The Grand Canyon Monitoring and Research Center's Response to Science Advisor's Review of the Glen Canyon Dam Adaptive Management Program Biennial Budget and Work Plan—Fiscal Years 2013–14

This document provides responses from Grand Canyon Monitoring and Research Center (GCMRC) scientists to comments and suggestions received from the Science Advisors in their review of the biennial work plan developed for fiscal years 2013-14. The GCMRC staff appreciates the efforts of the Science Advisors to conduct a thorough review of the work plan. In particular, we thank Dr. L. D. Garrett for summarizing the reviews as well as his willingness to share all of the individual reviews. The Science Advisor's thoughtful suggestions and constructive criticism has helped to improve our proposals and will result in better science in support of the Glen Canyon Adaptive Management Program. Our responses to individual reviewer questions and comments are presented below and are organized by project.

Project A.
Sandbars and Sediment Storage Dynamics: Long-term Monitoring and Research at the Site, Reach, and Ecosystem Scales

Reviewer 1

1) Reviewer 1 asks whether the information that would be gained on the relative proportions of pre- and post-dam sediment in the geochemical signature project (A.5) is a critical information need of the AMP.
   Response: This research addresses the question of long-term trends in sediment storage, which is a critical information need of the AMP. However, as we are monitoring storage change by other methods, the reviewers question is valid. We believe the project has merit and have tried to explain in the project description the value of exploring this approach. However, this project is proposed as a research project, not a monitoring project, and we recognize that it may be of lower funding priority.
2) Has a cost effectiveness assessment been accomplished recently of the programs to determine if management and stakeholders require this level of monitoring and science findings or if there is another level of accomplishment that could fully meet management needs with cost savings?
   Response: A formal "cost effectiveness assessment" is not a part of this or any other project description. We have made every effort to describe the rational for the propose approach and the expected benefit in terms of information that will be supplied to the AMP. We have also sought to describe the problems and limitations associated with previously attempted approaches. The current approach follows the AMP guidance that the area of interest for monitoring and research includes 300 miles of river. We could envision a process in which we described information trade-offs that would be associated with a reduced monitoring scope, if that is desired.
3) An issue is identified from a Knowledge Assessment workshop related to vegetation and sediment interactions, and specifically loss of camping beach area to vegetation and changes in tamarisk vegetation due to the introduced beetle. This issue is pursued in two of seven presented sediment questions (pg 24). Project A.1.1., in part, is designed to provide necessary assessments. However, insufficient information is provided in A.1.1. to evaluate the expected accuracy of outputs or benefit from the remotely sensed data, except that it will differentiate vegetation from other land forms. For this approach to be effective it would have to differentiate tamarisk, both with and without leaf cover from

other vegetation types. Has this level of capability been demonstrated in pilot studies? Will there be an integrated analysis on sites with traditional ground based surveys that would permit assessment of remote sensing effectiveness. It is eluded that ground based surveys would be used for calibrating remotely sensed data models.

Response: The primary purpose of this project is to map exposed sandbar area. The approach described will allow differentiation between changes caused by changes in vegetation cover and changes caused by sandbar erosion or deposition. Even in the context of tamarisk defoliation, the tamarisk stands will remain and are unlikely to cause changes in exposed sandbar area in the scope of this study period. Other projects (see I.2 Riparian vegetation dynamics and trophic level linkages related to tamarisk defoliation) specifically address issues related to the spread of the tamarisk beetle.

In response to other aspects of the reviewers question: Our vegetation classification using the 2002 remote sensing data produced a mapping accuracy greater than 80% for tamarisk; the 2009 will do even better. In fact, when we complete vegetation mapping using the 2009 image data we will revisit the 2002 vegetation database to increase its accuracies, then proceed to map 2005 vegetation using the knowledge from both 2002 and 2009 vegetation databases. We have extensive ground truth for 2002 and 2009 vegetation patches that are distributed throughout the canyon; these large, diverse vegetation sites will be revisited during the 2013 image acquisition to update the ground truth sites for the 2013 image analysis.

What good is remote sensing? These data are the only means to map the surface for the entire corridor; no ground studies can accomplish such mapping over a 4-5 year period, maybe never given the fact we map up to the 250,000 cfs stage. For example, we can map the gross vegetation within a 5-km river reach, up to the 250,000 cfs stage in two hours; accurately classifying species within that gross vegetation would take 1-2 days.

Can we distinguish bare tamarisk? Within the 4 image bands that we collect, stem and bark of many species have very similar spectral characteristics. If we did not know that a leafless tree was once a leafy tamarisk, then we could only distinguish a bare tamarisk from other bare, woody species by the typical canopy texture (branch structure), but this is unproven. Texture for large stands will be different, but the texture of a young, small tmarisk will probably look very similar to bare shrubs. Basically, we have to rely on the changes in spectral signatures of tamarisk, based on our prior vegetation databases, where prior healthy tamarisk with leaves having a distinct chlorophyll signature will have NO chlorophyll signature for beetle-affected bare tamarisk. Bottom line -- It is easy to detect changes in leaf density and health, but very difficult (with CIR imagery) to detect differences between bare vegetation species without a priori information, which we do fortunately possess.

4) The first paragraph of section 4.1 would be an appropriate place to better justify Project A.4.
   Response: The text has been revised as suggested.

Reviewer 2

No comments for Project A.

Reviewer 3

1) Page 22, first full paragraph: From experience in small streams, hydrologists often speak of various issues (USP, pools vs. riffles, etc.) and how they change the dynamics of

erosion and deposition as a function of flow (low vs. high flow). Is that set of ideas not relevant to this system? And is there equivalent theory that might apply here?
Response: Good observation, and yes there are parallels and those ideas are relevant. Although that theory is not discussed in this proposal, better understanding of the similarities and differences between the very large pool-riffle system of Grand Canyon and smaller pool-riffle streams that have been studied more extensively is of research interest.

2) Page 22, next paragraph: The key point here is that there are models but they do not deal very effectively with sandbar formation. What kinds of things should be done to fix or overcome that problem?
Response: We believe the next steps include improvements in physical understanding of the linkages that control sand transport, which is project A.4 and research to better understand and learn to quantify site-to-site sandbar variability in project A.3. We have revised text to clarify those connections.
3) Page 23, First sentence of section 3.2: Key sentence here. Not sure the next section of text really plays out the plans of what will be done in the future to change that reality.
Response: The referenced text discusses AMP goals. The specific concern of the reviewer is not clear.
4) Page 23, Second sentence of section 3.2: How will managers know when they have achieved that goal? What benchmarks or index values can be established to recognize when progress is made or when the task is completed?
Response: The AMP has not provided specific goals or benchmarks. The purpose of this section is to make that clear.
5) Page 23, end of second paragraph of section 3.2: Key assumption seems to be that the tributaries must supply all the sediment needed to meet the needs of the system. Can the tribs be expected to yield what is needed from their current more or less undisturbed state? Or will disturbance have to be generated in those tribs in order to ramp up the sediment supply to keep the sand bars active and present? This seems very important as a mechanism to understand what is needed and if replacement is possible.
Response: The stakeholders of the AMP have discussions about strategies for sediment augmentation. Augmentation of the Paria River sediment supply was considered in a Bureau of Reclamation "Colorado River Ecosystem Sediment Augmentation Appraisal Engineering Report" completed by Tim Randle and others in 2007. Our research is focused on monitoring the outcomes of current management strategies.
6) Page 24, first sentence of second paragraph in section 4.1: The dichotomy between above and below water sandbars/sediment is intuitively appealing but I wonder if it leads to simplification that is not as good as it should be. The dichotomy may be important to humans but many other creatures and the hydrology itself may be more intimately tied to the details of below water level distribution of "sandbars" that alter flow at levels below the water surface. That is, the dichotomy may be more appropriately considered as a continuum, especially with the great diversity of areas below water surface. Has this been considered?
Response: Good observation and we certainly recognize that sand is stored at a continuum of elevations and that the above/below 8,000 cfs water surface is a gross simplification. We have used more continuous representations in some contexts (see Grams and others, 2010 Open-file report on the effects of the 2008 high flow on aquatic backwater habitats). One of the objectives of projects A.1 is to develop improved metrics for reporting our sandbar and campsite monitoring. A more continuous representation of the sandbar monitoring data may be result from that effort.

7) Page 25, paragraph below the table: In the case of eddy modeling, how far and up and downstream of the eddy is considered in efforts to model the eddy itself? Hard vs. soft banks, curves in channels, depth of areas, angles of flow, and so on some distance from the eddy defined narrowly seem like they would be important to understand something of the diversity of eddy behaviors. Is this being considered? Could it help to focus on important eddy contexts for modeling beyond local eddy shape or size? Perhaps this is already being done but it is not clear.
   Response: All of the listed aspects of channel geometry are incorporated in efforts to model flow and sediment transport in eddies, with some limitations resulting from data availability.
8) Page 29, second from bottom paragraph: Good but very brief summary of the study approaches. One might conclude that none are great, all are equally bad. Where does one go from here? How is the future work helped by what has come before? Can something more be said about the lessons of this for the future?
   Response: The intention in the proposal is to outline the path forward. This consists of infrequent repeat mapping of the long (>30 mi) monitoring reaches between the sediment transport gages. Revised text to improve the clarity and the logic of the approach.
9) Page 31, next to last paragraph: Only two dimensions of biology (aquatic primary productivity and the very vague "aquatic habitat") are included in this conception. What about taxonomic or other diversity? Are we to conclude that higher productivity is better? What components of habitat are considered? Why those and not others? Aquatic habitat is so vague. Habitat for whom? What dimensions of habitat (physical, chemical, biological)? And so on? Too often habitat is used as a catch-all phrase without careful thought of what is really meant. It too often means everything and thus can mean almost anything from nothing to everything, from well conceived and defined to not at all defined. What is the situation here?
   Response: The intention here is to state the applications for the development of methods to characterize bed texture and a complete map of bed texture. We are not proposing to conduct research on the aquatic food web or habitat requirements for aquatic species. Knowledge of bed texture is a specific requirement for modeling primary productivity. Bed texture and changes in bed texture are of specific interest to biologists studying native fish. We have revised text in an attempt to clarify the intention of this project.
10) Page 32, first paragraph: last couple sentences in the paragraph: And what hypotheses about the array of factors that may be responsible have been developed? How has this thinking helped to define what is included as of likely importance as a measurable variable? It seems a long time has passed before the level of thought suggested in the last sentence was initiated? Is it being attempted now in a rigorous and systematic fashion? Or is it a fishing expedition? Perhaps it has been systematic but one can't tell from the text provided here.
    Response: Hypotheses are discussed in following paragraph. The purpose of this project is to investigate eddy sandbar variability in a rigorous and systematic fashion, which has not yet been done.
11) Page 32, second paragraph, last sentence: The phrasing here "what aspect of channel geometry" leads the reader to assume only one factor is being sought. Why not "aspects?" Seems very unlikely that there is a single "aspect." Isn't it more likely the result of multiple factors acting in complex ways? The task is to understand the set of factors and how and where their relative influences vary with other things such as the comments above re eddy dynamics.

Response: Correct. There may indeed be more than one important aspect of channel geometry. Revised text to clarify.

12) Page 32, next to last paragraph: Some of the obvious examples of relevant factors are mentioned here. But it is still not systematic and the effort to show how those things will be used is not very comprehensive. Other things that come to mind include hardness of banks and bottoms, angles of channel flow along the thalweg, and so many more. Have these and others been considered in a systematic and comprehensive way as opposed to a laundry list of possible important factors. How will they be systematically investigated? How will the selection of sites and design of sampling be formulated to increase the likelihood of success? Which of the many may not be attacked effectively in a study of the systems here, given the configuration of eddy situations, time and money available, and so on? In short, what has advanced planning done to improve the probability of project success?
    Response: Revised text to improve project description.
13) Page 34, bold statements near bottom: Very important question. Again, how will this be determined? What kind of experimental or sampling design will be employed to ensure solid results?
    Response: We have revised the text in an attempt to address these concerns.

Reviewer 4

No comments for Project A.

Reviewer 5

No comments for Project A.

Reviewer 6

No comments for Project A

Reviewer 7

1) The report generally is quite effective at providing background information based on previous results and the importance of continuing work. It would be useful to very briefly reiterate at the start of Project A the idea that fine sediment can be stored along the submerged portion of the channel and then brought into suspension and deposited along the channel margins during high flows, although we have limited knowledge of the amount of sediment stored along the bed and the conditions under which that sediment is brought into suspension.
    Response: Revised text in section 3 to address this comment.
2) The report effectively identifies and differentiates core monitoring and research information needs addressed by the projects.
    Response: No action required.
3) I like the multi-level approach of a subset of sandbars with annual surveys and repeat photos, plus aerial overviews of the system every 4 years, and channel-wide surveys of river segments on a rotating basis of 3-10 years.
    Resoinse: No action required.
4) Are the photos and annual surveys conducted at the same sites? Why or why not? Why have 50 ground survey sites and only 30 repeat photo sites (presumably cameras are cheaper than ground surveys, so why not have more cameras)? It would be useful to justify the location of the 50 sites-do they represent simply a continuation of existing data (to create longer datasets), or has the distribution of sites been re-evaluated in light of increasing understanding of sediment dynamics along the river?

Response: The same 50 sites are monitored each year. Text was revised to clarify and address comment.

5) In Project A.1.1. Are these the same sites that have always been monitored? What about re-evaluating whether these are the most appropriate or feasible subset within the canyon? If the length of continuous record at these 50 sites is a prime criterion for their selection, note this explicitly.
   Response: The same 50 sites are monitored each year. Text was revised to clarify and address comment.
6) For Project A.1.3. the description is not as clear as those for other projects. It is not clear why the existing campable area metric is not sufficient, for example, and thus why a new project is needed. Why can't this be part of A.1.1. or even A.1.4.? Perhaps there is sufficient justification, but if so it needs to be better explained in the report.
   Response: The project description has been revised to address these comments.
7) In Project A.2.1. it would be useful to explain the rationale for greater detail to RM 87 and lesser detail downstream. The proposal for annual data collection regardless of HFEs seems more appropriate.
   Response: The project description has been revised to address these comments.
8) Project A.3. describes a statistical analysis of observed eddy and sand bar characteristics to predict sand bar response to changing discharge. It is questionable that this will work because presumably inflowing sediment concentration and flow duration are also very important in controlling sand bar dynamics but is not clear that these will be included in the analyses (the text mentions using metrics derived from computational hydrodynamic models, but this is very vague). Even though existing attempts to numerically simulate dynamics have not been successful (cited Logan et al., 2010 report), this empirical approach should be combined with a modeling approach Project scientists should explore collaborations with other modelers(for example, those involved in computational fluid dynamics) beyond Nelson's group.
   Response: We believe that an empirical analysis as proposed is worth attempting. With 20 years of observations, we have the ability to examine sandbar responses for a variety of streamflow and sediment supply conditions. Because we have identified significant correlation between a flow variable and sandbar size, we have some confidence that we will be able to discern some differences among sites. We do agree that it is important to continue with modeling efforts and have expanded the project description to provide some more detail on the modeling approach that we are currently planning to follow. At this point, we are not committed to a specific model or modeling group and may pursue additional collaborations.
9) Project A.4. does not include a very convincing description, partly because the site and methods are left open-ended. It would be more effective to briefly summarize observed relations (e.g., coarser sediment=less suspension?) and propose a conceptual model that explains why area immediately (2-4 km) upstream from a gage site influences suspended sediment characteristics. The stated hypotheses are a little vague in that they postulate relations but not the direction of those relations.
   Response: The project description has been revised to address these comments.
10) Project A.5. is quite important to understanding sediment dynamics with the study area, but the project description in the report is as vague as to be largely useless. What exactly will be done? How many sample sites? Hypotheses? The brief description provided is completely unclear and needs much more detail.
   RESPONSE: We have revised the text to provide a better explanation of the proposed research.

Project B.
Streamflow, Water Quality, and Sediment Transport in the Colorado River Ecosystem

No comments for Project B.

Project C.
Water-Quality Monitoring of Lake Powell and Glen Canyon Dam Releases

Reviewer 1

1) Reviewer 1 states that the Lake Powell Water Quality program has not produced the in-depth interpretive assessments that are common to other GCMRC sediment and biology projects. The reviewer is concerned that the proposed increase in funding in 2013 may be insufficient to meet the goal of a completed synthesis in November 2013. The reviewer recommended that GCMRC review its funding allocations for FY 2012 to ensure the interpretive synthesis can be accomplished.
   Response: Dr. Dale Robertson has been identified as an integral component in developing a comprehensive synthesis of the published physicochemical data and the soon-to-be-published biological data. Development of this draft of the synthesis is indeed planned for completion by November 2013 for presentation at the 2013 North American Lakes Management Symposium. Formal publication would occur in the following year. Program funding to meet these goals remains a limiting factor. Funding for the program is provided directly by Reclamation, apart from AMP funding, to fulfill Reclamation's monitoring requirements and is shared with other Upper Colorado River Basin water-quality funding. No other sources of funding have been identified. The proposed funding increases for FY2013 reflect time for Dr. Robertson's participation in the data synthesis as well as funding for a part-time GS-5 technician at GCMRC. The 2013 Interagency Agreement for Lake Powell monitoring is currently being developed between Reclamation and GCMRC and proposed funding levels must be approved by Reclamation.

Reviewer 2

No comments for Project C.

Reviewer 3

1) Page 67, first paragraph: Reviewer 3 states, "Many things proposed here but not enough detail about exactly what will be done, where, when, and why to really provide any useful judgment about the merit of the decisions." The reviewer adds that not much detail is found in the subsequent discussion to really understand the components of study design and analysis.
2) Page 67, first paragraph of background: Reviewer 3 asks, "What are the sites, how were they selected, what is the design context, and so on for these multiple levels of sampling.

If this isn't explored and explained in the next sections, it should be." The reviewer also expresses concern that much of the money expended in these efforts bears little fruit that is useful over the long term and that there is little integration with other studies.
Response: The Lake Powell monitoring program has been in existence since 1965, with the initial identification seven monitoring stations on the reservoir. Since that time, changes in the number of reservoir stations sampled, the frequency of monitoring, and methodology have taken place to reflect improvements in technology and evolving focuses of interest. The current program reflects a suite of sampling sites, monitoring frequencies, and methodologies to meet the scientific objectives of the program, at a reasonable cost on a sustainable basis. Specific details of site selection, frequency, and methodology are listed in USGS Data Series Report DS-471 (Vernieu, 2009). Text was added to the project proposal for clarification. The changes and their rationales detailed in this report provide important background information, but were thought to be beyond the scope of the project proposal. Design of an evolving long-term monitoring program must also take into account comparability to the record of previously-collected data. As the interpretive synthesis develops in the next year, it is expected that further refinement of the monitoring program will take place. As to the reviewers concern that much of the money expended may bear little useful information over the long term, the monitoring program at the very least, has provided a continuous depiction of reservoir stratification and inflow patterns and their effect on release water quality. This information is invaluable in describing the history of the reservoir and provides the basis on which to evaluate hydrodynamic and biological processes in the reservoir and evaluate results of simulation modeling.

Reviewer 4

No comments for Project C.

Reviewer 5

No comments for Project C.

Reviewer 6

No comments for Project C.

Reviewer 7

1) Under "Project C. Water-quality monitoring", Reviewer 7 states that it would be useful to specifically identify core monitoring and research information needs addressed by the project, as was done for project B.
   Response: While there are many science questions addressing the effects of water quality on various resources (sediment, foodbase, fisheries, recreation), no strategic science questions (SSQs) were developed that directly addressed tracking and predicting changes in the water quality in Lake Powell or Glen Canyon Dam releases. SSQs most closely related to the effects of water quality on key resources include SSQ 3-5, SSQ 5-1 and SSQ 5-3. While several of the existing core monitoring and research information needs relate indirectly to the quality of water in Lake Powell and Glen Canyon Dam releases, the objectives on the long-term program listed in "3.1. Scientific Background" provide the basis of the monitoring program design.

Response to Budget Comments in TWG meeting minutes 6/20-12/2012:

1) Project C, P. 96: Clarifying question: please clarify the statement "Equalization resulted in the evacuation of cold water from deep portions of the reservoir. These unusual conditions resulted in the warmest release temperatures since 2005, reaching 15.2°C on November 12, 2011, in spite of higher reservoir elevations." NOTE: This issue was not

addressed at the BAHG. Jack: this was odd but happened as described. Jack will consider adding more information.

Response: (Not sure where reference to P. 96 comes from. This statement was made at the top of p. 72 of the May 30, 2012 revision of the BWP) Wording in the project proposal was modified to clarify this phenomenon as follows: These increased releases began in January and continued through 2011. The increased releases from the penstock withdrawal zone, situated below the thermocline of the reservoir, resulted in a large volume of cold water (8-9^{0}C) being released during the months of February through May 2011. This depleted much of the cold water from the deeper portions of the reservoir near the penstock withdrawal zone, which was replaced with warmer water from surface layers and reservoir inflows, essentially lowering the thermocline much the same way that low reservoir levels in 2005 brought warmer water nearer to the withdrawal zone. Release temperatures began to increase sharply in June 2011 and the warmest release temperatures since 2005 occurred, reaching 15.2^{0}C on November 12, 2011, in spite of higher reservoir elevations.

Project D.
Mainstem Humpback Chub Aggregation Studies and Metapopulation Dynamics

Reviewer 1

1) Native fish research in Project D.2.1. And D.2.2. Specify sampling of native fish for otoliths (natal origin) and egg maturation using various procedures. Due to fish ESA status the sample sizes are very small. No estimates of expected variance are given on the proposed procedures. Can the effectiveness of these procedures be determined from these proposed sample sizes in 2013? The issue is raised because of the relatively high costs of sampling. If expected high variances are associated with the procedures would requests for additional take of the species to increase sample size be more appropriate.
   Response: Samples will be collected during Project D.1 (Aggregation Sampling), so additional costs of sampling are relatively low. We are hesitant to sacrifice more than 30-40 young-of-the-year humpback chub without further technique and methods testing.
2) The overview of Project F.1. does specify its relationship to Project D for both the sampling approaches and inference related to modeling. As stated previously it seems implicit that an ecosystem research design must be followed over 5-10 years to address the native/non-native interactions regarding habitat competition, food base needs, predation, etc. Yet, it is not provided. It should be provided in general terms in an introductory chapter for this plan and in more specific terms in the fish biology section.
   Response: We will address general ecosystem research design in an introductory chapter. Project F is intended to maintain continuity with long-term monitoring efforts.

Reviewer 2

No comments for Project D.

Reviewer 3

1) Page 80, second paragraph on "synthesis of existing aggregation data": When will this analysis be completed? How important is that to the careful design and planning of work expected to continue or be revised here?
Response: Much of the analysis and synthesis has been done, but has not been through the USGS fundamental science process. A report entitled "Colorado River Fish Monitoring in Grand Canyon, Arizona: 2002-2011 Humpback Chub Aggregations" is being prepared and should be ready for review by fall 2012. The report will present and summarize fish sampling results at humpback chub aggregations from 2002 through 2011, describing results of trammel net and hoop net sampling. The report also summarizes data collected at additional sites not associated with humpback chub aggregations. Population variables evaluated for the study include catch per unit effort, humpback chub abundance, species composition of the catch, size structure, and PIT-tag recapture information. The report also provides information on humpback chub translocated to Shinumo Creek and Havasu Creek that have subsequently moved to the mainstem Colorado River.

Reviewer 4

1) Aggregation and Metapopulation**:** We're still in the search for ways to expand understanding of how HBC define important habitat. This may help. But, the reality is that the LCR continues to be the focus of juvenile recruitment and adult abundance. Focus there!
Response: The majority of humpback chub monitoring will continue to be conducted in the Little Colorado River, with more than 2,100 planned annual net nights of effort compared with approximately 500 net nights of effort in the mainstem on each aggregation trip. There is no intention of deemphasizing any research efforts on assessing juvenile recruitment and adult abundance for the LCR population, but rather to better understand the relationship of these aggregations to the LCR population.
2) Hopes for undiscovered alternatives seem dim! There's high uncertainty elsewhere. That's no surprise given that there are damn few fish elsewhere! And, trammel nets continue to be a problem. Temperature is an issue, but that can only change if dam releases include a direct effort through multi-million dollar dam intake modification toward fostering gonad development through higher temperatures and/or if thermocline waters are entrained by lower water levels in Powell. The latter can be anticipated (by water level) and, as seen in the recent past, use this prospect to test for changes in distribution. We can't sample tissue maturation, but the indirect outcome of changes in distribution might help.
Response: No response necessary.
3) Natal origins of HBC**:** The otolith component of this has good prospects. Those involved (Limberg) is a proven performer. The integrated outcome may be very informative about where HBC's spend their time. LCR should offer an importantly unique signature about duration and frequency of repeat visits. Again, everything we know about HBC's says that managing the LCR habitat is most essential.
Response: No response necessary.
4) The ultrasound and Ovaprim prospect are interesting, but it seems that assessments of hatching success, etc., are a secondary issue. Constraints to spawning and recruitment may be temperature sensitive, but the more parsimonious explanations lie in ecological interactions in the LCR and the adjacent CR habitats.

Response: Ecological interactions in the LCR and nearby CR habitats are most likely the keys to continued recruitment of humpback chub. At the same time, having all of our eggs in one basket poses threats to the species.

Reviewer 5

1) Table 3: Have these data really been mined fully? There is a lot here to work with.Comparing the 1993 abundance data (Table 1) to the mark-recap data (Table 3), there is great variation among sites in the ratio of mark-recaps to abundance, suggesting that some populations have been fully tagged while others are scratching the surface. Can these disparities be used to estimate population sizes.
   Response: We agree that data needs to be more completely mined to better describe aggregation population dynamics (see reviewer 3 comments on data synthesis). The abundance data in Table 1 are from mark-recapture estimates during 1991-1993. Table 3 incorporates data from 1991-2011, and does not indicate the number of fish captured that have **NOT** been marked, only the location of recaptured fishes to indicate direction of movement and exchange of individual fish between the aggregations. In some cases, such as at the 30-mile aggregation in 2002, there is a high ratio of recaptures to marks. This may be due to sampling the same "hot spots" from year to year and catching the same fish. In the case of the Shinumo Creek inflow aggregation, 66% of the fish captured were previously marked, but most of the marks were from fish that had been translocated into the Creek. We have not found a way to estimate population sizes based on the ratio of marks-recaptures.
2) The mark-recap data clearly show two centers of distribution, also agreeing with Table 1. It would seem then that identifying the dispersal pathways between LCR and HGG should be a priority--we know they are connected based on mark-recap, so what are the key stopover points, and what can be done to ensure that these sites remain available? Focusing non-aggregation sampling efforts on the LCR-HGG connection points would address this issue.
   Response: We intend to devote more sampling time to the area between the LCR and Middle Granite Gorge (we assume you mean MGG, not HGG). Project G (brown trout removal) has been proposed as a possible way to improve connectivity between the LCR and MGG. Identifying "key stopover points" would probably require telemetry studies, and is not planned for this work cycle. The quantities of chub dispersing from the LCR as well as their vital rates (growth and survival) may vary spatially and temporally; but it remains unlikely that there are different pathways of dispersal between aggregations sharing the same corridor of movement.
3) These aggregations are intriguing--what physical or biological characteristics predict their location? Have they been profiled carefully, especially the sites not associated with LCR? The mark-recap data strongly suggest that many "aggregations" (e.g. LCH) are merely pass-throughs, not stable population centers.
   Response: We agree that the Lava Chuar to Hance "aggregation" is an extension of the LCR aggregation. We are developing a multi-attribute model to describe habitat parameters at aggregations. Tributary or warm spring inflows appear to be important factors affecting aggregation presence (30-mile, Shinumo, Havasu). Based on captures of adult chub near the LCR, large eddies associated with debris flows may also be important habitat for adult chub (see Valdez and Ryel 1995).
4) Page 83 below Table 3: Need to explain how two nights of sampling allows BOTH closed population estimates AND capture probability estimation. The first is obvious; the

second is not if it is to be a parameter independent of the first. The rationale is intuitive, but the statistical approach and its rigor need to be outlined.

Response: We have revised the paragraph to clarify that the two nights of sampling is intended to increase mark rate. Capture probability will be estimated based on pooled data across all aggregations, and abundance will be estimated based on marks and recaptures across years.

5) Page 83: "…colonize new habitats…": This is key, and great that non-aggregation sites will be surveyed. What is the level of confidence that there are not and have never been fish present beyond the aggregations? Given the movement patterns documented by mark-recapture, can we gain any insight into dispersal pathways e.g. rate of exchange is highest between LCR and HGG sites, so there must be a corridor connecting them that is more easily traversed than other such corridors.

   Response: Our confidence that there are NOT fish present beyond the aggregations is low, hence the desire to sample more extensively around aggregations. Monitoring efforts in the last 15-years have continued to resample previously defined aggregations (Valdez and Ryel 1995), and only more recently has monitoring been extended outside the sampling areas of these known aggregations (Ackerman 2008); therefore, it is remains uncertain whether or not aggregations are actual discrete local populations or reflect a sampling bias in the current monitoring program. Without further expanding on the sampling effort the current sampling of the downstream aggregation will only reinforce this potential sampling bias.

6) Page 83 bottom: Odd that estimation of HC abundance at non-aggregation sites is not listed as a product. Even if the fishes are simply moving through, it is important to know where else they can be captured.

   Response: We know that the fish community changes longitudinally downstream of Glen Canyon Dam, and because of the historically clumped catches of adult humpback chub, we are hesitant to extrapolate abundance estimates from aggregations to the rest of the river.

7) Page 84 - This section is begging for a long-term record of water temperature, to evaluate how often the 16C threshold is reached at each of the aggregations. That must be available?

   Response: Modeled water temperature (Wright 2008) for the period 1992-2010 indicated that 16°C was exceeded during 10% of the months between May and October, 1994 – 2010 at RM 140, and 50% of the months at RM 210 (fig. 2).

8) p84 - Are there no observations of gravid female HCs during previous sampling? Have museum specimens been examined to assess phenotypic indicators of spawning condition?

   Results of Kaeding et al 1990 TAFS 119: 135-144 suggest that breeding in Upper Colorado occurs in mid-late June, so why not do some surveys during that time window (or the equivalent thermal time below the dam)? Kaeding et al found expressible gametes in both males and females, so this should be an easy approach to resolve the question. Also, use implantable temperature loggers (perhaps as an ultrasonic tracking tag) to ascertain the actual thermal regime experienced by these fish. They are surely good at finding the right thermal habitats, if they exist.

   Response: Gravid females are caught in the LCR during the spring and occasionally in the fall, but rarely in the mainstem Colorado River. One gravid female was collected at the 30-mile aggregation during June, and five were collected near the LCR inflow between May and November. Ripe males have been captured between March and

November, and at more locations than females in the mainstem (USGS unpublished data).

We are recommending a sampling trip during July, when we expect humpback chub may be gravid in the mainstem. In the upper Colorado River, Kaeding et al. (1990) reported humpback chub spawning in the Black Rocks population at 14-24 °C during June and July following peak spring runoff. There is really no equivalent thermal time downstream of Glen Canyon Dam since the warmest release temperatures now occur in the late fall and winter (Voichick and Wright, 2007). Unlike the upper basin populations of chub, it remains uncertain how the receding limb of the spring run-off (Kaeding and others, 1990) and temperatures interacted as spawning cues for chub mainstem populations , especially since the extant LCR population of chub remains predominately a tributary spawners.

Sonic implanted temperature loggers are an interesting idea, but logistically extremely difficult to deploy and track.

9) D2.1 - Excellent choice of method, but some pilot data should be shown to prove the stated ease of differentiation, and capacity to resolve multiple different tribs. Water d13C and d18O can vary widely though time, particularly in arid landscapes where groundwater vs. runoff mix varies seasonally. So robust inferences will require some seasonal work. Nice use of surrogate species to establish system baseline comparisons, and that will also allow evaluation of seasonality.

   Response: Water $\delta^{18}O$, dissolved organic carbon (DOC) $\delta^{13}C$, and dissolved inorganic carbon (DIC) $\delta^{13}C$ stable isotopic ratios were very different between the Little Colorado River and mainstem Colorado River (K. Limburg written communication).

10) Why not also use trace elements. Would be a worthwhile investment to assess whether additional resolving power could be achieved, as they can be more definitive than C and O isotopes in some settings. Be sure to keep otoliths from any incidental morality of adult HC.

   Response: Distinctive trace element signatures were detected for some tributary streams; however, trace elemental chemistries of the LCR and mainstem Colorado River were similar (K. Limburg, written communication). Limburg plans to use a combination of trace elements and C and O isotopes.

11) D2.2 - This doesn't seem like a compelling first step. Why not sample HC at the right times of year, use thermal monitoring, etc. instead of focusing on manipulations and lab comparisons that are of dubious relevance to field circumstances. The ultrasound approach is appealing, but better as a secondary rather than primary approach to addressing the data need.

   Response: We intend to increase sampling during the summer (July) when we expect chub may be gravid in the mainstem Colorado River, but logistics of thermal monitoring (e.g., temperature logging sonic tags) preclude its use at this time due to limited sampling frequency.

Reviewer 6

1) Project D [monitoring and abundance estimation in humpback chub aggregations in main-stem]: This project description was not well written and did not give precise details in some cases about the number of fish being handled and tagged.

   Response: Based on sampling during 2010 and 2011, we expect to handle approximately 300-400 humpback chub (table 1). Project description has been revised to provide more details.

2) Two issues need to be evaluated. The concept of a classic meta-population applies here because of the network organization and directionality of flow in this network of subpopulations (see papers by Bill Fagan in Ecology on Aravaipa Creek). It is more likely that a source-sink paradigm applies (Pulliam 1988). Specifically, the aggregation at the LCR and in the LCR are the populations responsible for production and export of larvae downstream with some occasional straying upstream of adults or sub-adults. The important questions to answer here are: 1) do YOY move downstream and how far and is this related to floods during snowmelt or monsoon in LCR (as in Project E)? 2) What is the survival of fish in downstream aggregations (is it so low that these populations are not self sustaining)? And c) How much do the downstream populations serve as a lifeboat for the LCR population when it is waning in abundance? I.e. is there more than negligible movement upstream that would offset bad years when downstream production is high (if production downstream occurs at all). The second large comment for this project is the statistical analysis. One of the goals is to provide a robust estimate of capture probabilities by sampling more frequently. There should be some proof of concept here. A quick and dirty simulation showing how additional sampling might lead to lower CI bounds and more rigors in obtaining the capture probability. Also, it would be nice if this unit's statisticians could develop a method for combining estimates from electro fishing and hoop and trammel nets into something that gives a robust total population count.
Response: We agree that in most years, the mainstem Colorado River downstream of the LCR represents a classic sink, with the source being the LCR. The apparent persistence of these aggregations suggests that they are maintained by immigration from the LCR source, as well as by occasional local production and by low adult mortality. Based on PIT tag recaptures, few of the young chub that are captured in downstream reaches return to the LCR. Young-of-the-year chub have been collected throughout the river downstream of the LCR and we presume that most of these fish originate in the LCR. However, during years with warm releases (2003-2005 and 2011), water can exceed 16 C for several months near the MGG aggregation (fig 1) (Wright and others, 2008)). Survival of adults in downstream aggregations is probably very high, based on adult survival estimates of 85 - 87% (S. Martell written communication). Project E is designed to help address questions of timing of humpback chub movement from the LCR by marking additional young-of-the-year humpback chub.

We have included a table showing number of fish marked and recaptured in each aggregation and worked with Dr. Carl Walters and David R. VanHaverbeke to simulate how increasing sampling and capture probabilities may improve our ability to estimate. It is possible that increasing sampling from one trip to only two trips will provide little improvement in confidence interval bounds of abundance estimates.

Electrofishing is generally ineffective at capturing adult humpback chub, although it can be used to capture young fish. Projects E, humpback chub early life history and Project F.3, juvenile chub monitoring, will help refine capture probability estimates for hoopnetting and electrofishing. We combine trammel and hoop net captures when estimating abundance and capture probability, but don't have a comparable electrofishing dataset from aggregation sampling.

Reviewer 7

No comments for Project D.

Table 1. DRAFT PROVISIONAL Number of fish captured by hoop net (H) and trammel net (T), Colorado River aggregation surveys, 2002-2011. Data from 2002-2006 are from (Ackerman, 2008).

	2002		2003		2004		2006		2010		2011		
Species	H	T	H	T	H	T	H	T	H	T	H	T	Total
Flannelmouth sucker	5	9	42	43	22	68	107	240	271	634	252	265	1,958
Speckled dace	5	-	27	-	34	-	185	-	786	-	183	-	1,220
Rainbow trout	32	84	45	133	16	122	4	28	124	323	108	131	1,150
Humpback chub	30	27	32	35	41	27	53	88	143	153	240	150	1,019
Bluehead sucker	-	46	1	11	-	8	5	73	12	120	13	125	414
Fathead minnow	4	-	-	-	-	-	62	-	34	-	39	-	139
Brown trout	2	11	3	41	2	24	-	4	2	7	2	1	99
Common carp	-	15	-	18	-	10	-	10	19	4	2	1	79
Red shiner	-	-	-	-	-	-	1	-	72	-	5	-	78
Channel catfish	-	4	-	7	-	6	-	12	-	5	-	1	35
Black bullhead	-	-	-	-	1	-	1	3	-	1	-	1	7
Striped bass	-	-	-	-	-	1	-	5	-	1	-	-	7
Plains killifish	2	-	-	-	-	-	-	-	-	-	-	-	2
Green sunfish	-	-	-	-	1	-	-	-	-	-	-	-	1
Total catch	80	196	150	288	117	266	418	463	1,463	1,248	844	675	6,208
Net sets	393	370	596	488	576	472	504	407	398	447	571	319	5,541
Hours of effort	6,782	706	11,797	947	11,441	931	9,581	801	8,072	899	11,174	622	63,752

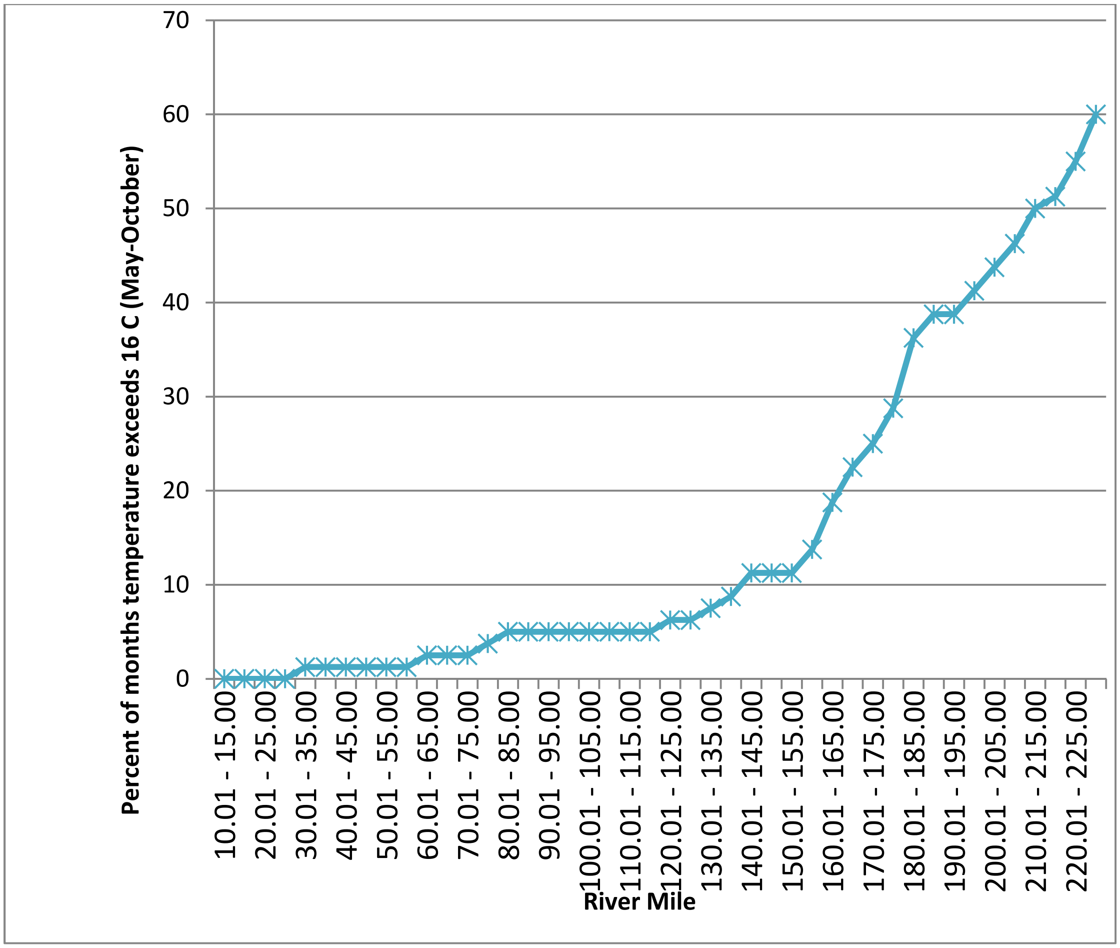

Figure 1. Percentage of months in which temperature was predicted to exceed 16C based on Wright (2008) model, May – October only, 1994 - 2010.

References:

Ackerman, M.W., 2008, 2006 native fish monitoring activities in the Colorado River, Grand Canyon: Flagstaff, Ariz., SWCA Environmental Consultants, report to Grand Canyon Monitoring and Research Center, cooperative agreement 04WRAG0011, __ p.

Kaeding, L.R., Burdick, B.D., Schrader, P.A., and McAda, C.W., 1990, Temporal and spatial relations between the spawning of humpback chub and roundtail chub in the upper Colorado River: Transactions of the American Fisheries Society, v. 119, no. 1, p. 135-144, accessed on August 24, 2011, at http://www.tandfonline.com/doi/abs/10.1577/1548-8659%281990%29119%3C0135%3ATASRBT%3E2.3.CO%3B2.

Voichick, N., and Wright, S.A., 2007, Water-temperature data for the Colorado River and tributaries between Glen Canyon Dam and Spencer Canyon, northern Arizona, 1988-2005: U.S. Geological Survey Data Survey Series 251, 24 p., accessed on July 19, 2011, at http://pubs.usgs.gov/ds/2007/251/.

Wright, S.A., Anderson, C.R., and Voichick, N., 2008, A simplified water temperature model for the Colorado River below Glen Canyon Dam: River Research and Applications, v. 25, no. 6, p. 675-686. (Available at http://dx.doi.org/10.1002/rra.1179.)

Project E.
Humpback Chub Early Life History in and Around the Little Colorado River

Reviewer 1
No comments for Project E.
Reviewer 2
No comments for Project E.
Reviewer 3
No comments for Project E.
Reviewer 4
No comments for Project E.

Reviewer 5

1) Regarding back-calculated estimates, needs to be clearly stated whether there is a signature in the adult pop age structure of the low recruitment during those years? If not, it suggests that YOY survival is not the key bottleneck alleged here, unless it can be show that the imprecision in back-calculation of survival is as high as to swamp interannual variation in recruitment (seems unlikely if recruitment failure is observed in some years).
   Response: There are a number of problems plaguing the back-calculated estimates, chief among them the high uncertainty in assigning age to fish of different size given the variable growth rates over time and between the LCR and the mainstem. As a result fish reaching 150 mm (when they were first tagged up until 2008) can be anywhere from 1.5 to 5 years old thus creating imprecision that can in fact swamp interannual variation in recruitment. There is universal agreement that recruitment to 150mm was low from the mid-1990s through early 2000s and played an important role in the population crash and that overall recruitment has been better since, however the back-calculated estimates do not allow us to discern finer year to year variation in recruitment.
2) p94, H1**:** Are we sure that adults are spawning in these low recruitment years? That seems a reasonable first hypothesis, given the known variation in thermal regimes across years, and temperature-dependence of reproduction.
   Response: Yes, we are sure. There is strong evidence to suggest that spawning occurs in the Little Colorado River every year. The Little Colorado River is an unregulated tributary with only minor water extraction, where thermal regimes have always been observed to allow for reproduction.
3) p94, H4: Flood years may also just dilute the population or otherwise reduce sampling efficiency, giving the appearance of low recruitment/few juveniles. Again, is there clear cohort evidence that these were really bust years?
   Response: Evidence comes from sampling that generally occurs after the major floods have passed and suggests that the size of the after floods population is highly variable.
4) E.1: Are hoop nets an efficient way to capture these small individuals? would seem the mesh size would not be appropriate if the nets are designed to catch adults and minimize drag in river flow.

Response: We have just completed a pilot study suggesting that hoop nets are effective, but that other methods (e.g., seining) may be more efficient at capturing young-of-year. As these efforts were done under high turbidity conditions, we plan to use multiple gear during the project in the event that the efficiency of other methods declines under less turbid conditions.

5) p96 : Mass emigration from LCR hypothesis: do the bulk catch rates support the inference that lots of juveniles are there in July, but few in fall? Or is it just the low-N mark-recap effort?

   Response: There has only been limited surveying in July and only in years with relatively low variability in discharge, however more intensive studies in the mainstem in July and August suggest an influx of juveniles during periods of high LCR discharge.

6) P96**:** Temperature and trout are presumable directly correlated, yes? Please clarify how the two would be disentangled, as implied by the phrasing here.

   Response: Over the range of temperatures found in the mainstem, rainbow trout are relatively insensitive so there is no direct linkage between temperature and trout. In the 2000s, there was an indirect linkage between temperature and trout, however this occurred because trout first increased and then crashed in response to high water years and high flow experiments (Korman et al. 2012) that serendipitously occurred a few years prior to lowering of the water levels in the dam that lead to warmer releases. It is possible that a similar scenario will play out in the coming years and we will not be able to disentangle these drivers. We hope that individuals responsible for the LTEMP EIS and members of the AMWG will manage the resources to allow us to disentangle these effects and we are working to provide the best monitoring design and modeling tools to enable us to learn, however at the end of the day it is within their power to decide how much they are willing to alter dam operations to resolve this uncertainty.

7) E.2**:** This element is all over the place. Really needs to be restructured to clarify what issues are being addressed and why the proposed methods are worthwhile. The toxicological component was the only one that was at all convincing and only because it constitutes a survey of a fundamental pattern rather than an attempt to draw big-picture inferences from shaky data. All other aspects of this section are valuable for basic understanding, but the case was not compelling that they would allow any real questions to be resolved. In particular, the diversity of hypotheses for controls on juvenile performance is only indirectly assessed (at best) by this diverse suite of methods. Bottom line: it is hard to have confidence that these data can be stitched to tell the story that the applicants are interested in.

   Response: This section has been reorganized to better describe what issues are being addressed and why the proposed methods are worthwhile.

8) P97**:** Components of energy flow estimation: this set of shaky estimates will yield an even shakier overall estimate of energy flow. The authors must at least recognize the spatiotemporal variation in most of these factors, and the difficulty of constraining all of them well enough to have a meaningful estimate of production potential in the end.

   Response: We fully acknowledge spatiotemporal variation in many of these factors. But rather than argue that this variation makes the quantitative food web approach infeasible or intractable, as the reviewer does, we argue this variation makes a quantitative food web approach all the more critical and important. The authors of this project used these same approaches to describe quantitative food webs for the mainstem Colorado River (see Cross et al 2011). Such an integrated, ecosystem approach allowed us to easily reconcile counterintuitive food web responses to the 2008 artificial flood. A significant increase in rainbow trout populations occurred after the flood, in spite of a large decrease in total invertebrate production (~60% reduction, from 30 g AFDM m^{-2} yr^{-1} to just 13 g AFDM m^{-2} yr^{-1}) that was largely driven by a 70% reduction in the production of a dominant taxon (G. lacustris; from ~8 g AFDM m^{-2} yr^{-1} to 2.5 g AFDM m^{-2} yr^{-1}), which earlier investigators had concluded was a critical prey item for rainbow trout. This same counterintuitive ecosystem response would have confounded interpretation of traditional 'foodbase monitoring' indicators such as benthic invertebrate biomass.

9) P97: Extrapolating fish consumption rates: but aren't good population size estimates for these fish species very hard to obtain (thus the motivation for much of the GCDAMP work)?
Response: It is because of the tremendous effort directed towards estimating fish population size in the LCR and the mainstem near the LCR confluence that we are able to propose describing quantitative food webs and estimating fish consumption. GCMRC scientists and cooperators and constantly striving to improve these estimates, particularly for humpback chub, because of the need to identify cause and effect relations between possible limiting factors (e.g., water temperature, non-native fish, food limitation, etc.). Project descriptions describing limitations of current monitoring programs might give the impression that these monitoring are flawed and/or population estimates cannot be reasonably constrained; however, the fish monitoring programs in Grand Canyon are some of the most robust and rigorous large river fish monitoring efforts in the nation and the estimates they yield are certainly good enough to develop reasonable estimates of consumption.

10) P97**:** Contaminants: Why start with fishes instead of their food resources? Will contaminants be used as a tracer of trophic pathways, or as a stressor in their own right?
Response: We do not propose sampling just fishes; the proposal calls for sampling contaminants in all food web compartments so as to trace contaminant fluxes through the food web. The primary focus of the contaminant research is identifying whether they are stressors in their own right. If possible, contaminants will also be used as a tracer of trophic pathways.

11) P98**:** Isotopes: N isotopes will not be informative of prey N content for carnivores. How will the C and especially S isotopes be used?
Response: We have dropped the stoichiometry element of resource quality (i.e., N content) and now focus solely on contaminants as lever on resource quality. We will attempt to use C and S as tracers of trophic pathways, contingent on pilot research (to be conducted) that demonstrates there is sufficient separation among end-members for this to be a useful tool. We also propose gut content analysis of fishes in case there is not sufficient separation in end-members for stable isotope analysis to be a useful tool.

12) P98**:** Comparisons to other species: How about just doing some simple modeling of how food resource changes vs. competition changes could affect juvenile performance in HC? That might be a better starting point.
Response: A recent foodbase Protocol Evaluation Panel (PEP) identified describing food webs in the LCR and other tributaries as a critical research need.

13) P100 top**:** The food quality case is weakly developed, at best.

Response: We retain the distinction between food quantity and quality in the proposal but have dropped the stoichiometry aspect of our food quality investigation and now focus only on contaminants as a lever on quality.

14) P100 toxic metals: It is not clear that the applicants realize how much work it will be to conduct these analyses well, and draw inferences about bioaccumulation pathways. Not all of these elements are likely to bioaccumulate at the same rate, but that is not mentioned. Is there any evidence of levels of these metals that are toxic to fishes themselves? Has Hg and other metals never been analyzed by state authorities regarding the major trout fishery?
Response: The applicants have extensive experience in contaminant research and biogeochemistry (https://profile.usgs.gov/rwanty, http://www.fort.usgs.gov/staff/staffprofile.asp?StaffID=1000, http://www.fort.usgs.gov/staff/staffprofile.asp?StaffID=1073) and are aware of the effort required to conduct these analyses. The proposal describes a report (Andrews and others 1995) that documented potentially toxic concentrations of metals in fish in the LCR upstream of the perennial reach that is the focus of the proposed work. The proposal describes how the LCR appears to be a major potential source for contaminants in the mainstem river. The trout fishery is far upstream of the LCR, so any estimates of Hg or other metals from that site are not germane.
15) E.3**:** This project component is poorly specified; more detail is needed on the age and spatial states that will be modeled, and the robustness of the approach for estimating different life history parameters. The specification of the 'deterministic models" is even less well developed. Overall, this approach seems useful, but is poorly presented.
Response: Initially we have focused on developing a simple model with three size states (40-100, 100-200, 200+) and three spatial locations (LCR, NSE, mainstem outside of NSE), however we plan to try more size and spatial locations if the data allow it. The basic statistical approach (multistate modeling) is statistically very robust (see papers by Kendall, Nichols, etc.) however we expect some parameters will have high uncertainty because of gaps in the sampling design. This will in turn inform future monitoring efforts. The deterministic model is not as standard and will require more innovation. Conceptually we imagine a relatively simple spatial and size structured model with survival based on the density of individuals within a size and spatial class as well as interactions between size classes within spatial units and movement between spatial units based on either hydrology or density dependent dispersal.

Reviewer 6

No comments for Project E.

Reviewer 7

No comments for Project E.

Project F.
Monitoring of Native and Nonnative Fishes in the Mainstem Colorado River and the lower Little Colorado River

Reviewer 1

1) The overview of Project F.1. Does specify its relationship to Project D for both the sampling approaches and inference related to modeling. As stated previously it seems implicit that an ecosystem research design must be followed over 5-10 years to address the native/non-native interactions regarding habitat competition, food base needs, predation, etc. Yet, it is not provided. It should be provided in general terms in an introductory chapter for this plan and in more specific terms in the fish biology section.
Response: We will address general ecosystem research design in an introductory chapter. Project F is intended to maintain continuity with long-term monitoring efforts.

Reviewer 2

1) When one reads the individual native and non-native fish projects it is implicit that they represent an integrated approach. However, what is not provided is a science design of the linkages of individual science components and linkages to the stakeholders' goals, objectives, DFCs, etc. These goals, objectives, DFCs, etc. are the basis for science programs for the AMP.
Response: We have added section 3.2 and 3.3 to address linkages to stakeholder planning documents.
2) There is a lot of overlap in sampling efforts of the fisheries program and to a lesser degree in the conceptual basis for this effort. In my opinion some of the sampling programs could be combined in creative ways to give the same answers with less sampling ($) and less handling of the fish.
Response: Because of sampling overlap in Marble Canyon between trout monitoring and specific research studies (Natal Origin Study) sampling has been reduced. Concurrent with the Natal Origin Study, invertebrate drift and trout diet data are being collected to reduce overall sampling cost and fish handling stress. In addition to the cost savings these combined efforts allow for foodbase studies to be integrated with fish sampling.

Reviewer 3

1) Page 116, end of second paragraph**:** This paragraph deals with some issues at the heart of very important questions for scientists, especially biologists: how to decide how much sampling is needed. Is this really practical and even necessary to sample all habitat types? I agree that the decision to only sample the most productive ones can mislead us. Wouldn't it be just as useful to have focal sampling on a smaller range of microenvironments but with greater precision of parameter estimates? That leaves the question of how to select the focal habitat/microenvironments. The project leaders here have decided that it is not necessary to sample in all seasons, a wise decision that too many biologists are reluctant to make. Here the reasons are practical and financial. But the same kind of process could be used to decide that not all habitats need to be sampled, or can be sampled at an intensity that gives enough information to have confidence in the

results and their interpretation for ALL habitats. Can one, for example, get enough information from all habitats to make strong conclusions from each habitat or would it be better to sample a selected set of habitats with greater intensity to narrow the bounds on estimates of characteristics of the biology in those microenvironments. These are important questions that must be explored eventually to get the strongest results with the greatest fiscal efficiency. Another issue is the collection of so much data that the lab analysis of field samples can't be done in a timely manner to guide work for the next year. See comment below re page 160 for an example of this kind of effort to determine how many samples are needed to have robust results. Also see a report on sampling invertebrates from streams by Leska Fore on the same issue available at her website: http://www.seanet.com/~leska/index.htmlSee, for example, the following papers available on that site:

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

Response: We have added text describing the approximate number of samples that will be collected in each annual benthic survey (100) and the time required to lab process these samples (500hrs). Such a level of effort is sustainable and lab processing will be completed prior to the following years efforts so subsequent sampling can be informed by previous years efforts.

One purpose of the annual benthic survey is detecting new invertebrate taxa that might become established in the Colorado River in the future. For this reason, we have elected to sample all habitat types (cliff, talus, depositional, cobble) rather than just focusing on the most productive habitat type (cobble). Sampling just cobble could miss new invading taxa if they, for example, prefer depositional environments.

Reviewer 4

1) Monitoring native and non-native fish populations. In summary, this is an expensive but proven and essential part of the monitoring efforts required for effective AMP. Therefore, strong support is essential.
   Response: We agree.

2) Monitoring native and non-native fish populations**:** This set of efforts is essential. Monitoring of juvenile HBC is essential to understanding population dynamics. The Chute Falls work is an essential program. The new PIT tag detection system provides important data that enhances ongoing field programs. Re-evaluation of the ASMR model is worthwhile. Using HFE's to control rainbow recruitment works and requires monitoring. Appearance and abundance of non-natives is discovered from monitoring. Management of the recreational fishery requires monitoring and is augmented by the natal origins project. Food base, drift and fish food habits are essential to painting the food web picture(s).
Response: No response necessary

Reviewer 5

1) Project F: Native and non-native fishes**:** Overall: All components are reasonable, basic monitoring that should be continued. A few more details would have been nice. It would be very helpful to show some of the historical data. There is a major risk with such monitoring efforts that no one ever circles back to evaluate the patterns carefully after collecting all that data. In this case, it would help to justify continued efforts if we were shown time series of past results under each section, rather than just stating the year range of monitoring. Costs seem pretty high.
Response: We have included citations to historical data, but have not included the data in this work plan.
2) P106 summary**:** Summary includes no statement of what will actually be done!
Response: We have included a brief listing of the work that will be done within the 12 project elements.
3) F.6**:** Natal origins are difficult to assess with mark-recap since the fish have been alive for quite a while before they can be marked by most standard methods. Perhaps apply the otolith microchemistry method from D2.1?
Response: Large numbers of young-of-the-year rainbow trout are being effectively marked with PIT tags. Previous work with otolith microchemistry suggests that it is not a feasible tool for differentiating natal origins of fish from different reaches of the mainstem Colorado River.
4) F.7.2**:** This is a really great idea. Have the guides already signed on?
Response: Yes, we have had good initial participation from guides during the 2012 season.
5) F.7.3: Primary production will be analyzed by what method? Presumably sondes measuring DO fluctuations, but it should be stated clearly. There is no statement of methods to integrate the 5 spatial estimates, either. That can be very complicated to do properly for a large river.
Response: We have added a citation for the method we will use to estimate primary production (Hall and others 2010). This method provides an estimate of primary production for an approximately 10-20km reach upstream of the sonde location. We will not attempt to integrate the 5 spatial estimates into a canyon-wide estimate of primary production. Instead, we will focus on identifying causes (i.e., dam operations, turbidity and light, etc.) of temporal variation within sites and variation among sites. Yard (2003) will serve as a framework for identifying causes of the spatial and temporal variation that are identified through monitoring.

Reviewer 6

1) Project F [monitoring]: This section is a little mundane and lacking in detail. Nevertheless this work is core and essential. Focus the monitoring in a way that moves towards capability for estimating abundance and combining gear types in a rigorous way such that we know how relative abundance (not CPUE) of all members and life history stages of the community are changing over time.
   Response: Where relative abundance (CPUE) is considered an inappropriate metric, GCMRC has moved toward estimating actual abundance of trout and native fish. A research study (F.6) (PI Josh Korman Ecometric Research, Inc., B.C.) is determining the origin and movement of rainbow trout in the Marble Canyon/LCR confluence area via a large-scale mark and recovery effort, and continuation of juvenile chub monitoring to provide additional estimates of juvenile native fish abundance and survival in the mainstem near the LCR. These data are needed to evaluate the linkage between trout populations in the Lees Ferry reach and Marble Canyon, the efficacy of a possible Paria to Badger Rapid nonnative fish removal effort, and the response of juvenile native fish to changes in trout density near the LCR area resulting from removal and experimental flow treatments.
2) H.2.1: This primary production work will expand understanding the ecosystem dynamics, but it is hard to see how it is central to dam or fish management, which are the overall stated objectives
   Response: A prevailing hypothesis thought to explain the current distributional patterns and abundance of aquatic biota in the Colorado River (Blinn and Cole 1991; Carothers and Brown 1991) are that the different trophic levels (invertebrates and fishes) are now strongly linked and dependent on basal production originating from the autotrophic community (Stevens et al. 1997; USGS 2005). Suspended sediment loads supplied from tributaries (Topping et al. 2005) as well as channel deposits mobilized by flow operations (Tipping et al. 1993) diminish the frequency of underwater light available for primary production (Shaver et al 1997; Yard 2003). Resulting turbidity has created a light-gradient and a potential for food resource limitations (Stevens et al. 1997). Although more recently, foodbase studies have shown that certain invertebrates (i.e., Simulium arcticum) intermittently use high proportions of allochthonous sources from tributaries (Wellard 2011); nevertheless, it still remains uncertain what role primary production (quality vs. quantity) as well as its provenance (upstream vs. system-wide) has on supporting this aquatic ecosystem. Alternatively, for fishes like trout that feed primarily by sight, variations in daily turbidity may affect visual prey detection (e.g., Barrett et al. 1992; Stuart-Smith et al. 2004), such that turbidity may have differential effects on individual species rather than the whole fish community. Understanding how turbidity influences primary production and the foraging ecology of trout (rainbow trout and brown trout) may provide insight in how to manage non-native fishes without adversely affecting the entire fish community.
3) Would this model or measurements actually affect management practices? If so, sketching out that scenario in more detail would be helpful.
   Response: Monitoring primary production and developing a mechanistic model of primary production will allow us to make predictions about how primary production will be affected by novel dam operations that might be considered for evaluation as part of the

LTEMP EIS. Conversations with AMP stakeholders indicate they are interested in having such a tool available. Managers may not be interested in identifying flow regimes that maximize primary production, but they are interested in being able to evaluate and predict how operations that are targeted at meeting goals for sand and fish (i.e., trout suppression flows) will affect primary production.

Reviewer 7

1) F.1, Question – It looks like only one annual trip is being planned, please clarify one trip or two and why only one trip is needed now when we used to conduct two and AGFD has provided rational for two in past discussions? (Shane Capron)
   Response: We have re-established a second annual spring trip under Project F.1.
2) F.4.3 – We had been promised by FWS and GCMRC a Chute Falls translocation plan before the next budget cycle. It appears that document has not been prepared and no mention of it is in the description. Please clarify when we can expect to see a Chute Falls translocation plan? We believe it is important to understand the goals of the project, methods, research plan, important results to date, rationale for continuing it, etc. The second to last sentence on next page talks about a peer review, that would be helpful as well but we think a draft translocation plan would be a good starting point. (Shane Capron)
   Response: A draft translocation plan for Grand Canyon has been developed by the U.S. Fish and Wildlife Service and Grand Canyon National Park (Translocation and Refuge Framework for Humpback Chub (*Gila cypha*) in Grand Canyon). The report is being reviewed by the two agencies and by GCMRC prior to further external peer review. We expect the report to be finalized by mid 2013.

Project G.
Interactions between Native Fish and Nonnative Trout

Reviewer 1

1) Project B is most effective at linking the science and monitoring activities to stakeholder objectives….Perhaps this project outline and approach to documentation could be repeated in other projects to provide the critical linkages….a science design of the linkages of individual components to the stakeholder goals, objectives, DFC etc. is not provided.
   Response: Some of these linkages were omitted to try and make the proposals more concise. Linkages have been added back into the proposal for project G to include AMWG priority questions, strategic science questions, research information needs and Science Advisors summary questions as suggested, using Project B as a template.
2) No discussion is provided as to how high system variability is effecting time and costs to obtain the outcomes desired and expected reliability and utility
   Response: High system variability does increase the time and costs to obtain reliable information. This is one of the reasons the laboratory component has been added to the work plan. By controlling some of this variability in the laboratory we can greatly speed up the learning process to more quickly resolve stakeholder information needs in regards to fish and narrow the subset of potential management actions for field experimentation.

3) Research in project G on laboratory experiments project significant accomplishment….yet in reality this research would seem to be used primarily in developing hypothesis that would take years to assess in the Colorado River environment due to high variability.

Response: The value of laboratory research for addressing questions related to complex natural systems is a valid concern. Both laboratory studies and field studies have advantages and disadvantages and it is important for research scientists to acknowledge the limitations of each. Using the combination of laboratory and field data to answer research questions often gives more information than either source alone (Pack 2010, Hairston 1989, Kimball and Levin 1985). Relying solely on field data especially in regard to predation relationships can be problematic. Using only gut content analysis from field data as evidence of predation risk has been shown to be unreliable in some circumstances because of rapid mechanical and chemical digestion of tissues (Schooley et al. 2008), and high temporal and spatial variation in predation rates (Laske, et al. 2012) which can yield misleading interpretations.

The advantages of laboratory studies are that they allow a high degree of control over extraneous variables and ease of replication often at a reduced cost compared with field studies. In the Colorado River in Grand Canyon there are numerous confounding factors that make it very difficult to assess the effects of a single treatment (example: trout removal concurrent with increased water temperatures during the 2003-2006 period). Also, some of the treatments that we would like to evaluate, such as warm mainstem water temperatures (20 °C) at the confluence with the Little Colorado River, never occur under current dam operations. Therefore, evaluating the effects of high water temperatures with sufficient replication in this system becomes difficult if not impossible in the field. Laboratory research also has particular appeal for Grand Canyon fishes because of the difficult logistics and high associated costs of conducting field work in such a remote location.

Physiological responses are typically consistent in both laboratory and natural settings, making laboratory studies particularly useful for evaluating physiological responses of organisms to changes in the environment. Temperature plays a large role in regulating physiological processes in ectothermic animals like fish (Reviewed in Coutant 1976) and thermal effects on fish physiology are commonly studied very effectively in laboratory settings (Reviewed in Brett 1979). Because predation vulnerability is largely driven by physiology (swimming ability of the prey and reactive distance (vision) of the predators), it also lends itself well to laboratory evaluation. Predation vulnerability can have a behavioral component as well (for example, use of cover) which can be altered by the laboratory environment. It is therefore important to validate laboratory results with field studies to verify that the behaviors observed in the laboratory are not distorted. Field verification of laboratory results are common (Johnson, and Li 2010, Kimball and Levin 1985), and typically can be done at a fraction of the cost of a full scale field experiment. In project G we propose to evaluate relative predation vulnerability of juvenile humpback chub to rainbow trout, brown trout and adult chub and to evaluate those relationships as a function of water temperature and turbidity. In each case we are seeking relative measures of predation vulnerability to help us refine and target management actions. Pilot data from laboratory studies conducted thus far indicate that these relative relationships are consistent with field data (Yard et al. 2011). The real strength of adding this laboratory component is that it gives us an additional data source to use in making important management decisions at a relatively low cost.

Reviewer 2

1) It is difficult to track linkages from stakeholders to individual projects….When one reads the individual native and non-native fish projects it is implicit that they represent an integrated approach. However, what is not provided is a science design of the linkages… to stakeholder goals, objectives, DFC's etc.
   Response: This appears to be a common thread identified by reviewers. As indicated in the response to reviewer 1, these linkages were omitted to make the proposals more concise. They have been added back into the proposal for Project G as suggested using Project B as a template.
2) With the significant concerns regarding incorporation of tribal values in non-native fish control programs some expectation existed that efforts to incorporate tribal values would be more apparent in this program.
   Response: We have added wording to the proposal to emphasize that brown trout removal will be undertaken in conjunction with consultation and collaboration with regional Native American tribes.

Reviewer 3

1) Any long-term study should explicitly include in the effort a plan to determine how many samples are needed….
   Response: The laboratory components that are included in project G will assist scientist in answering questions related to sample size. For example, data on digestion and evacuation times collected in the laboratory during predation experiments at varying water temperatures will greatly enhances our ability to interpret gut content analysis from fish collected in the field and assess how many samples are needed to generate a more accurate estimate of predation rates.
2) What is known about the microhabitat distribution of the predators (both trout species) and the chub that are moving through the area? Does this provide any guidance about the places to search for and remove trout…. What is known about the size range of trout that feed on the chub and what sizes of chubs? Seems like these kinds of things must be important and some thinking along these lines could be used to guide the sampling and removal strategy?
   Response: The questions identified by the reviewer are accurate and important. Unfortunately our ability to remove trout is often limited to the locations that they are vulnerable to our gear. These areas include Bright Angel Creek (weir and electrofishing removal), and nearshore areas in the mainstem (electrofishing removal). Fish located in other areas are typically not vulnerable to capture. Understanding the relation between fish size and predation vulnerability is critical in designing and implementing a removal program such that those efforts are effective as possible in reaching management goals. For this reason we are undertaking laboratory studies to help interpret some of these interactions. Some information about the size of predators and prey related to gape size and predation vulnerability is available in the literature, but these relations do not tell the whole story as predation vulnerability can change as a function of water temperature and microenvironment.

3) I didn't see anything on the merit of tracking the size and condition of the fish removed. Are certain sizes classes missed with this method relative to other methods such as netting or fishing? How does that interact with what is known about the size of fish preying on chub.
   Response: Size and condition of fish that are removed will be tracked as a way to evaluate removal efforts. This is a common practice and was likely not explicitly stated in the proposal as it is standard procedure for mechanical removal efforts of this type. Data on fish size and condition will help us to interpret the effectiveness of removal efforts. Electrofishing is selective for larger trout, which are also typically the most piscivorous on native fish. Other collection methods also have their drawbacks. For example, fish captured by hoop net are not useful for diet data because larger fish can eat smaller fish while confined in nets.

Reviewer 4

No comments for Project G.

Reviewer 5

1) I remain very skeptical of the value of lab assays for understanding the patterns in the natural river. The value of this lab approach for understanding predation and competition in the field is dubious at best. The species substitution is just fine, but the conditions in these lab experiments can't come close to capturing the actual controls on the interactions in the field. This must be acknowledged, and there is need for an explanation of why targeted sampling of trout in the field could not be used to assess their actual foraging patterns with respect to experimental flows or natural fluctuations from the monsoon. That would be far more relevant.
   Response: This is a common concern that was expressed by several reviewers. See response to reviewer 1 on the value of laboratory assessments. The second half of this question is about why gut content analysis of fish collected during removal efforts could not be used to assess foraging patterns. Any fish collected during removal efforts will be used as efficiently as possible to gain the most information. This will include gut content analysis on fish collected in areas where little diet information is currently available (such as Bright Angel creek) and for fish collected under conditions for which gut analysis was not conducted during the 2003-2006 removal efforts (Yard et al. 2011).
2) How about sampling juvenile fishes and invertebrates in the drift? 20 d of trout removal with 25% efficiency on first pass should be enough to see effects on drift of invertebrates and fish pretty quickly.
   Response: This is a good suggestion which we will consider. The effectiveness of drift nets is limited to only the smallest size classes (<30-40 mm TL) juvenile fishes because as fish get larger they are able to swim sufficiently well as to avoid capture in these nets. Changes in invertebrate drift rates may be able to be detected using drift nets. Additional foodbase sampling to be conducted in conjunction with any nonnative fish removal efforts at Bright Angel Creek is being considered to address the exact question proposed by the reviewer.

Reviewer 6

1) There is a lot of overlap in sampling effort…. In my opinion some of the sampling programs could be combined in creative ways to give the same answers with less sampling $ and less handling of the fish.
   Response: One of the reasons for proposing laboratory studies on predation and competition between trout and chub was to try and limit sampling and handling of fish in the field. Secondly, multiple studies are being conducted concurrently to avoid redundancy and reduce logistical costs (e.g., Project H combined with Trout Natal Origin project).
2) It would be nice if this unit's statistician could develop a method for combining estimates from electrofishing and hoop and trammel netting into something that gives a robust total population count….Focus the monitoring in a way that moves toward capability for estimating abundance and combining gear types in a rigorous way such that we know how relative abundance (not CPUE) of all members and life history stages of the community are changing over time.
   Response: Unfortunately this is a very difficult task. The reason fish biologists commonly have to use CPUE indexes and relative abundance trend data is that estimating total abundance is very difficult as capture probability varies highly by gear types and environmental conditions making population level inferences that are comparable over time from multiple gear types problematic. Fortunately in many cases this kind of detail is unnecessary in order to understand the important dynamics of a fish population, as most of the important interactions that drive population dynamics relate to survival and recruitment of early life history stages.

Reviewer 7

No comments for Project G.

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Project H.
Understanding the Factors Limiting the Growth of Rainbow Trout in Glen and Marble Canyons

Reviewer 1
No comments for Project H.
Reviewer 2
No comments for Project H.

Reviewer 3

1) How about the hypothesis that a fishery that concentrates on catching and harvesting large fish culls the fast growing genes from the population, yielding a fish population that grows slowly and matures at smaller sizes, never reaching the large size of the more desired but rapidly harvested large fish…..Was fishing pressure high enough to have this kind of influence?
   Response: Very little harvest actually occurs at Lee's Ferry. A majority of the anglers are catch-and-release fly fishermen who are ethically opposed to harvesting fish. Angling regulations at Lee's Ferry also prohibit keeping fish over 12 inches (This regulation change took place in 2003, before that time keeping fish over 16 inches was prohibited) so large fish are not harvested and have not been harvested for many years at Lee's Ferry. Fishing pressure at Lee's ferry because of its limited access is actually very low for a fishery of its size and based on Arizona Game and Fish creel data, angler harvest is minimal.
2) The presence of large numbers of small fish would certainly attract the attention of large rainbows, fish that often take small fish as part of their normal feeding activity. What specific habitat condition do you have in mind here. Habitat condition as a general explanation is just too vague to be very useful in understanding pattern or in defining specific management actions that are likely to resolve the problem.
   Response: Small size classes of trout are believed to be vulnerable to cannibalism, however under current dam operations these sizes of fish utilize habitats that provide refuge from larger trout (Post et al. 1998, Landry et al. 1999). Some scientists believe that under dam operations prior to the early 1990s, which included much lower flows at

certain times, small rainbow trout would have been less able to avoid cannibalism because fish would have been concentrated in less water (perhaps in pools), however published literature indicates that rainbow trout exhibit limited piscivory in nature (Sweetser et al. 2002, Haddix and Budy 2005, Yard et al. 2011) and an abundance of small fish does not necessarily indicate that there is an abundant food source for larger rainbow trout.

3) Seems simplistic to look at prey size without also considering the abundance of prey as a crucial determinant of growth rates. Many trout populations subsist on very small but very abundant prey.
   Response: The bioenergetics modeling will include information on both prey size and abundance. Abundance has been added into the first sentence to clarify. The intent is not to dismiss importance of prey availability and its role on fish growth; however, trout diet data (Mechanical Removal 2003-2004) suggest that prey size limits use and or prey capture efficiency within and among trout species. The reviewer is correct that in some aquatic systems trout subsist on small but very abundant prey; however, most examples are for clear water lentic systems (). In this regulated river system, benthic invertebrates consist primarily of Nearctic dipterans (Stevens et al. 1997; Sublette et al. 1998; Stevens et al. 1998) and other non-native invertebrates that were introduced either intentionally (e.g., G. lacustris) or accidentally (e.g., P. antipodarum) (Blinn and Cole 1991; Cross et al. 2010). Both flow regulation and thermal constraints may have allowed some of these non-native taxa to flourish in the clear water section of Glen Canyon; traits that may not be as adaptable under turbid conditions (Stevens et al. 1997). This aquatic macroinvertebrate community is taxonomically depauperate and small in size. Invertebrate size plays an important role in vulnerability via changes in reactive distance (Shaw et al. 2001) and prey capture efficiency (Buddy 2005).
4) Extrapolating this simple experiment to wild situations seems a dangerous path to me. There are so many complications that are not considered by this approach. At least those complications should be acknowledged here ad a rationale developed to show how their more ominous consequences will be avoided.
   Response: We agree that this is a simple experiment and is designed to evaluate just one hypothesis (that the strain of rainbow trout present in Glen Canyon is incapable of growing to larger sizes). If we can rule out this hypothesis then we can move on to evaluating other explanations. Resolving the validity of this hypothesis continues to be of high importance to many stakeholders and since it can be addressed relatively easily and definitively, we have decided to include it in the work plan. We acknowledge that this is a simple experiment that does not resolve more complicated issues.

Reviewer 4

1) Lab studies of ad libitum outcomes simply provide boundaries that will, I forecast, be substantially in excess of anything observed to date. RBT has been thoroughly studied may time and many places. It’s the ecological context that matters most.
   Response: We agree that the proposed ad libitum experiment to evaluate growth of the Lees Ferry strain of rainbow trout is likely to simply provide some boundaries on growth, but those boundaries may be useful for interpreting other data and are needed to resolve stakeholder concerns. An abundance of literature and information is available on rainbow trout growth and based on that literature, it seems unlikely that the strain would

limit the growth of fish in Glen Canyon, but the experiment needs to be conducted to definitively rule out this hypothesis.

Reviewer 5

1) The lab growth experiment will be of marginal value without direct comparison to other strains raised under the same conditions. And since these fish never see ad lib trout chow in the wild, the results would be dubious anyway. Transferability issues should be addressed.

   Response: This is a valid comment/concern with the same sentiment being expressed by other reviewers. As noted above, this hypothesis is of high importance to several stakeholders. Despite its limitations, we believe the simplicity and relative low cost of this effort is worthwhile as a means to directly address stakeholder concerns on this issue.

Reviewer 6

No comments for Project H.

Reviewer 7

No comments for Project H.

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Project I.
Riparian Vegetation Studies: Response Guilds as a Monitoring and Modeling Approach with Landscape Scale Vegetation Mapping for Change Detection

Reviewer 1

1) What is the rationale for using 250,000 cfs stage as the upper limit of the proposed corridor-wide database to evaluate vegetation?
Response: The database compares vegetation and changes in other resources among datasets. The first dataset, 2002, used this upper limit. The database includes vegetation and sandbars and wind blow sand deposits that potentially affect sites that contain archaeological artifacts up to the 250,000 cfs stage. For vegetation, this is the upper limit of predam vegetation, the status of which is of interest to some stakeholders. This upper limit also provides a measure of the effects of local climate, and the shoreward expansion of xeric vegetation changes.
2) Project I highlights potential effects of tamarisk decline on the protected willow flycatcher and drift food base. Although integration with Project F is cited as a need for high resolution data to extend the remotely sensed data, more detail is necessary to determine the full need for the site surveys.
Response: The project refers to the terrestrial food web. There is no mention of integrating this with Project F. Remotely sensed data will identify the extent of tamarisk defoliation and overtime tamarisk mortality, but this information cannot provide linkages to arthropod response or terrestrial food webs. The text was not changed.

Reviewer 2

1) Here is a lack of integration of science, research and management across disciplines. This document seems to reinforce disaggregation of the science rather than attempt ot integrate across them. This is apparent in the sediment and vegetation programs. More research and monitoring is needed on the interactions between what can be done with policy experiments on the interaction between flow, sediments, vegetation control etc. to push both improvements in recreational beaches and desired plant succession trajectories.
Response: Disagree with this comment. Project I sampling strategy utilized the long-term sandbar sampling sites to incorporate vegetation response to flows and sandbars. Additionally, State and Transition Modeling that would incorporate flow scenarios into a predicted ecological/guild response is being proposed in the revised proposal (see section following the field measurement description). This latter element was initially developed through separate funding. The preliminary model will be brought into the discussion through a proposed workshop in fall 2012 the outcome of this may be further development by a small working group.

Reviewer 3

1) P. 160 in proposal. Ground Dwelling Arthropods section. Consider number of traps, duration trapping.

Response: In the pilot project that utilized this trap-line set-up and trap duration, we determined through a species area curve that the traps sufficiently characterized the sites. The duration of the traps which was from June through September in 2008 was sufficient to differentiate between habitats and sites. This project element was removed.

Reviewer 4

No comments for Project I.

Reviewer 5

No comments for Project I.

Reviewer 6

No comments for Project I.

Reviewer 7

1) In addition to stratifying the sampling sites with respect to geomorphic responsiveness, it could be useful to also characterize sites with respect to magnitude and frequency of recreational use (Use data from Grand Canyon River Guides for large commercial camps).

   Response: Campsite usage information could be included in the discussion of the vegetation response, particularly in relation to those sandbars that are monitored that may be more responsive in heavily vegetated river segments.

   Stakeholder Comment – Larry Stevens, Wildlands Council

1) My concerns with the terrestrial program are that there is no underlying conceptual model to guide the proposed work. My only request is that in addition to the work proposed, you all use the next two years to clearly frame a conceptual ecosystem model that will use the data collected to test and refine. This model should incorporate the river continuum approach developed by Walters and the flow-sediment folks in a landform-based terrestrial model of how debris fan-associated vegetation develops (all types of habitats, not just channel margin, sandbar and rocky surfaces, but also fluvial wetlands and upper riparian zones). The primary question to be addressed is: How does riparian vegetation develop in relation to dam releases, and it will be important to tease out short-term and longer term climate impacts as well as recreation effects.

   Response: A modeling approach was incorporated into the revised proposal with a diagram of one response surface (figure 3. reattachment model). In addition, state and transition modeling, also called frame-based models, is proposed. A preliminary modeling effort using this approach was developed outside of the the Adaptive Management Program and funded separately. Frame-based models provide an opportunity to evaluate knowledge of system response to disturbance or other events that cause shifts in ecological states (Westoby et al. 1989, Starfield, 1990; Starfield and Chapin, 1996; Stringham et al. 2003). Instead of a continuum or response, the model assume a state exists at the end of each growing season—corresponding to annual data collection. The construction of the state and transition models incorporates assumptions of explicitly defined variables that elicit changes in ecological states or guilds. The resulting shifts in ecological states can provide points of discussion among ecologists and resource managers about uncertainty of knowledge, levels of acceptable risk associated with a possible treatment, or the identification of ecological states that are irreversible and require active management (e.g., plant removal and planting).

2) An issue is identified from a Knowledge Assessment workshop related to vegetation and sediment interactions, and specifically loss of camping beach area to vegetation and changes in tamarisk vegetation due to the introduced beetle. This issue is pursued in two of seven presented sediment questions (pg 24). Project A.1.1., in part, is designed to provide necessary assessments. However, insufficient information is provided in A.1.1. to evaluate the expected accuracy of outputs or benefit from the remotely sensed data, except that it will differentiate vegetation from other land forms. For this approach to be effective it would have to differentiate tamarisk, both with and without leaf cover from other vegetation types. Has this level of capability been demonstrated in pilot studies? Will there be an integrated analysis on sites with traditional ground based surveys that would permit assessment of remote sensing effectiveness. It is eluded that ground based surveys would be used for calibrating remotely sensed data models.
   Response: The primary purpose of this project is to map exposed sandbar area. The approach described will allow differentiation between changes caused by changes in vegetation cover and changes caused by sandbar erosion or deposition. Even in the context of tamarisk defoliation, the tamarisk stands will remain and are unlikely to cause changes in exposed sandbar area in the scope of this study period.
3) In response to other aspects of the reviewers question: Our vegetation classification using the 2002 remote sensing data produced a mapping accuracy greater than 80% for tamarisk; the 2009 will do even better. In fact, when we complete vegetation mapping using the 2009 image data we will revisit the 2002 vegetation database to increase its accuracies, then proceed to map 2005 vegetation using the knowledge from both 2002 and 2009 vegetation databases. We have extensive ground truth for 2002 and 2009 vegetation patches that are distributed throughout the canyon; these large, diverse vegetation sites will be revisited during the 2013 image acquisition to update the ground truth sites for the 2013 image analysis.
4) What good is remote sensing?
   Response: These data are the only means to map the surface for the entire corridor; no ground studies can accomplish such mapping over a 4-5 year period, maybe never given the fact we map up to the 250,000 cfs stage. For example, we can map the gross vegetation within a 5-km river reach, up to the 250,000 cfs stage in two hours; accurately classifying species within that gross vegetation would take 1-2 days.
5) Can we distinguish bare tamarisk?
   Response: Within the 4 image bands that we collect, stem and bark of many species have very similar spectral characteristics. If we did not know that a leafless tree was once a leafy tamarisk, then we could only distinguish a bare tamarisk from other bare, woody species by the typical canopy texture (branch structure), but this is unproven. Texture for large stands will be different, but the texture of a young, small tmarisk will probably look very similar to bare shrubs. Basically, we have to rely on the changes in spectral signatures of tamarisk, based on our prior vegetation databases, where prior healthy tamarisk with leaves having a distinct chlorophyll signature will have NO chlorophyll signature for beetle-affected bare tamarisk. Bottom line -- It is easy to detect changes in leaf density and health, but very difficult (with CIR imagery) to detect differences between bare vegetation species without a priori information, which we do fortunately possess.

5) The first paragraph of section 4.1 would be an appropriate place to better justify Project A.4.
Response: The text has been revised as suggested.

Project J. Monitoring Cultural Resources at a Small Scale and Defining the Large-Scale Geomorphic Context of the Processes affecting Cultural Resources

Reviewer 1

1) Reviewer 1 notes that the AMP made a significant investment in developing a mitigation plan for cultural resources (presumably this reviewer is referring to the "treatment plan" for 151 sites developed by Damp and others in 2007). The reviewer wonders whether Project J is replacing the treatment plan, whether that earlier plan has been terminated, and why that earlier plan is not referenced in Project J. The reviewer requests an explanation.
Response: The 2007 treatment plan was developed by Reclamation and the National Park Service to meet some of their respective compliance obligations under the National Historic Preservation Act. After three years of field work, implementation of the plan was temporarily put on hold due to various concerns and issues raised by the tribes involved in the GCDAMP. In the FY2013/2014 work plan, under Reclamation's portion of the work plan and budget, there is a proposal to revisit the treatment plan and revise it in light of tribal concerns. This work falls within the purview of the management agencies and signatories to the Programmatic Agreement for Cultural Resources and is largely outside the purview of GCMRC, hence that work is not discussed in detail within Project J; however, much of the background research and geomorphic assessment work conducted by Utah State University geologists for developing the treatment plan was incorporated into two separate reports authored by O'Brien and Pederson (2009a, 2009b), and both of these reports are extensively referenced in Project J. Moreover, in Project J, we propose to use some of the assessment work done by Pederson and O'Brien as the foundational basis for focusing in on specific sites that are either stable or very unstable. We will evaluate these sites in more detail to determine the geomorphic characteristics and environmental conditions contributing to stable or unstable conditions of sites in the CRe.

Reviewer 2

1) How was tribal input incorporated in the form of goals, objectives and information needs to address tribal values, issues and concerns?
Response: The tribes involved in the AMP have diverse traditions, values, and cultural perspectives regarding archaeological sites and traditional cultural properties in the CRe; however, they all share a concern with minimizing human impacts to these places. They also share a concern about the source of impacts affecting these places, which in turn influences their views about what constitutes appropriate activities to conduct in these

culturally sensitive places. For example, several of the AMP tribes have maintained that if the agents of change that are impacting the sites are from natural sources (e.g., due to rainfall or wind blowing sand or the normal weathering and decay that comes with many years of exposure to the elements), they prefer to see the sites erode and would not support preservation treatments or archaeological excavations, whereas if the cause of the impact is primarily from human actions, then at least two tribes view mitigation through excavation or by some other means as potentially appropriate in certain cases. Project J is sensitive to these diverse perspectives and addresses these tribal interests and concerns to varying degrees, first by deliberating selecting monitoring approaches that maximize information returns while minimizing impacts to the sites, and secondly, by collecting information to help inform both the managers and the tribes about the interplay between various natural and human agents in affecting current site condition in the CRe. The tribes are also conducting their own monitoring programs (see tribal monitoring programs under Reclamation's section of the budget) using a more qualitative approach. The quantitative data being collected under Project J (J.1 and J.2) on changing site conditions and the information on geomorphic contexts of the sites in general (J.3) will complement, but will not duplicate, the more qualitative monitoring data being collected by the individual tribes.

Reviewer 3

No comments for Project J.

Reviewer 4

No comments for Project J.

Reviewer 5

No comments for Project J.

Reviewer 6

No comments for Project J.

Reviewer 7

1) The combination of more detailed monitoring at limited sites using ground-based LIDAR and other techniques, and system-wide monitoring based on remote data is likely to be effective in addressing research questions
   Response: We share the reviewer's optimism about the potential effectiveness of this approach.
2) The sequence of events through time described on p. 186 for a gully affecting an archaeological site represents the type of channel adjustment described in channel evolution models (Schumm et al. 1984; Simon and Castro, 2003; Simon and Rinaldi, 2006). These models have been used to predict channel incision and subsequent adjustment, typically following base level fall, and application of these models to gullies in the Colorado River system might provide some useful insights for management.
   Response: We appreciate this suggestion and have added language in both the science background section and the modeling section to acknowledge the potential utility of these gully evolution models to past and future work.
3) I am surprised that there is no mention of how the archaeological sites to be monitored do or do not overlap with sites to be monitored with respect to sand bars and riparian vegetation. Given the high likelihood that changes in base level, sediment supply, and

sediment stabilization by riparian vegetation influence stability of archaeological sites, it would be useful to understand whether sites chosen for their archaeological value have any geographic overlap with monitoring sites chosen for other reasons, and whether the results of sediment and vegetation monitoring can be directly related to specific archaeological sites. Project J.3, in which supply of eolian sand is considered only in relation to valley geometry, and not in relation to riparian vegetation extent, is an example of the surprising lack of coordination.

Response: We agree with the reviewer that we should have been more explicit about this. For some areas of the canyon it is true that sandbars and riparian vegetation influence the supply of windblown sand to archaeological sites and upland sediment deposits that will be investigated under Project J. The sandbar monitoring sites in project A.1 are not located close to the archaeological sites where intensive monitoring is planned (see sections J.1 and J.2 of Project J); however, we do intend to monitor sandbars located in proximity to those archaeological sites using remote cameras, and those data and imagery could be integrated with the other sand bar data being collected under project A.1. We have added language to the revised proposal (J.1 and J.2) to clarify this point. Sites where vegetation monitoring will be conducted include fixed sites (which are the same as the Project A.1 sand bar sites) and random sites, which have not been selected yet, so it is unclear whether the specific study sites in Project I will be informative at a site-specific level for Project J. However, the vegetation base map will be available for the entire river corridor, and we have added new language in the proposal (under J.3) to clarify the important role that vegetation is known to play with respect to influencing aeolian processes in some places and to clarify that where riparian vegetation monitoring data is available in close proximity to monitored sites, we will be using those data to help interpret local conditions. This is reflected in the newly added statement Section J.3: "This work will link to studies conducted under Project A (fluvial sandbar studies) and Project I (vegetation studies) in some of the study localities—the locations and enlargement of sandbars (Project A) is known to affect sand supply into modern-fluvial-sourced dune fields, and the effectiveness of this process depends in part upon the amount of riparian vegetation bordering the sandbars (Project I, between the sandbar and upland areas)" (see also the sentences that follow in section J.3). As part of the historical aerial-photograph analysis planned for Project J, we also plan to investigate changes in vegetation (riparian and upland) over time, the results of which will be communicated to scientists on Projects A and I; it is already known, for example, that both riparian and upland vegetation area and density has increased in postdam time (Turner and Karpiscak, 1980; Webb and others, 2011), which likely has limited the supply and activity of windblown sand in the river corridor. This is now discussed in the revised proposal (section J.3). However, it would be scientifically unsound to try to link every study site investigated by Project J with the sandbar and vegetation study sites of Projects A and I, because as we have now stated clearly in the revised proposal: ". . . the investigations in Project J include many sites and large areas where, because of the lack of postdam floods, the aeolian landscapes and cultural sites within them are disconnected from modern river sandbars and riparian vegetation (Draut, 2012); the (desert) vegetation assemblage in relict aeolian dunes above the postdam high water line is distinctly different from the riparian vegetation assemblage and does not have an apparent connection to dam-controlled flows (see vegetation study by Draut, 2011)… in many upland sediment deposits and associated archaeological sites (those that are in relict-fluvial-sourced dune fields; Draut, 2012) the prevailing wind direction does not supply sand from modern, HFE-deposited fluvial sandbars (such as at Palisades, RM 66), but rather the source of sediment in the dune field and protecting the cultural sites was left by predam floods larger than any postdam flows have been." Therefore, the results of Projects A and I will relate to some, but not all, of the sites and results investigated in Project J.