and science strategies would be planned. Yet, it is not in this plan. We conclude as above that it awaits policy direction from the EIS, which seems most appropriate.

Project 2: The monitoring of water quality and sediment transport is fundamental to evaluating changing riparian and aquatic habitats in many other projects. It appears that the proposing investigators have identified appropriate timescales, locations, and methods for profiling the water quality and sediment fluxes in the main river channel and select tributaries. Integration with other programs is demonstrated in collaborative work of these project scientists with other projects conducting interpretation of findings to biotic and cultural resources. Other major accomplishments are web-based efforts to continually create improved public real time access to data. Two questions always exist with costly monitoring programs. The first relates to stakeholder level of need for information as regards type and degree of specificity. The plan reviews in the last decade would indicate the program is responding to information desired and using several direct and indirect methods to provide the information to stakeholders and managers. The second question relates to methodologies used for data development, analysis and interpretation to managers and stakeholders. Again, reviews would indicate that ongoing assessments of improved science and methods are evaluated and implemented as proven more effective. New technologies are especially important to sediment transport and, perhaps, might be very useful in collecting sand budget data, which are critical to evaluating both sand storage and beach building and loss. What could possibly help at this juncture and especially on completion of the EIS is a reassessment of AMWG explicit information needs when cast against goals and critical questions being pursued. This process by AMWG/TWG would assure that only necessary information is being required of the GCMRC to assure future needed flexibility in science and management programs and budgets.

Overall, the proposal would be considerably more compelling if it focused more on evidence that continued monitoring and upgraded methods can change our perspectives. Even for strong supporters of monitoring, it is worrisome to perceive that the effort of monitoring itself takes precedent over creative use of the data to inform fundamental understanding and real-world management. That was the sense conveyed by the proposal; while 2 full pages of lists of relevant agency priorities/mandates was provided, only a single paragraph ("Recent research on the Colorado...") offered any specific indication of how the new data can boost understanding and improve management. How wrong were we with 60 minute time resolution instead of 15 minutes? How much change in sediments was observed during the ongoing drought compared to before? How might these data inform future climate change adaptation efforts? Just how are these data used to trigger and evaluate the High Flow Protocol? Tackling these kinds of issues with even a few sentences would go a long way toward justifying associated budgets. The work is really important, but it appears that funding success is taken for granted by the proposers (although SA sabers were rattled regarding the calamitous consequences of under-funding this monitoring).[p2]

The website is a great concept, though it took several minutes to create a figure of just 6 years of data (even requested in the early AM when server demand would be low). That suggests that the server is badly underpowered or overtaxed, and/or that the coding is far from optimized (e.g. using default bias settings, as most users would, should allow instantaneous generation of graphs because all values can be pre-calculated). The duration curve will indeed be a welcome addition, though the proposal makes it sound like a major technological feat when in fact every aspect of presenting such a graphic and allowing user-defined calculations is very easy to code and serve.

As noted in the introduction to this project, the data collected are used for a number of other investigations, including those related to socio-cultural resources (p. 55). This project would benefit from integration of archaeologically and culturally significant sites. It isn't clear how the important information that is being collected and will be collected is actually being used or could be used to monitor the impacts on cultural resources in the CRe. Thus, while it is stated that "Collaborations also exist between this project and every other funded physical-sciences and biology project at the Grand Canyon Monitoring and Research Center, mostly in a supporting role, and with researchers in academia" (p. 61) no mention is made of collaborations with archaeologists, or that the locations and physical attributes of archaeological sites are in any way being looked at as part of their work with the USGS Center for Integrated Data Analytics (CIDA). If it is, it should be mentioned how and what the results have been.

Project 3: This project has significant focus on the impacts of HFEs and intervening flows on riverine sediment abundance, movement, and storage. It also has focus on sandbar development and maintenance in the system, and modeling of sandbars in the system. It continues to address through both monitoring and research one of the most critical questions of the AMP, i.e., can appropriately managed high flow events and other required flows through time provide general stability to the number, location, and size of sandbars in the system. The program also provides critically needed inputs to understand flow regime impacts to riparian and aquatic habitats. Extensive funding is proposed for 2015/16 to evaluate differing data recovery methods and data quality to assess sediment and sandbar conditions. It is difficult to determine explicitly from the write-ups if duplicated effort exists. Most of this project effort, approximately $1.2M is directed at providing definitive assessments of the status of sediment and sandbar resources through multiple approaches. It was unclear whether the increased effort to gather higher resolution data will truly be more informative. Again the question addressed above might be posed. Would less data resolution serve managers well and also save costs?

Objective 3 proposes to utilize some of these data to develop and refine a model to predict sandbar development and variance in the system and how that variance is linked to operations management, including normal operations as well as event flows such as HFEs. Development and testing is to occur through the 2015-17 period at approximately $100 K per year. It is not made clear how this modeling effort will integrate in overall AMP and EIS program accomplishments, including Argonne's sandbar modeling efforts in the LTEMP. The two modeling approaches use different methods but seem to be addressing similar if not the same

questions. Although costly, this modeling project is addressing one of the most critical problems in the AMP. If in fact duplication exists by design it could be useful in moving more quickly to more effective monitoring methods and improved modeling in the three-year period. In total over $0.3 million may be expended in three years in this modeling effort, which seems cost effective if successful. Results from Project 3 should also provide useful information on the distribution and size of camping beaches, an important element in visitor satisfaction. On the surface it appears that parts of projects 2 and 3 could be integrated to reduce costs, and yet each project element provides critical elements to overall goals and objectives. Further, assessment of costs for each element does not reveal excessive expenditure.[p3] However, this should not preclude a closer look at potential cost savings from integration.

There were several more detailed issues that merit consideration. Element 3.1 focuses on sandbar dynamics, but seemed to treat each potential influence as being independent of others. An 'experimental design' perspective might be more informative, wherein sandbar growth and loss rates can be envisioned as integrating the effects of location in the river, bar configuration, local currents and sand inputs, event characteristics (HFEs, etc.), and interaction among these factors. Such an integrative way of thinking would be more powerful than treating each as an independent predictor. The work could then merge bathymetric and topographic perspectives by testing the spatial association between riverbed dynamics and sandbar dynamics. This merger would align with the overall geohydrological approach: is sand transport a local or long-distance phenomenon, and how does that scale with event size? By determining the scale at which sand exchange between bed and bar occurs, it would be easier to reconcile the interpretation of data from individual sandbars with the large-scale flux view of mass balance between tributary inputs and reservoir sink.

More generally, it was striking that no work was proposed on climate or other controls on sand loading from tributaries, or delivery of sand to the downstream reservoir. Those topics seem the logical way to tackle the sustainability context raised repeatedly in the background section. Figure 8 is fascinating, and prompts the question of whether it is coincidence that the one site where high-resolution data indicates major reductions in sand bar elevation is also the site where the validation of the RS approach is also very weak? It seems this could indicate that the RS approach works well for growing sandbars but poorly for shrinking ones. That has important direct implications, but could also point the way toward methodological refinements that enable better estimation of flattening sandbars using RS. The SFM approach is a nice addition as a high-risk, high-reward element, and it was good to see the precision will be quantified to pave the way for citizen science implementation. But why does the methods comparisons focus on down-sampled data to make high-res methods comparable in sample density to low-res methods, when the more salient comparisons would be comparing the gold-standard (LIDAR) to SFM and total-station without any down sampling? It seems the key aim is knowing how well each can perform in an absolute sense, which dictates which constitutes the minimum acceptable effort to achieve adequate accuracy. Similarly, under element 3.2, why not validate using new aerial imagery that could be directly compared the 3 methods proposed in the previous element (SFM approach). I understand that value of the historical perspective, but comparing against multiple methods in the present would provide richer validation and methods development possibilities.

As the 'long-reach' approach is discussed (p103), does the failure of the three small-scale sampling strategies to yield the larger-scale mass balance simply suggest that the wrong predictors are being used in sample site stratification? Apparently it is not enough to focus on large eddies, but it is not clear that the team has fully mined the 2000-2004 data by comparing depth changes to other mapped characteristics (depth, flow velocity and vectors, proximity to bars, nearest upstream or downstream riffle, etc.). It seems there must be some way to predict which places are most dynamic if the old data are mined more fully? Also, it was surprising that hyporheic issues were not mentioned. Perhaps some hydrological tracers of hyporheic exchange would help to predict locations where bed thickness is likely to be dynamic (by virtue of reflecting both substrate characteristics and hydraulic forcing). With regard to sonar application (p105), similar efforts are underway even with single-beam sonar in lake environments-- see lakemap.com for details. They use a standard Lowrance echo-sounder and can resolve macrophytes and hard vs soft substrates. To validate your methods (p107), will you collect grab samples of surface sediments and plant material. That is important since you are expanding the spatial scale of surveys enormously, and the identification of bryophytes and chlorophytes seems like a reach.

Finally, a few statistical comments. The test of predictors of sand bar change (p118) is a perfect application for boosted regression trees, which deal nicely with non-linear predictions and complex interactions. They also offer the capacity to predict unstudied areas from the suite of descriptors, though that is more complex than with multiple regression or other basic parametric approaches because there is no singular predictive equation. The idea of grouping sites is unlikely to be informative because groupings result in: a) reduced statistical power to detect and estimate influence of a particular predictor, b) losing the capacity to use small deviations in multiple predictors among broadly similar sites to inform fitting of each predictor (i.e. groups will still have modest heterogeneity, but that information is discarded in fitting), and c) lower large-scale predictive power over treating sites individually. In any case, that empirical descriptive statistical approach will be a nice complement to the mechanistic LES modeling approach. It is also admirable that the team will establish a control network to ensure consistent elevational standards for application to all types of data being collected for this and other Projects, but most readers would benefit from clear presentation of some what-if scenarios. How badly would the research mission be compromised if the control network did not exist (since it currently doesn't)? For instance, what would the consequences be of not having the RS and field observations of sand bar height on the same precise elevational benchmark?

According to the plan, the project has three research components, but as with Project 2, none of these explicitly addresses the integration of archaeological resources into the models. Nor do the "key monitoring and research questions addressed in this project" (pp. 81-82) specifically mention archaeological sites. Nonetheless, this is an important project and the monitoring portion could be extremely valuable for assessing impacts on archaeological sites, assuming that areas that are targeted for repeat photography, remote sensing, etc., are areas with archaeological sites or other culturally significant areas. However, there isn't anything in the plan that states that the sampling strategy included consideration of archaeological sites and/or culturally significant areas. Many of the cultural resources may not be directly affected by this

specific project because they are above the elevations being studied, but if they are in areas that overlap with sandbar areas and could be included in the studies then they should be explicitly part of the sampling design. The plan mentions that there is "a pressing need to develop a representative sandbar sampling design" (p. 97), so now would be the time to add areas with archaeological sites to the areas being sampled. The subproject (Project Element 3.5) to support the geodesic control network has important contributions to make to archaeological site monitoring as well as other research projects.

Project 4: Project 4 represents one of the main investigations into cultural resources during the next three years. The sub-plans to the project each tackle (1) mapping with remote sensing techniques areas of "active aeolian sand" and quantitative analyses to understand the sources and interactions with other elements such as barriers; and (2) analysis of historical photographs to more qualitatively assess landscape change associated with active erosion. The latter will result in the preparation of a long-term monitoring plan. The proposed components in the plan aim to determine rates of erosion that will contribute to the desired goal of preservation in place. So, understanding rates of erosion is extremely important for planning purposes, and especially for the Long-Term Experimental and Management Plan. However, we are concerned about effective specification of this project and it is difficult to connect directly the science effort in 4.1 and 4.2 to expressed stakeholder needs for mitigating impacts from dam operations to archeological sites. Research has been ongoing for multiple years to evaluate the relationship of fluvial processes below 45K CFS flows and geomorphic processes above 45K CFS flows. However, although establishing association, proofs are lacking to justify full entry into the proposed monitoring approach. The projects small sample size should be increased and TCPs added. It is not clear that the effort is a priority for Tribes. According to the Triennial Work Plan (p. 149), the research project is tied to suggestions in the prior Protocol Evaluation Panel (PEP) and addresses Strategic Science Questions (p. 151). A concern is that it is not clear if this would be considered the highest priority cultural resources research program to pursue for the next three years. For example, ongoing work involves the classification of archaeological sites in terms of the origin of sediments being deposited at archaeological sites, barriers to aeolian deposition, and prevailing wind directions. The result is a 5-category classification based on a small number of sites and the goal is to expand the number of sites classified (n=13). A larger sample is definitely needed and if the project is approved work should continue on this project to better understand the multivariate nature of deposition. Understanding why these sites are not receiving sufficient sediment deposit to stabilize the sites is a complex process, and variances related to assessments are high, at least in part to small sample size. It, therefore, may be unwise to launch a monitoring program of these processes in 4.2 at significant costs without stronger empirical support for the original stated hypothesis and increased sample sizes. As this sample is increased, it needs to include not just archaeological sites but also other TCPs. Identification of erosion to other kinds of cultural resources needs to be explicitly integrated into the project. This was a recommendation of the PEP report (Doelle 2000) and also brought out in the legacy monitoring review committee report by Kintigh and others.
The greatest concerns with the project are (1) understanding its potential contribution to Tribes or the NPS in assisting mitigation strategies for archeological sites affected by dam operations; (2) how the plan objectives will be achieved for all sites given the very small sample of sites that are included in 4.1; (3) how data from the quantitative and qualitative analyses will be

integrated; (4) how can changing weather patterns affect application of potential results, and how could it be mitigated? and (5) LiDAR seems to be an integral part of the project but funding for the technology has not been secured (see p. 44). Many of these issues relate to insufficient science effort in plan specification an interaction with Tribes and manager. Some of these concerns might be alleviated by inclusion of more detailed information in the plan. For example, how many of the total archeological sites that are determined to be impacted by flow operations in the canyon have attributes expressed in this research? Although not disclosed in the project description, we assume knowledge exists of this number and it is a significant percent of the total to support the need for this effort. If the entire approach, i.e. hypothesis testing and monitoring protocols, is successful how will they assist resource managers—i.e. NPS and the tribes in implementing mitigation strategies? Again unless improved science design can be presented, this seems an area where funds might better be used to pursue management actions. A goal for the project might best be to produce information to help anticipate worst-case scenarios and develop management actions to mitigate irreplaceable losses. Further documentation of site classification is perhaps helpful, but information on specific vulnerabilities and how management could mitigate them seems just as important. The use of LiDAR to answer the question of whether NPS use of check dams to reduce erosion gullies relative to areas without check dams is a part of the project that would follow this reasoning. In principle, this may be an extremely important project to conduct for the cultural resources if the methods and models could be implemented. The science presentation is not sufficient to produce confidence in these outcomes.

Project 5: This project presents new program thrusts related to EPT absence/low abundance in the Glen Canyon/Marble Canyon reaches; continuation of work on invertebrate drift in the river and primary productivity monitoring in the Glen and Marble Canyon reaches. The Colorado River below the dam exhibits a remarkable absence and rarity of insect groups found in other river systems. This group of investigators face an interesting set of problems owing to interactions of variable flow velocities and temperature effects as causes for the low diversity and low productivity observed in the river below the dam. The possible solutions are also complex and difficult to test. The issue presents a tough restoration problem. Answers will be importantly related to food web interactions. Comparative insect drift studies conducted in river reaches above Lake Powell and those in the canyon below the dam may offer important insight about what is possible vs. what simply won't work due to life history constraints within the realm of current management practices of flow variability and temperature effects. A parsimonious outcome may be very helpful in evaluation of management possibilities and priorities. I strongly suggest that this is a very worthwhile effort. Management needs to know if and how the challenges of evolutionary history can be accommodated and, therefore, what expectations are realistic. Developing a bottom-up modeling approach will be helpful in evaluation of the top-down constraints apparent in the productivity of higher trophic levels. Overall, the monitoring of invertebrate drift and associated budget is in major part a continuation of needed assessments of habitat quality for main-stem native fish and rainbow trout resources. The proposal for sampling work in the upper Colorado River to provide the context for ongoing assessments in the CRE would help validate methodologies. These benefits must be weighed against the $141 K cost by stakeholders. The proposed efforts on primary

productivity to develop approaches to derive algae production estimates from dissolved oxygen measurements present opportunities for more efficient assessments of aquatic biology metrics. The new effort on EPT discussed above follows on scientist and stakeholder discussions of general hypotheses. From the five presented hypotheses, the selected hypothesis recommended for testing is the impacts of hydro peaking on egg mortality. As noted the flow experiment portion of the research (34 weekend days of low steady flow from May to August) is not required to develop preliminary evaluations of the hypothesis. With the emphasis that was placed on the need to evaluate effects of low flows on biotic communities in the 1996 EIS it is disheartening to have had the 2000 and 2011 low flow experiments and not have had effective monitoring in place to evaluate aquatic insects. Project elements 5.11-5.17 propose evaluations of conditions in other riverine systems, literature reviews, citizen science assessments, and laboratory experiments to develop initial evaluations of the hypothesis. This engagement of publics in the research effort has been demonstrated effective in previous program efforts and adds important extensions to the AM collaborative process. Clearly a need exists to evaluate elements that could contribute to absence of EPT in the system and flow variance seems a reasonable hypothesis to test. Laboratory testing of water temperature effects also seems reasonable to evaluate even if a selective withdrawal device is not in current management planning. A management action such as translocation might have merit as well, but as noted would be difficult to assess in this system.

Overall, Project 5 encompasses an elegant set of observational, comparative, and experimental studies on insect ecology and algal productivity. Presenting management implications after scientific rationale was very persuasive, and the citizen science dimension is praiseworthy. However, much hinges on the validity of H5, and it is worrisome that the proposing team offers very little evidence in support of that hypothesis. Simply put, given that conditions below GC dam are lousy for most aquatic inverts (cold water year-round, low particulate organic matter from upstream, no substantial riparian organic inputs, hydro-peaking creating daily scouring and monthly hydrological instability, deep/wide channel that may lack microhabitats with algae and detritus accumulations), why would anything except small insects with rapid life cycles based on filter-feeding or collecting ever use such habitats? And given the extreme flow variation from hydro-peaking, it is perhaps not surprising that chiros and simulids (both of which are often pretty sedentary) are forced to drift, yet drift in low numbers due to the combination of low productivity (cold, no food) and behavioral tendencies against drift. By extension, it seems pretty unsurprising that EPT taxa would not do well below GC dam. It is quite interesting that they seem to do better in other tail-water areas, but the proposal does little to show that shoreline desiccation from water level fluctuations is likely to be the major cause of low EPT. The practical dimensions (readily manipulated without hitting hydropower or other interests very hard, weekly cycle over warm season, etc.) are great, but additional pilot data, direct observations, comparisons to hydro-peaking regime at other sites, etc should be offered in support of a costly proposal.

Another limitation of the approach is that it focuses on singular mechanisms that could explain the lack of EPT species below GC dam. Never did stressor synergies come up, despite the fact that GC dam clearly imposes three unnatural conditions: cold water, low turbidity, and large numbers of visually-oriented insectivorous fishes (trout). Is it really more likely that a single

stressor has extirpated sensitive insects than a synergistic combination of stressors (scour, low food resources, high predation, cold, and maybe also too few wetted oviposition sites)? Indeed, it was surprising that habitat limitation for larval insects was hardly mentioned. Many benthic insects require solid structure with interstitial spaces to thrive (sand and silt have more limited faunas), so it would be helpful to hear more about substrate patterns from the tailrace downward. Perhaps these concerns can be addressed by the proposing team by providing some details from the data that they already have in hand (e.g. dealing with temperature, substrate, and hydro-peaking amplitude in the comparisons indicated in Fig. 1), along with providing some additional details on drift netting to demonstrate that EPT are not just being missed by the nets.

Life history issues received less discussion that expected; midges and blackflies are small and develop quickly, and are talented filter feeders and collectors rather than scrapers (like many EPT taxa). So it seems there could be an important role for trophic ecology, as well as general habitat flexibility that is well known for small insects like midges and blackflies that are short-lived (whereas most EPT are likely to be uni- or bivoltine in rivers that are cold year-round) and often found in low quality streams. The oviposition site information presented in table also suggests that these flies may be more flexible than most EPT taxa in that regard.

Finally, despite the elegance of the proposed experimental manipulation of dam discharge (which is a great idea), it was difficult to assess whether May-August is a long enough window to see life-cycle completion (the basis for the multigenerational amplification argument offered in opposition to a favoring a longer low-fluctuation period) leading to a population-level response. Given the unnaturally cold temperatures below GC dam, the expected growth rates may be too low to allow much response. This could be calculated easily from existing knowledge of midge secondary production, generation times, and temperature dependent growth. Such an argument would strengthen the case for the potential for this novel manipulation to unequivocally resolve whether oviposition site limitation is the core problem.

Project 6: This project presents continued main-stem monitoring of HBC populations, RBT, and other native and non-native fishes represents maintenance of long term assessments of a resources considered critical to the AMP in understanding native and non-native fish dynamics in the system .It is unclear how monitoring is a conservation measure, but rather should be justified by reference to ESA or other administrative guidance. An extensive sampling effort has derived insights about distribution and abundance of humpback chub. Much the same is true of rainbow trout. Much less is known about many other non-native fishes and, more importantly, their interactions with native fishes. The SAs in their review of the 2013/14 Plan supported improved methodologies and assessments, many of which are continued in the 2015-17 Plan. An important factor in effective continued AMP science and management activities on both native and non-native fishes is the collaboration of GCMRC with federal and state agencies and tribal resource specialists, which is very evident in these projects. Regarding project element 6.8 on the Lees Ferry Creel Survey, we would encourage funding of this survey in 2015 if the recreational angler survey is to be performed that year, unless the recreational angler survey will collect that data. It would be extremely valuable to have creel census data in the same year as the angler survey so that an objective measure of catch per unit effort could be related to angler satisfaction and values. Another concern is whether $25,000 is sufficient for the creel survey. Seems unlikely. Also why is USGS charging burden on Cooperators non-USGS dollars? This seems counter productive to get cooperators to provide funding for these programs.

Overall this project is developing well in its major obligations and offering creative approaches for additional effectiveness. We suggest that the information from these assessments be integrated with other studies to help develop an understanding of multivariate factors that influence HBC .

A major shortcoming of the proposal document was the lack of concrete evidence from the abundance of past work provided to justify the approaches proposed for FY2015-2017. For instance, otolith chemistry is proposed without any clear statement of the scale and species for which it has been proven in this system (despite two citations that appear to provide exactly what would be needed). Oddly, otolith chemistry was not even mentioned in regard to brown trout tracking. Instead, a rather speculative analysis of color phenotypes is proposed, with little apparent evidence that existing observations suggest differences within the LCE. With regard to the SWEF effort and other monitoring, the background section makes passing reference to upstream movement from Lake Mead by non-native species, and increases in abundance of chubs, yet substantiating details of these patterns are not offered. This gives the reader the sense that monitoring is being conducted but rigorous analysis of the results is lacking. That sense, which hopefully is not accurate, raises questions about the value of monitoring even though the relevance is clear.

The PIT tracking at aggregations and extension of that approach to guides is a great idea, but it would be worthwhile to specify which parts of the river are assessed regularly by PIT reading and which are not. It is clear that the fishery biologists have an intuition for important areas than might be overlooked, but it is less clear whether that is based on a systematic assessment that could turn up additional target sites for the work. Will these data, and the new CPUE data (p217), be comparable enough to older datasets to rigorously test whether there are more chubs today than before, and how much they move?

With this and the other fish-tracking projects, it might be worth considering citizen science reporting based on distinct physical marks that anglers could recognize easily if they hook a chub while trout-fishing. For instance, a small V-notch in the dorsal fin crossing several soft rays heals rapidly yet leaves a long-term mark that is hard to miss, and could be applied only to translocated fish. That would facilitate angler reporting of translocated fish, since they will not have PIT-readers. The survey of exotics extending all the way to Lake Mead during spring is a worthwhile addition, since many of the invasive centrarchids and percids are quite mobile in the spring as they look for spawning habitat.

Project elements 7.1-7.5: These projects represent a very focused and complex assessment of adult and juvenile HBC population variance in the LCR and its confluence with the Colorado River. The multiple projects developed over time are attempting to both evaluate and confirm factors relating to habitat, competition, predation, etc. that contribute to population variance in HBC juvenile and adult fish. This is recognized as a critical element of the AMP. Results from this effort over the past three years have been extensive with abilities for modeling success greatly enhanced as referenced in the recent LTEMP efforts. Publication of new modeling approaches and their capabilities in contrast to existing and past modeling efforts will be important to maintaining confidence in all modeling efforts in the AMP. Continued work on the Asian

Tapeworm potential impacts to juvenile fish is important. The CO2 issue and other water quality dimensions in the LCR could become more extreme over the next two decades if projected dry warming trends persist. The studies to evaluate the effects of CO2 in LCR water, the role of water temperature on the extent of Asian fish tapeworm effects on juvenile humpback chub, and the potential for Bioelectrical Impedance Analysis as an evaluation of condition factors are laboratory studies designed to answer questions about physiological ecology of fish and may or may not pay a role in growth rates and population dynamics. The recent advances in modeling are associated somewhat with focused information needs of the LTEMP/EIS process with limited exchange with the AMP processes. Proposed capabilities of the model certainly seem to warrant proposed expenditures at the levels proposed. The approach creates a holistic picture of variability in humpback chub population dynamics and movements between the LCR and the main-stem of the Colorado River. As a result of both the laboratory studies and field monitoring, the work addresses interactions of young-of-the-year chubs and rainbow trout. However, it would be helpful to see some graphics of past results to demonstrate the inspirations for the next round of work proposed at $1.6M/yr. For instance, illustrating the documented variability in juvenile outmigration rates, fall survival rates in the main-stem, and shifts in population size structure, etc. would help connect the new work to intriguing patterns in the existing data.

In light of the patterns indicated in the opening of the Scientific Background (p240), structural equation modeling would be an ideal way to fit these data if the path diagram can be kept simple (which is necessary given the low number of years in the dataset). Jim Grace at USGS in Louisiana might be willing to help with such an analysis.

The advances in modeling are promising, but what are the quantitative consequences of the uncertainties in juvenile production and outmigration? Offering the reader something more concrete would help strengthen the case for further data collection and model development. For instance it was striking that temperature was rarely mentioned as a constraint on chub performance (only in 7.5 on p248), yet it would appear easy to look at water temperature records longitudinally and across years in LCR to test effects on chubs. Since water temperature is invoked as a key difference between the main-stem and LCR, digging into the LCR temperature data would be useful. Similarly, could inter-annual variation in water temperature be an important influence on tapeworm prevalence and impact on chubs? The potential effect of warming in the main-stem is noted, so presumably that could apply to LCR too.

The idea of spawning gravel limitation in LCR is interesting (p248), and worrisome in light of projected lower discharge in the future. Is there potential to use pumps to power-wash existing gravel beds, then use automated cameras to document whether chub preferentially use washed sites for spawning? The CO2 issue is also a nice element, but the background statement and approach appear to differ in indicating substrate vs respiration as the pathway of impact. Which is the case? Are there any field observations that suggest chub performance is compromised by high CO2 (e.g. during recovery after electrofishing)?

Finally, the proposal repeatedly mentions triggers for non-native species control, but never states the link between data collected and such triggers. What are the barriers to using new data to pull such triggers, and how will the proposed extension of basic monitoring help to

overcome those limitations? In other words, how bad would things need to get, and will the 2015-17 effort be certain of detection such a change?

Project 8: This project emphasizes AM processes related to implementing management actions, monitoring and revised actions to accelerate the learning process. These non-native fish control and native fish translocation management activities appear to be proving effective and should be duplicated in other science areas as possible. Proposals for expanded efforts on invasive species in the entire LCR watershed are critical. Invasive species transfer down the system would be expected to increase in importance in the future, especially if changes in climate and more intense weather events occur. Also recommended is citizen participation in evaluating LCR water quality changes related to land use practices. Extensive development is occurring in the upper drainages with increased expectations of pollution related to municipal water treatment, rural single-family housing, and small rural industry. The SAs proposed the expansion of collaborative adaptive management activities in the 2013-14 Plan as central to managers' success in understanding risks related to water quality in the upper LCR watersheds. The PEP scheduled for 2016 is most important and should incorporate questions related to system wide risks to water quality in the upper LCR watersheds.

While there is no question the non-native removal is a key tool, the proposal should make it more clear how many trout can be removed a year, and what kind of impact that would have on their overall abundance. Of the fish removed in the past, what proportion are big enough to eat small chubs? There is mention of relationships between removal needs and water temperature; what have the years of data since Coggins 2011 taught us about the strength of that relationship? It would be helpful to know whether chub (positive effect) or rainbow trout (negative effect) are more temperature sensitive, since that helps to frame how the future balance between trout fisheries and chub conservation can be struck under climate change. Is there potential to encourage recreational anglers to fish the Bright Angel area for brown trout, with a mandatory culling rule? That could potentially yield much higher removal rates, imposed year-round at low/no cost, as well as engaging citizens in the control effort.

There is also a need to be more clear about the success of past translocations. Does PIT monitoring indicate survival of all/most fish translocated since 2008? In terms of genetic assessments of chub aggregations, microsats may no longer be the best method; SNIPS or extensive sequencing is now within reach to gain very high resolution. These methods can now be outsourced at low cost, allowing investigators to focus on interpreting the data. If population sizes are small enough above Chute falls, detailed parentage analysis may even be possible for translocated and naturally-spawned fish.

Project 9: This project incorporates the ongoing monitoring efforts to evaluate status and trends of rainbow trout resources. Project 9 is aimed at filling a large and critical knowledge gap, which has significant implications for humpback chub and recreational angling. The hypotheses proposed on p. 281 seem reasonable and important to test. Overall the individual proposed projects within Project 9 seem to have some capability to address the key issues and hypotheses sufficient to warrant the amount of budgetary funds involved. It also proposes multiple new studies to evaluate and define key drivers that can impart change in RBT population size, movement, survival, reproduction, size, and condition. All of these factors are hypothesized to

have some effect on individuals and populations, and previous evaluations of varied scope have occurred in the program. Some assessments are extensions or add on analysis to evaluations approved in the 2013-14 Plan. To reiterate the point made in Project 6 we believe that discontinuing creel surveys may be ill advised in the short run. Presumably while the new mark-recapture methods for estimating trout populations are being developed the creel census will continue so that a relationship between the two can be established that will be useful for back-casting trout populations using the new method in order to have a consistent time series. Sport fishing for RBT in the Glen Canyon NRA is an important social benefit of the tail-water from GCD and brings with it many socio-economic issues. RBT growth rates have declined and abundances have become highly variable. Although downstream migrations and reproduction by migrants are still not well understood there should be continued effort to expand learning regarding relationships of Glen Canyon and Marble Canyon populations. Continued efforts are also recommended in providing better definition to HBC/RBT predation relationships. The capacity of this species to expand its habitat quickly on potential warming water should receive increased attention. Management of operations can affect this species and attention to water level management, experimental flows, and related food-base efforts need to continue. Although this premier sports fishery is a critical resource to maintain, it also could be a significant threat to HBC. Given that warmer water is probable for this river over the next two decades yet no management action is proposed regarding a selective withdrawal device, HBC at the LCR could receive threats from RBT and other predators in the river. It would be important for managers to understand how quickly RBT populations could expand in warmer water and their predation expectations.

Most of the specific project elements build on earlier work, and the proposal would be strengthened considerably by drawing more directly on evidence from previous data collection. For example, in element 9.4, what has been learned from all the past drift netting and stomach content analyses? If there is not strong evidence of selectivity, then the morphometric dimension of this study might be difficult to interpret.

In addition, is there a way to engage anglers as citizen scientists in the effort to understand trout movement patterns? Assuming that angler efforts range more freely in space and time than scientists can, then creating a physical mark (adipose clip or v-cut in dorsal) on trout caught in one place (e.g. tailrace of GC dam) could enable a small army to contribute to monitoring trout movement. Alternatively, can otolith chemistry approaches be used in the trout studies?

The lipid approach in element 9.3 could be powerful, but lipid storage probably is not the primary shift in resource allocation with trout size. Rather, the primary shift would likely be toward gametes rather than somatic growth (including lipids). Thus, the prediction of differential allometry of lipids in small v large trout may not be valid as proposed. The lab studies of turbidity effects in element 9.6 will be very useful, but under field conditions can differences in detection distance overcome density- dependent encounter rates and size-dependent detection rates? The literature values could provide a rough answer to that question prior to doing the work of lab manipulations. Similarly, for comparing different tailwaters, can all the other factors which differ be controlled for to allow strong inferences about the effects of temperature or other factors?

Project 10: The project focuses on Glen Canyon Dam rainbow trout tailwater fishery. This project nicely integrates information from other projects (2, 3, 9) together to address the issue of where does the trout tailwater fishery end. The project will evaluate select shoreline sites at flows below 8000 cfs in Glen and Marble Canyon to provide to ecologists evaluating food base definitive information of channel geometry and bed grain size. The project has been discussed by GCMRC at two TWG meetings and results from stakeholder requests for assessments. Introduction of rainbow trout in this system has been a huge success, which that now is sometimes expressed as a curse of riches. Biotic and socioeconomic issues surround management of the RBT. The project proposes a novel and potentially important approach to building a bridge between the detailed studies of river sediment particles and those that change habitats and productivity in support of desirable ecological conditions. Restated, this means that in developing the adaptive management approach at the GCe scale, there is need for more than sole attention to building beaches for campers. Before the dam, there was a very large annual flood. Now there are the realities of diurnal and seasonal flow fluctuations plus those of the weather, and the HFE's that have shoreline effects analogous to sending a tornado down the canyon. So how can things change in way that benefit food web interactions? In other words, what ecological benefits would develop if there were little or no HFEs for a significant period of time? This echoes the voice of conservationists in support of stable flow conditions and that recognizes climate change as an ominous reality. The scientists have the capacity to estimate hypo-symentric flow inundation effects. Unfortunately, I wonder if they have changed things with many HFE's in ways that do not provide a baseline condition. In ecosystem studies, these are known as reference or control systems that develop during time of the Holocene. It may take some time to build a reference condition that creates the habitat required to enhance like life histories of the invertebrates, etc. If they succeed, fishes will eventually find the prey resources. If gravel conditions develop to the point where fishes will spawn successfully, then monitoring efforts might provide evidence of success. The comparative study proposed by Project 9, and perhaps the drift study offered by Project 5, could offer some guidance in planning derived from tail-water sites where a regular pattern of seasonal or daily fluctuations has a history different from that of the GCe events and HFE effects. The SAs strongly endorse the potential learning from this unique project. If the project scientists implement strong collaboration in data gathering stages and design with Project 5, 9, 11 and especially 12 scientists, it would offer the type of opportunities in science and management integration that can advance science and learning at a much more rapid rate. Mother Nature has a time clock that is modified on an evolutionary scale with internal sensitivity to ecological interactions. That's how the GCe operated before the Anthropocene before when Glen Canyon Dam was constructed and the march of invasive species began.

Project 11: This is a continuation of the new vegetation monitoring and assessment programs supported by the SAs with proposed revisions in the 2013-14 Plan. River corridor vegetation dynamics associated with dam operations can affect physical, biotic, and cultural resources of concern to the AMP. The project uses the generally equivalent background evidence argued about Mother Natures' clock cited in Project 10. Stop the horrendous floods, remove the sediment, make the water cold and clear, and add in the invasion of tamarisk and its leaf beetle plus others that have come on regulated waters with human intervention at all watershed scales. The place was different then and it took millennia to set that clock. And, humans are constantly intervening in its current state in all the physical, biological, and social resources. That is where Mother Nature cast upon the system a more predictable slow changing set of

changes, humans invoke less predictable and much more rapid changes that the system must adjust to. Yet, it is the system we must understand if we plan to manage it while it continues to change toward some new equilibrium. An equilibrium that humans through our own actions keep in at least a moderate a state of flux. Several previously abundant native fish species are now gone. That's an indicative reality. While restoration to some state yet to be fully defined is applauded, it's difficult to imagine or forecast how successful those efforts can be. This is not to discourage the efforts, but the relative successes may offer some guidance to managing rates of change for the creatures and invasive species now known in the aquatic habitats. The experience with terrestrial life forms is an ongoing result of efforts by this group. In many places some but not all of the invaders become established, flourish to levels of strong negative effects on natives, then decline to lower and somewhat stable and lower levels as diseases, parasites, and consumers increase their effects. Battling the invaders is sometimes successful and sometimes not, while the invader persists at lesser levels. Many, many cases like that are known from the literature. The high priority research category shows direct attention to the interaction between hydrology, vegetation, and sediment dynamics. This should be highly relevant to the sedimentologists and the prospects for collaboration with Projects 9 and 10. The SAs support this effort as it has great potential for providing guidance in integrative science and management actions.

Project 12: This project evaluates dam effects on distribution of culturally important plants. This is an important step in science toward policy issues related to tribal traditions and culture, i.e. plants deemed important to Tribes for reasons related to religion, traditions, and culture. There does not seem to be a plant scientist on this team as one of the Investigators. That would seem be important given the basic science questions being asked. However, this project seems to reflect the interest of tribal members in understanding dam management impacts to plant resources of specific importance to tribal members. We are not convinced this project is specified effectively and there are several problems with this project that need to be addressed. First, one of the leading scientists is also working intensively on Project 4 and it difficult to see how effectively she will be able to do both especially since both projects seem to have critical design problems. Second, the project is severely under budgeted in terms of both time and funding. For example, in one day, the list of plants that are of significance to tribes will be identified. Even given the use of prior research this is impossible to do thoroughly in one day (and will condition everything that follows in terms of data collection and management recommendations). Third, there is little reference to the anthropological literature on TEK that could be used to help guide the research. Fourth, although the methods proposed include a mix of qualitative and semi-quantitative approaches it would seem possible for project members to collaborate closely with the collection of quantitative data to be collected in the vegetation assessment program (Project 11). This would further the goal of incorporating more TEK in all of the scientific projects, but would also provide explicit data sharing and discussion of plant community and individual plant distribution changes. The use of citizen scientists in documenting plants and their distribution, as used in Projects 3 and 5, for example, would be exemplary. There is a lost opportunity in this project to use multiple sources of data for analysis for what is an extremely important management issues. Cutting this project completely is unacceptable, however, because it is the only one that explicitly includes tribes in the research, and is one of only two that explicitly addresses cultural resources. However, concern exists that

appropriate science methodology are absent from both project 4 and 12 which are led by the same specialist.

We make several specific recommendations with respect to this plan to make it more doable as well as to ensure future duplication of effort. First, the project should take into account *both* plants and animals. Second, because the project is undoable at the level of funding requested ($35K), these funds should be used instead to fund the first phase of the project— a pilot project to convene a series of meetings to come up with the list of plant and animal resources identified as important to the tribes. This should also include discussion and planning for the implementation of the documentation phase of the plants and animals and their historic and contemporary distributions. That planning should include ways of taking incorporating citizen science and tribal members. In addition, that proposal should include ways of using existing and current data sources from other projects currently being conducted. Finally, this project will seemingly have significant difficulty establishing effect relationships, i.e. causation. In its rewrite perhaps a descriptive analysis should be considered instead.

Project 13: This project presents proposed socio-economic research programs provided through the leadership of GCMRCs newly placed economist. The proposed studies for 2015 for this project emanated in the SEAHG proposed and approved recommendations to the AMWG in 2011/12. Project 13.1, originally recommended by the SEAHG for 2012/13, was proposed for initiation by GCMRC in 2014 with carryover socio-economic funds from 2013-14 ($241K). The socio-economic research ties to GCDAMP goals (page 401), Strategic Science Questions (page 404-405), Core Monitoring Information Needs (CMIN page 405) and two Research Information Needs (RIN, page 405). This assessment of expenditures on recreational fishing and boating in the CRe will be accomplished from surveys originally proposed by NPS. Inclusion of several regional economic specialists in the analysis will assist the project. Project 13.3 represents a proposed SEAHG project for initiation in 2012/13 on tribal resource values in the CRe. It was recommended to the TWG by the SEAHG in 2013 as an originally approved program by the AMWG that is currently not being planned by any agency or group of the AMP. The approaches proposed by GCMRC are similar to general methods originally proposed by SEAHG. Use of focus groups for initial assessments and the Choice Experimental Method are recommended approaches for these types of assessments. Project 13.3 is a project proposed and approved in the SEAHG recommendations to assist in improved decision analysis by the TWG and AMWG. In all of these projects the approaches originally recommended by the SEAHG to the TWG are generally being proposed.

- Project 13.1. The trout fishing study is an achievable beginning and is targeted at a natural resource (trout) that has become more of a priority over the last few years. The whitewater boating study in GCNP provides a critical update to a very old economic study that past research suggests is sensitive to flow regimes. P. 403. We think the two recreation hypotheses put forward are foundational hypotheses that are critical to test. However, we would suggest it might be worth considering an additional hypothesis: that the value of angling Glen Canyon and whitewater boating in Grand Canyon NP will have increased over time due to changes in “improved” dam operations over the last two decades. Of course a one-year survey may have difficulty teasing this out from other events, but we think it would be worth at least considering. The recreation angling and

whitewater boating recreation economic studies used widely accepted methods and largely replicate earlier studies so as to provide comparable data so there should be a very high likelihood of success. The one concern is that the budget for Project Element 13.1, pages 413-414. We do not see funds for the actual printing and mailing of the surveys in this budget. Is AFGD or NPS picking up this cost?
The true merits of the recreation values assessment are revealed by an unfortunate history of very limited research and advisory efforts by socio-economists over the past three decades. In several past SA reviews concerns have been expressed regarding this program shortfall. In reality only flow event related hydropower financial impact assessments by Western Area Power have afforded any glimpse of this programs impacts on resource economic values. The economic surveys that will be used to determine regional expenditures, trip quality for anglers and tourists, direct recreational use values etc. are important to decision making on the Lees Ferry sport fishery. Relating these values to differing operations of GCD will also be valuable. What will be important is to differentiate short and long term operation effects on socioeconomic factors. General approaches proposed are common practice in economic assessments and related outcomes are needed for this program. Costs for the assessments appear reasonable given the diverse expertise of specialists proposed.

The SAs strongly agree that a formal program to assist the AMP in development and use of decision methods is needed. This has been proposed in several SA reviews and the subject of a brief white paper by the SAs on the subject, "Evaluating Decision Support Methods for the GCDAMP". As an outcome of this effort two attributes of preferred DSS by TWG were determined to be user friendly more simplistic models that are easily understood and models that can be readily used by a group in real time, i.e. meetings and workshops. There is an extensive base of literature to support this area, several of which are noted in the above mentioned SA report. It was a recommended area of pursuit proposed by the SEAHG and endorsed by the AMWG. GCMRC in discussions with the SEAHG/TWG has proposed this as a collaborative effort. The SAs encourage that approach as a collaborative effort with the TWG and SEAHG. The goal of the Decision Support System to integrate the physical and biological sciences with economics and address uncertainty using a dynamic model is an important one. (p. 402) However, Project Element team for 13.3 would benefit from seeing the ongoing work of Sandia Labs who are developing a much more general model of the Glen Canyon-Grand Canyon hydropower-natural resource system. It is proposed that this effort would benefit from discussions with Dr. Tom Lowry, systems analyst with Sandia Labs. . Framing (pages 408-410) of the DSS as a cost-effectiveness analysis of humpback chub recovery is a good choice that will increase its acceptability among AMWG and TWG. The reliance on cost-effectiveness in analyzing options for endangered species recovery have been used successfully in the past as well (e.g., spotted owl recovery).
Development of approaches for assessments of Tribal values is important. Although recommended by the SEAHG/ TWG and approved by AMWG, the activity has not been initiated by any entity of the AMP. As such this proposal is encouraged by the SAs. However, how it is accomplished, i.e. a necessity for full engagement of the Tribes in all project elements, is most critical. The manner in which the Tribes hold values must be

first determined through the focus groups proposed. Some resource values expressed by the Tribes may be wholly spiritual, making pursuit of economic values incongruent with Tribal desires. We suggest GCMRC evaluate the work by Failing and others on First Peoples of Canada for more insight into this issue (Failing et. al. 2007).

Project 14: This project overviews administrative costs for the Center, which generally tracks from costs in the 2013-14 programs supported by the SAs. An area of administration that has received some support in the past but appears to receive less support in this plan is the continued need for adaptive management assessments and planning. The EIS/LTEMP will be complete in the 2015 period and will have established significant new policy direction for the AMP. The AMP of necessity must develop new strategic and operational direction to respond effectively. All parties have struggled in this review in trying to sort out what of the total program is long term monitoring efforts that should not be changed in annual reviews, determining which science efforts need to be addressed with PEP assessments, and what alternative programs are best to pursue. We agree that trying to accomplish this without EIS policy guidance would be ineffective. On the other hand when the EIS is complete this will represent a significant administrative need. This is eluded to in several places but we do not see the needed budget emphasis on the effort in 2015 or 2016.

SCIENCE ADVISOR RECOMMENDATIONS

Without question this plan is the most comprehensive and well developed plan ever produced by the GCMRC, including presentation of science linkages to goals, response to Asst. Sec. direction, and stakeholder guidance in specified information needs and critical questions, as well as science review panel concerns. Over the years the stakeholders and managers have asked for increased inquiry based on challenges they face. The wealth of newly created knowledge is almost to the point of overwhelming stakeholders as to how best they can apply this knowledge. This program is about implementing management actions (dam operations, non-native fish control, habitat restoration, translocation of native fish, etc.), and following with iterations of science efforts (monitoring, research) to learn if outcomes can help us reach goals related to desired future conditions. Many iterations are necessary, some lasting 5-10 years, to accomplish desired knowledge and management outcomes. The management and science needs in this AM direction are very challenging with many surprises, and no absolute final independent answer for one resource or even several interacting resources will exist. Instead they are time-space- environment-bound with all factors in constant flux.
The proposed research and monitoring plans capture sufficient complexity of the CRe to be meaningful, and inordinately complex, demonstrating great progress toward integration of understanding across methods and disciplines. This is precisely what is needed to address the GDAMP needs. No plan that addresses this level of physical, biological and social science complexity can be perfect, and both the SAs and the stakeholders are in this process recommending improvements they feel might help the research center on development of future plans.

Many recommendations are mentioned throughout the review report. However, some points deserve second mention here and additional elaboration.

- The complexity, need for and quality of the information in this report is deserving of an executive summary that duplicates in summary form the primary report elements. Although a challenge, it should not exceed 25 pages, with significant dependence on figures and graphs.

- The introduction to the report should be modified, so that it provides a useful guide to the reader. The report is so massive that it needs to have the guide so that it can be easily followed. In chapter 2 we suggest that the authors present a summary of the overall budget either in the introduction or as a new chapter. This should be upfront in the report and consist of only one page. The detailed budget appendix should be retained. It would also be useful to present a short discussion about the collaborative budgeting process, and how decisions were made about allocations among projects, and costs within a program.

- The science advisors feel that significant improvements have occurred in interdisciplinary cooperation and integration of the monitoring and science across programs. Project 10 exemplifies this shift. However, we also note areas where it might be improved. Even where that collaborative process was not mentioned it is intuitive from the list of scientists involved in each project. We support this trend and encourage continuation in the future.
- The SAs feel that this plan although large needs to incorporate additional elements. Missing is an agreed to longer term management and science strategy, agreements on critical management actions and stakeholder AM actions as well as agreements on critical monitoring activities. The SAs propose consideration that in 2016 this plan be revised to incorporate agreements on all of these critical elements. In this manner the GCDAMP will have one major working plan that incorporates strategic and operational management/science/monitoring programs of all active entities. The SAs feel that this could be accomplished with one or two additional chapters to the existing plan.
- The SAs feel that the AM paradigm as applied in the AMP is working well. However, this belief does not therefore mean adaptive management in the GCDAMP does not need improvements. The GCDAMP has many accomplishments which far outweigh its inabilities to gain solutions. It has integrated science and management in ways that address key resource uncertainties and advances understanding about resource dynamics and interactions. However, this program has applied the AM paradigm for 18 years and AM processes should be reviewed and evaluated for change. This assessment of "double-loop" learning in AM programs , i.e. what do we now understand about the shortcomings of AM in this venue that should be modified, is critical to continued use of the management process.
- The plan does not describe well system level uncertainties nor how the management actions will address these uncertainties in a concerted and planned effort. These uncertainties fall into categories such as impacts of climate change and re-connecting to river system issues and opportunities upstream and downstream of the CRe in many biophysical and socioeconomic processes. Rather, the continuing emphasis is on monitoring and assessing actions that are in a constrained ecosystem definition largely derivative of political and legal mandates within that system. Perhaps the AMP is fully aware of this need and is developing both policy and strategic direction apart from this document. Some of this need is surely included in the EIS/LTEMP that is being developed external to the GCDAMP process. Some reference is made to the potential need for science and management revision to this plan once the EIS is completed. That being the case there should be both recognition of this need and budget placeholders to perform the needed work since it is known to be imminent. One potential approach to mitigating this expected need is collaboration with basin wide entities in evaluating threats and opportunities for the basin and

developing a system wide model that can adequately address policy, management and science planning needs.

- It is not clear that optional management treatments were considered in each of the projects. Due to the applied nature of GCMRC research, the SAs agree that it would benefit the proposal authors to create a project proposal template that includes separate sections for scientific rational and management implications under each element, much like that presented in the present proposal for Project 5. Making those linkages explicit would help to promote creative thinking about implementation as research projects are being designed. When such context was presented, in most cases only one management dilemma or scientific approach was presented for resolution. Obviously several options can exist which give more/or less benefit in learning and management resolution associated with greater or lesser costs. Overall, the entire AM cycle will move faster if science-based 'solutions' are always presented with their pros and cons, and alternative approaches to the same end are discussed more openly. In the guidance provided to proposers in the next funding cycle, we also feel there is a need to require more coverage of past results or other concrete context for requesting funding. There is mounting pressure for accountability, and some of the proposed projects with the largest budgets also offer the least rationales for further work. The SAs are not skeptical of the value of these efforts, but rather wish to encourage the proposers to make the most of their existing data in designing the next generation of studies.
- We would ask the AMP program and GCMRC to consider evaluating the social and organizational learning that is part of adaptive management. A few of the projects address this issue, such as the traditional ecological knowledge program for plants. But a more direct overview about modes of learning, repositories of learning (more than web available data or GIS files), would help facilitate the collective understanding and the adaptive management program. In the past, projects such as the State of the Colorado River Ecosystem (SCORE) report, knowledge assessment workshops, and integrative conceptual modeling have been very effective at fostering and facilitating institutional learning within the entire AMP. It is true that GCMRC produces an excellent workshop of its annual accomplishments and therein reflects somewhat on where they are related to uncertainties and learning as referenced to DFCs. Using that knowledge to specify and chart a future direction, i.e. strategic management with stakeholders and managers as noted above is critical.
- The overall budget in the 2013-14 plan seemed suitable from a short term perspective, but the SAs proposed that it might be insufficient in the longer term. The review of the 2015-17 budget creates more of an alarm for the long term for several reasons. These budgets have and are using up savings (carry-overs), base support from USGS, lower cost rents to maintain facilities, etc. to continue to respond to the very complex needs of the system as expressed by stakeholders and managers. Although excellent progress is being made in physical and biological sciences with promise in social sciences, much uncertainty still exists regarding very critical biological resources in the system. These science areas are also the most expensive to pursue. Already some critical areas of research cannot be sustained at desired levels and other potential needed research cannot be started or must be postponed. With the clear knowledge that administrative costs will rise significantly, and the threat that warming water temperatures could require new research efforts we believe the 2018-2020 management and science cycle will be significantly under-funded. Several strategies seem available; increase funding from internal or external sources, define lower requirements and reduce management and science activity, reprogram internally, and evaluate lower cost alternatives. The AMWG needs to consider a management review of the entire AMP management and science program to address future budget needs as well as the AM structure and process being used.
- The SAs support the program administration changes recommended, including the POAHG, Science Advisors and Lake Powell programs. A history of the GCDAMP is something that should be

accomplished at some point. However, we propose future opportunities to accomplish that task when currently limited energies and resources might be better focused on other AMP issues. In the meantime extensive documentation is and will be available to the task. Transfer of the Science Advisor program to BOR seems reasonable. It will be critical that the independence of the group remains inviolate, that its tasks continue to be directed from the full AMWG and that it accomplishes its tasks as defined in its operating procedures. Transfer of the Lake Powell Program to BOR seems appropriate as its future accomplishments lie more in applying its science findings directly to management needs. In major part BOR is currently leading these activities. It will be important that the Lake Powell reservoir and future implications of its management be considered for a new program initiative to evaluate impacts to the Colorado Basin from climate change.

Project 1: The Lake Powell Program, especially in its new administration by the BOR offers great potential for management application of accomplished science. The SAs propose that if the total time and cost of analyzing all biological samples is large, small subsamples should first be analyzed to determine the expected value of additional learning. If it is low perhaps energies and funding should be focused on more rapid application of physical data and analysis to enhance existing and needing modeling efforts. The SAs also propose that a new initiative should be undertaken by the BOR in collaboration with GCMRC and other basin entities. That initiative should focus on basin system assessment and modeling of management needs to mitigate predicted impacts of climate change in the basin.

Project 2: This monitoring project provides critical data and analysis to many other projects and should be considered as an ongoing need over multiple planning periods, i.e. a core monitoring need especially as relates to evaluating impacts of climate change on water temperature. Remote sensing technology continues to advance and as in the past this project should continue to test new technology for application. Although it is mentioned that this project collaborates with programs in cultural resources, it is not made clear how that occurs. This should be made more specific. It is also important that the AMWG evaluate the level of information resolution it needs from this project as it relates directly to increasing costs.

Project 3: The SAs see this as a critical monitoring project and should continue to provide important data and analysis inputs to other important resource areas, including riparian and aquatic habitats and recreation beaches. It provides the critical basis for enhancing modeling capability to both assess sediment balance in the system and predict flow implications to sandbar maintenance over time. Not emphasized is the potential capabilities of this project to integrate with data recovery and assessment related to archeological sites which needs to be included. Modeling efforts should proceed in collaboration with sandbar modeling efforts developed in the EIS/LTEMP process. Because of high sampling costs efforts should continue to adopt advanced remote sensing technology. High program costs dictate that AMWG continue to evaluate both the amount of information needed from this program and its resolution.

Project 4: The SAs feel this is an important project to consider even though it lacks effective science design and the small samples represented in empirical work have not fully validated approaches recommended. Several sets of information should appear in a revised project to help its full evaluation, including: How will it assist NPS and Tribes site mitigation approaches; improving sample size validate approaches; how will qualitative and quantitative data be integrated, etc.

Project 5: This program has developed critically needed understanding of food base in this system. In its ongoing efforts, management needs to know if and how the challenges of evolutionary history can be accommodated and what expectations in this system are realistic. Developing the bottom-up assessments and modeling approaches are helpful in evaluation of the top-down constraints apparent in higher trophic levels. The proposal for sampling work in the upper Colorado River to provide the context for ongoing assessments in the CRe would help validate methods. The mix of laboratory and in-stream experiments to probe basis for EPT existence/low abundance provides the type of science alternatives important to managers in their efforts to support broad based initiatives. Pursuing lab assessments initially to assist design elements of river based experimentation is applauded. Establishing proofs with river based experimentation will be difficult and longer term. The creative implementation of citizen science in these programs should be emulated as possible in other programs.

Project #6. The continued main-stem monitoring of HBC populations, RB, and other native and non-native fishes represents maintenance of needed long term assessments of resources considered critical to the AMP in understanding native and non-native dynamics in the system. Although continued assessments of new monitoring methods and enhancements of analysis modeling efforts are encouraged the project is most critical to advancing learning in this program. The SAs encourage funding of the creel survey in 2015 if the recreational angler survey is to be performed that year, unless the recreational angler survey will collect the creel census data as part of the recreation angler survey. It would be extremely valuable to have creel census data in the same year as the angler survey so that an objective measure of catch per unit effort could be related to angler satisfaction and values analysis. It will also be useful for back-casting trout populations to have consistent time series. Also, it is recommended to remove USGS burden on Cooperator dollars being provided to the GCMRC program. This discourages Cooperators from contributing to projects as it is essentially a tax on it.

Project 7: Elements 7.1-7.5 represent a very focused and complex assessment of adult and juvenile HBC population variance in the LCR and its confluence with the Colorado River. Due to the relative importance of the endangered HBC resource in the AMP program, LCR primary habitats for the species in this system and assumed predator interactions of RBT and juvenile HBC this program must continue to receive primary emphasis. Results over a short time span have yielded significant new understanding related to habitat, competition, predation, etc. Most important are the added values in enhanced modeling. Because of dependence on these modeling outcomes in several management applications it is recommended that publications in process contrast both improvements in model design and predictability of the new model to existing model. The studies to evaluate the effects of CO2 in LCR water and the role of water temperature on the extent of Asian tapeworm effects on juvenile humpback, may or may not play an strong role in HBC growth rates and population dynamics. Due to potential threats to water quality and non-native species introduction in the upper LCR system, a feasibility assessment of potential strategies to minimize impacts from these two factors should be evaluated. This project offers the greatest opportunity to evaluate predator interactions of the two species and should continue to be a significant element of this research.

Project 8: The success in more rapid learning noted for management actions in both non-native fish control and translocations of HBC should be continued in other resources and especially as

applied as collaborative efforts of science and management groups. Assessing most feasible approaches for management/science collaboration in EPT restoration, LCR water quality mitigation ,camping beach reclamation, gravel bed restoration, native vegetation restoration, archeological site restoration etc. could reveal different and more effective joint activities than are currently pursued.

Projects 9 and 10: These projects present the continued monitoring efforts and related research on factors that can induce variances in populations of this sports fishery resource and new investigations on implications of lower flows to critical reproduction habitats, and potentials for downriver migrations and establishment of new populations. This program is important to its contributions in maintaining a healthy sports fishery, but also to greater understanding of these populations ability to transition downstream and impose greater threats to native species in the system.

Project 11: This project proposed and supported by the SAs in the 2013-14 program represents new science in the river and offers promise to several other programs including camping beaches, aquatic habitat, food base, etc. It affords collaborative opportunities for NPS native plant restoration and camping beach reclamation projects.

Project 12. Concern exists regarding effective science design and specification of this project. There does not seem to be a plant scientist on this team as one of the Investigators. That would seem be important given the basic science questions being asked. If budget is a constraint, perhaps a specialist from the NPS could join the team in a collaborative capacity. Project 12 needs to be rethought because it lacks an effective design and is severely underfunded. It should be made more comprehensive with both more anthropological approaches to TEK included as well as integration with the data collection possibilities expressed through other projects with a much higher budget and/or use the funds requested for a pilot project instead. Citizen scientists could also be involved with the research to help with current and historical documentation. In the project rewrite perhaps the project should be specified as a descriptive analysis.

Project 13: Three important programs are presented in this project. All are recommended by the AMWG. Key elements are presented for each that support information needs sought by the AMWG. More emphasis on an initial pilot project to evaluate multiple decision analysis approaches with the TWG should be considered for 13.2 The socio-economics project team undertaking the Decisions Support Modeling (Project 13.3) should see the ongoing work of also consider collaboration with Sandia Labs which is developing a much more general model of the Glen Canyon-Grand Canyon Resource System. Perhaps both efforts would be needed, but collaboration would seem important. Dr. Tom Lowry, a system analyst with the Sandia Lab, is the leader on this project. As proposed 13.3 initial use of tribal focus groups to assist in program specification is critical. The SAs encourage continued engagement of tribal representatives in all stages of the project.

RECOMMENDATIONS ON REVIEW OF GLEN CANYON DAM ADAPTIVE MANAGEMENT PROGRAM TRIENNIAL BUDGET AND WORK PLAN: FISCAL YEARS 2015-2017

GCDAMP SCIENCE ADVISORS

SCIENCE ADVISORS

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- LANCE GUNDERSON, ADAPTIVE MANAGEMENT, EMORY UNIVERSITY
- JAMES KITCHELL, FISH ECOLOGY, UNIV OF WISC
- JOHN LOOMIS, ECONOMICS, CSU
- PETER MCINTIRE, RIVER ECOLOGY, UNIV OF WISC
- BARBARA MILLS, ARCHEOLOGY, UNIV OF AZ
- ELLEN WOHL, GEOMORPHOLOGY, CSU

A STATEMENT ON PLAN DEVELOPMENT AND QUALITY

- RESPONDING TO STAKEHOLDERS; Goals, strategic questions, DFCs, collaboration, information needs
- Providing science basis, design and methods, project detail
- Science Integration; cost effectiveness
- Management/science collaboration
- Quality; demonstrated worth through publication

REVIEW APPROACH

- General comments on overall plan content and structure
- Specific comments on project methods, design, outcomes
- Recommendations related to general and specific comments

General Comments: Plan Structure

- Need for executive summary addressing all major plan elements
- Introduction that provides basis for content, AM purpose, relationship of management/science in AM process; outcome application to management
- Need for road map, guide or pointers introducing sections, budget linkage, linkages to other sections etc.
- One page budget summary in introduction and detailed summary in appendix

GENERAL COMMENT: PLANNING

- The 3-year plan is a critical improvement for tracking longer term science programs and projects in short term; detail provides excellent working document
- Although a strategic plan is needed, SAs feel it is best incorporated in this plan as separate chapter giving the overall plan assessment capability of long term accomplishment
- Although a core monitoring plan is needed it is best incorporated in this plan as an integrated component with research activities
- Some, but not all directly related management and science activities of management groups (NPS, USF&WS, BOR, AZG&F, Tribes, etc.) are incorporated. SAs encourage documenting these activities in this plan

GENERAL COMMENT: IMPLEMENTING ADAPTIVE MANAGEMENT

- Both AM processes and outcomes are demonstrated effective in this program; i.e. HFEs, Non-native fish control, HBC translocation etc.
- AM processes in this program have been in place for 18 years. SA encourage AMWG recommend a review to evaluate potential improvements (i.e. double loop learning).

GENERAL COMMENTS: BUDGET

- Place one page budget summary early in text
- Overall budget in short term seemed sufficient but marginal in review of 2011-12 and 2013-14 plans
- Proposed budget now appears insufficient in short term due to both administrative costs and responding to stakeholder needs;
- Significant issues seem to exist in fitting a long term budget (6 years) of the general amounts presented to potential needs; i.e. resolving HBC threats, responding to EIS policy direction, responding to implications of climate change, developing basin wide systems assessment capability
- Management review by AMWG needed to mitigate impacts of budget shortfalls

GENERAL COMMENTS: INTEGRATING MANAGEMENT AND SCIENCE

- Significant improvements over past planning. Extensive joint management/science programming
- Continued and more aggressive approach to addressing system level uncertainties should be considered as a basin wide effort/systems model assessing management/science options
- Consider to present pros/cons of more than one management/science option for project areas
- SAs support movement of SA program to BOR given assurances of direction by AMWG and continued review independence
- SAs support movement of Lake Powell program to BOR. Propose a new basin wide initiative be developed in this program to address system wide impacts from climate change

SPECIFIC AREA COMMENTS: PHYSICAL RESOURCES

- This program has developed to a high level of efficiency and effectiveness in responding to management and stakeholder needs
- Encourage greater integration regarding cultural resource science and management
- Propose continued adoption of advanced monitoring technology
- Strongly emphasize completion of effective camping beach modeling capability
- Management/science evaluation of continued need for levels of data resolution developed in program

SPECIFIC AREA COMMENTS: CULTURAL RESOURCES

- Express more explicit linkages developed among projects critical to cultural resource assessments; i.e. 2, 3, 11,13, etc.
- Improve sample size in project 4, clarify if it is the most critical project to Tribes for implementation in 2015.
- SAs support project 4, but several questions exist; how will it support Tribes and NPS, would pursuit of management actions with science monitoring be more appropriate, with such small samples here how will long term needs be satisfied, how will quantitative and qualitative data be integrated, how would climate change effect results.

PECIFIC RESOURCE AREA: CULTURAL RESOURCES

- Projects 11 and 12 offer significant opportunities for integration of cultural resource assessments. More specification of this opportunity in 11 is proposed
- Archeology expertise is proposed for project 12, with knowledge in TEK.
- Project 12 seems severely underfunded and possibly should be redesigned if funding is not available. Redesign should also consider project expansion to improve outcomes even though it will increase cost
- The rewrite of 12 should address among others; time needed for plant identification, providing better TEK literature reference to guide research, provide greater clarification on how qualitative and quantitative assessments are integrated, opportunities for use of citizen science and other data, incorporation of animal assessments as well as plants, addressing ways to move the project forward under funding constraints, i.e. pilot efforts.

SPECIFIC RESOURCE AREA COMMENTS: CULTURAL RESOURCES

- Project 13 elicitation of values regarding cultural resources will involve many tribal sensitivities that may differ among tribes.
- It is critical that this project is initiated with focus groups, many consisting of tribal representatives.
- Many resources have spiritual/religious values that may be incongruent with placement of dollar values.
- Some method should be developed to insure tribal involvement in all phases of the project

SPECIFIC RESOURCE AREA: BIOLOGICAL RESOURCES

- Vegetation monitoring offers great opportunity in AM integration of management actions/science monitoring with the NPS and is encouraged to obtain more rapid learning.
- Restoration to some desired future conditions is applauded and offer extensive opportunities for resource improvements and learning. However, it will be a long term process with potentially decades necessary for the system to find balance under the new management regimes and determine relationships at that state among physical resources, flora and fauna.
- This program might best be evaluated as part of long term monitoring given the above and its important connectivity to cultural, aquatic, biological and physical resources. Both scope and funding needs might deserve additional consideration.

SPECIFIC RESOURCE AREA: FISH ECOLOGY

- Understanding the issues of food base, habitat need and predation and their integration regarding HBC have greatest focus and hope for success in projects 7.1-7.5. Significant results have been gained in the last 3 years. This is a most critical project to the AMP, especially elements contributing to current modeling success
- Continuation of HBC monitoring in project 6 is critical to both learning and planning management actions
- A management review of opportunities to mitigate threats to water quality and invasive introductions to the LCR should be completed and activities implemented as feasible
- Increased management/science actions on BT control is proposed to evaluate both costs and effectiveness
- Increased management/science actions on HBC translocations are encouraged to continue the accelerated learning reintroduction success from this program

PECIFIC RESOURCE AREA: RBT FISH ECOLOGY

- Continued project 6 monitoring assessments of natives/non-natives as regards health, reproduction, migration, predation, etc. , are critical to the continued learning of their changes and how effective they can coexist in this system. Both the monitoring of RBT and extended research in projects 9&10 will be critical in understanding variance that may be produced by future warming trends in the river.
- A major shortfall of the project is the lack of concrete evidence from the abundance of past work to justify approaches in 2015-17. Examples exist regards otolith work and movement of species up from Lake Mead. Otolith work is not mentioned for BT tracking. Concern exists that monitoring is occurring but rigorous analysis may not be completed.

PECIFIC RESOURCE AREA: FOOD ASE

- Project 5 has created significant progressive accomplishments in a short time frame.
- New proposals 6.11-17 provide alternative options for laboratory studies, pilots and river experiments to initiate and test various hypothesis related to absence/low abundance of EPT.
- However, the science team does not provide strong supporting evidence for several hypothesis. It is understood that other southwest tail-waters support EPT, but the GCD environment may be sufficiently different to be hostile to these species. Contrasting key attributes of these different regimes might be helpful.
- An argument might be why would anything except small insects with rapid life cycles based on filter feeding or collecting use these habitats. A more thorough literature search and assessments could produce more focused hypothesis.

SPECIFIC RESOURCE AREA: SOCIOECONOMIC RESEARCH

- Three proposals are presented. In brief, all proposals were recommended by the SEAHG and supported by the TWG and AMWG.
- The recreation assessment program approach is an accepted standard and an excellent science team is presented. Review of the survey instrument would be helpful.
- Initial collaborative work with the SEAHG and TWG will be important in properly specifying approaches that are most likely to be useful to and be used by the TWG and AMWG.
- It is critical in work to assess values of tribal resources that tribal representatives are involved in all phases of the effort. Inclusion in initial focus groups, as proposed, is critical.