Selection Panel Review Summary

Proposal No.: 022
Proposal Title: Linking habitat and spatial variability to native fish predation
Principal Investigator: Bernie May
Amount Requested: $730,307.00
Recommended Amount: $730,307.00

Summary: This proposal would use genetic assays to identify the presence of Chinook salmon, steelhead trout, Delta and longfin smelt, white and green sturgeon, and Sacramento splittail in the stomachs of predatory fishes (striped bass and largemouth bass) as well as the native piscivore, Sacramento pikeminnow (*Ptychocheilus grandis*)), across migration corridors and habitats of the north Delta. Subsidiary studies of evacuation rates will contribute to estimating predation rates. Results are to be combined with bioenergetic models to investigate population impacts.

Assessment: The Selection Panel found this to be an innovative proposal that would increase knowledge of predation in the decline of native fishes. Predation is a key stressor and meets PSP priorities - specifically BDCP implementation and OCAP requirements. The project team is credible and the Selection Panel found the linkages being made between predation and bioenergetics to be cutting edge. It will improve our understanding of predation in the north Delta. It will help develop analytical tools like DNA tags. However, it does not address how predation relates to flow, temperature, and turbidity. No performance measures were presented to apply to management. Finally, it would be good for the proposal to include how structures, such as the Sacramento drinking water intake on the Sacramento River, affect predation. The effort to connect these predation results into broader bioenergetic and survival models is admirable but the data are unlikely to match the needs of the model.

CALFED Ecosystem Restoration Program
External Scientific Review Form

Proposal Number: 022

Proposal Title: Linking habitat and spatial variability to native fish predation

Reviewer: #1

Conflict of Interest Statements:

I have no financial interest in this proposal (please mark correct response).

- Correct –

General Review Questions:

Along with your written observations in response to the questions below, please rate each using the following criteria:

Excellent: Outstanding in all respects
Very Good: High quality in nearly all aspects
Good: Quality work, but with some deficiencies
Fair: Lacking in one or more critical aspects
Poor: Serious deficiencies

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives, and hypotheses clearly stated and internally consistent? Does the proposal describe the ecosystem goals it is designed to address (link to ERP goals)?

Comments:

The answers to the questions above are yes.

Rating: Very good

2. **Approach.** Does the proposal clearly describe its approach (including study design and methods, if appropriate)? Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? Will the proposal contribute to our knowledge base?

Comments:

Question 1 is unclear and which predators are they talking about?

Question 2 is excellent and this is one of the few proposals that I have read that explicitly addresses the fact that biological phenomena in the Delta vary both seasonally and annually.

Questions 3 & 4 are fine but it seems a huge waste (Question 4) to limit striped bass predation studies to Chinook salmon alone (P.6) when analyzable stomach materials will be available for other species as well. Efforts should be made to analyze these materials for more species because predation may be important to many of the species of concern in the Delta and striped bass are known to be highly efficient predators.

It is not clear to me that the PI's can actually detect the amount of a given prey (or rate of prey consumption) consumed by individual striped bass as opposed to just being able to detect that the prey was consumed. Obviously, for predation impact studies the rate of prey consumption is what is essential not just the presence or absence of predation. There is little discussion of whether the proposed technique can do this and I did not see much evidence that this could be done with fish.

It is unfortunate that the PI's have proposed to sample predators via gill-netting. Predators frequently regurgitate when caught in gill nets as they try and untangle themselves via "backing out" of the net. Another sampling method should be used such as boat electroshocking. Even angling might be better than gill-netting although it selects for hungry fish. In addition, what is the point of randomly assigning gill nets to a short orientation. Don't the PI's know which orientation maximized catch of predators? Random sampling in this context would be useful for determining the spatial distribution of the predators but not for maximizing catch rates.

The detection rate experiments are good and necessary for this type of work

The use of bioenergetic models to estimate predation rates relies on highly accurate estimates of population size (i.e. low confidence intervals around the estimate) – do the population estimates for striped bass and other potential predators meet these criteria? Are accurate population estimates for pikeminnow, largemouth bass, smallmouth bass and silverside available?

Use of the Delta Passage Model is of concern because it has not undergone a final peer review via publication in a scientific journal.

It is unclear how sampling for water quality data will be conducted, especially with respect to spatial coverage of the site. Will the PI's only collect data at one spot, then there will be no replication within sites to contrast to variation between sites.

Are the numbers in Table 2 for the entire study, for a given year or for a given site (not likely)? The numbers for all species except striped bass are really insufficient for a study with such broad spatial coverage.

P. 12 paragraph 3 – I found the statements about the inadequacy of visual methods of dietary analysis (ideally coupled with volumetric or gravimetric analysis) to be overstated. Yes visual methods have their problems but frankly they're probably no more biased (although differently) than molecular methods. In addition, it is worth noting that the Corse et al. paper deals with feces not gut contents and the sample sizes are very small therefore it may have been easy to separate predators by diet because there was reduced intraspecific variation in diet.

It sounds like the PI's have the molecular dietary method nailed and it was good to read about specific examples. A figure presenting some of these results would have been even better.

Nonetheless, it still is unclear to me if the PI's can detect the relative amount of a given prey in a gut using this technique or can they just tell if a prey type is eaten or not?

The possibility of using the assay to detect "subpopulations" of salmon would be highly valuable and is to be commended.

The feeding trials are good but there is little information on temperature which can greatly influence gastric evacuation rates. These experiments should be conducted at multiple temperatures that represent those occurring in the N. Delta when Chinook are present. This is crucial to accurate estimates of field evacuation rates. Also from the looks of the title the Hurst paper deals with YOY stripers and evacuation rates for adult versus young fish can vary highly.

Use of the Loboschefsky model also is problematical because it has not undergone peer review and the journal to which it is submitted is not one that would be rated highly by modelers, okay I see Kenny Rose is a co-author so you've got good credibility there.

Given that this proposal focuses on estimating predation rates it is problematical to see a model described that already has consumption rates in it. In addition, some of the formulae were unreadable in my copy of the proposal.

As mentioned above prey consumed is not an index of prey availability (p. 15 "The variable P can be viewed as a measure of prey availability"). There are many fish feeding choice studies that indicate that some prey are selected for, some against and some consumed in proportion to their abundance in the environment. In addition, there is little evidence that vertebrate predators consume prey in direct proportion to their abundance alone (see other comments about Type 1-3 functional responses). This is a very poor assumption of the model, however it could be solved by assuming that different functional responses existed and doing different runs with those responses.

Once again, an unvetted model is a critical part of the analyses to be conducted in Task 4.

Rating: Ideas are excellent, methods are between good and fair

3. **Feasibility.** Is the proposed project's approach fully documented and technically feasible? Can the project be completed within reasonably foreseeable constraints (e.g., acquiring permits, construction, weather, etc…)? Does the proposal thoroughly address requirements such as environmental compliance and permitting? Is the scale of the project consistent with the objectives?

Comments:

Generally feasible from a conceptual point of view, although I can't really evaluate the permitting aspects of the proposal. Most of the ideas are very good and well within the scope of the RFP, but due to the methodological shortcomings it is difficult to rate this work as highly feasible. I believe that the PI's will build a model (Task 4) but I suspect that given all the assumptions and limitations of data that it will have limited accuracy, intermediate precision and limited management utility.

Rating: between good and fair

4. **Conceptual Model.** Does the proposal provide a conceptual model that describes the interconnections among the key ecosystem components relevant to the action(s) being proposed? Does the conceptual model clearly explain the hypotheses it is testing?

Comments:

The conceptual model and description of the predation process are overly simplistic. Prey capture probabilities do not always increase with prey density and all three functional response curves have asymptotic phases and a Type III functional response which is likely for these predators has two inflection points (see any basic ecology text book for functional response curves).

P.7 -- The probability of consuming a prey varies as a function of the size distribution within the predator population not "the average size of the predator". The latter metric is useful for estimating the effects of the predator population on the prey population, but actually a direct measure of the size distribution present within the predator population is a much better index of the probability of predation than the mean size alone.

P.9 line 7—the curves in the Nobriega and Feyer paper are not "functional responses" but are probability of consumption curves. These two things are very different because the functional response is the per capita capture rate of a predator per unit time versus prey density.

P. 9 – stop calling these curves functional responses they are probability of capture curves and they're a bit problematical because it is not clear if they are per capita or some sort of mean for the population.

I thought Table 1 was well constructed and useful although these are predictions not hypotheses.

Rating: Good

5. **Performance Evaluation Plan (Monitoring Plan and Performance Measures).** Does the proposal include a plan for project performance evaluation (monitoring to assess results and evaluate assumptions and hypotheses)? Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Will future studies or restoration projects be able to incorporate the information from this project?

Comments:

I didn't see anything on performance evaluation in the proposal and a search under that term did not turn up any usage in the document.

Rating: Non-existent

6. **Expected Products/Outcomes.** Are products of value likely from the project? Are products of value also likely from the individual components of the project? Will the results of this study be readily accessible?

Comments:

Conceptually the potential products are of high value, however, from a practical standpoint, the methodological shortcomings limit my ability to rate these products highly. Listing a final report as a deliverable seems strange, I've never had a grant that didn't **require** a final report.

Rating: Fair

7. **Previous Related Work.** Does the proposed project continue past work or include any work that could be considered a duplication of work previously done or currently being done by others?

Comments:

I can't really assess this, but I will say that the work proposed definitely needs to be done.

Rating: Don't know.

8. **Qualifications.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project? Do they have working knowledge of California streams and rivers?

Comments:

Prof. May certainly has a strong track record in genetic studies of fishes, Baerwald and Conrad are perhaps the next best qualified although neither has a strong publication record, even when one takes their "academic ages" into account. Perhaps most troubling is the description of Bradley Cavallo's qualifications "Cavallo is a recognized expert in the ecology and management of Central Valley salmonids. He holds degrees from the University of California at Davis (B.S. 1994) and University of Montana (M.S. 1997). Cavallo has more than 13 years working to understand and resolve ecosystem problems of the Central Valley. Brad is the lead developer of the Delta Passage Model and has authored numerous technical and peer-reviewed fishery studies, and regularly presents results of his scientific endeavors at public policy forums and professional society conferences." However, when I examined both "Web of Science" and "Aquatic Sciences and Fisheries Abstracts" neither bibliographic index contained a single refereed journal article that listed Cavallo as an author despite the claim above. I do not know what your policy is about "vita padding" but clearly the description of Mr. Cavallo's publication record is highly inaccurate. I find this very disconcerting.

Rating: Disappointing

9. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed? If the budget is considered to be excessive or inadequate for the work proposed, please highlight areas of the budget that may be of concern.

Comments:

For a budget of 700K+ more should be produced than 3 journal articles and a few talks at meetings. Seems like the agency is being low-balled on products. Listing a final report as a deliverable seems strange, I've never had a grant that didn't **require** a final report.

It's interesting that hourly rates for consulting personnel with BSc degrees actually are higher than those for PhD's at UC.

In general, the budget should probably be cut if they can't deliver more than they've promised. After all, three journal publications pretty much just represents one PhD dissertation so figure out the cost of 4-5 years of assistantship and compare the results to decide whether you have a good cost/benefit ratio.

Rating: Fair

Additional comments:

This proposal has some very good ideas but at times I really was not sure that they knew what they were doing. Certainly the most qualified PI has little experience in the non-genetic aspects of this study and it would have been nice to see another senior PI familiar with fish behavior and ecology.

Overall Evaluation Summary Rating

In the space below, please provide an overall rating of the proposal using one of the following categories:

- **Superior:** Outstanding in all respects with superior technical and scientific value and no significant concerns. Expected to add substantial new thinking/concepts to our knowledge/understanding of the topic proposed.
- **Above Average:** A very good proposal with at least high technical and scientific value and no significant concerns. Will add solid basic knowledge/understanding of the topic proposed.
- **Adequate:** A reasonable proposal without serious technical deficiencies and at least adequate value scientifically. Will add some useful knowledge to the topic proposed.
- **Inadequate:** A technically deficient proposal and/or one with low value, serious impediments or concerns. Will not likely change our basic knowledge/understanding of the topic proposed.

Rating: Above Average conceptually, and between Adequate and Inadequate on methodology.

Please provide a brief explanation of your summary rating:

There are just too many unanswered questions to be comfortable funding this proposal at this stage.

CALFED Ecosystem Restoration Program
External Scientific Review Form

Proposal Number: 022

Proposal Title: Linking habitat and spatial variability to native fish predation

Reviewer: #2

Conflict of Interest Statements:

I have no financial interest in this proposal (please mark correct response).

X- Correct
- Incorrect

General Review Questions:

Along with your written observations in response to the questions below, please rate each using the following criteria:

Excellent: Outstanding in all respects
Very Good: High quality in nearly all aspects
Good: Quality work, but with some deficiencies
Fair: Lacking in one or more critical aspects
Poor: Serious deficiencies

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives, and hypotheses clearly stated and internally consistent? Does the proposal describe the ecosystem goals it is designed to address (link to ERP goals)?

   Comments:

   This is a well prepared proposal with clearly stated goals linked to hypotheses, approach, and deliverables. The problem of predation by native and introduced predators (particularly striped bass) is highly relevant to ecosystem management within the Central Valley. I found the four central questions ecologically interesting and relevant to important issue of native fish declines.

   Rating: Excellent

2. **Approach.** Does the proposal clearly describe its approach (including study design and methods, if appropriate)? Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? Will the proposal contribute to our knowledge base?

   Comments:

   The genetics approach for detecting incidence of predation on native fishes is presented in a careful and deliberate manner. Care is taken to further verify approaches, which have been developed only in a preliminary manner to date. This includes one central aspect of the study, which is to determine the effect of digestion time on prey detection. (Here, I would suggest that too many duration periods are used at the expense of treatment replicates – 6 time periods would support a better experimental design). The survey design (2 years of gill net deployments in specific reaches where juvenile salmon mortality rates have been measured through biotelemetry) was well laid out and seemed feasible. The bioenergetics model was only partially

developed due to constraints on data inputs and may not support hypothesis testing related to consumption rates and demand by predators.

I also found it curious that conventional diet analysis was not described in the proposal – it would seem important to conduct this as well as a means of corroborating the genetic approach, but also for detecting more abundant prey species (e.g., striped bass?) which are not the target of the genetics technique.

Rating: Very Good

3. **Feasibility.** Is the proposed project's approach fully documented and technically feasible? Can the project be completed within reasonably foreseeable constraints (e.g., acquiring permits, construction, weather, etc…)? Does the proposal thoroughly address requirements such as environmental compliance and permitting? Is the scale of the project consistent with the objectives?

Comments:

Genetic evidence of prey seems initially feasible, warranting further testing and application. A central limitation however is that this approach only detects incidence, which limits its usefulness in bioenergetics modeling. There is an important opportunity to evaluate relative predation rates on specific Chinook populations. Still, there is no way to scale up to consumption rate or demand without measuring the mass of consumed fish. A series of equations seek to develop a consumption index from incidence data but for the most part this trades one set of limiting assumptions for another. Several issues I noted were (1) although "p" is a ratio and unitless, it is in fact a ratio of mass weights of biota consumed (2) the range of feasible scenarios to estimate parameters alpha (prey abundance) or D (striped bass abundance) would seem to make this exercise untenable; (3) I found myself very skeptical of equation 1 which found a linear relationship between consumption, fish size and temperature. This flies in the face of theory (e.g., Winberg) and seems to be an engineered fit, rather than one that is justified by sound bioenergetics modeling.

The team has carefully considered permitting issues and ways to sample around threatened species. One concern however is that winter –early spring predators may be consuming at lower rates than at other times of the year. Predictions of salmon prey abundance by the Delta Passage Model is not detailed nor is it available for review, causing some skepticism in whether this will provide useful or publishable information.

Rating: Good

4. **Conceptual Model.** Does the proposal provide a conceptual model that describes the interconnections among the key ecosystem components relevant to the action(s) being proposed? Does the conceptual model clearly explain the hypotheses it is testing?

Comments:

I thought the functional response conceptual model was an innovative means to relate the limits of the data (detection incidence) to types of predator-prey systems that might be observed (Table 1). The tabulated set of expectations clearly linked hypotheses to functional response

theory. The conceptual model was a bit narrower in this proposal – not an ecosystem model per se – but still relevant to ecosystem issues.

Rating: Very Good

5. **Performance Evaluation Plan (Monitoring Plan and Performance Measures).** Does the proposal include a plan for project performance evaluation (monitoring to assess results and evaluate assumptions and hypotheses)? Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Will future studies or restoration projects be able to incorporate the information from this project?

   Comments:

   I could find no description of how PIs will monitor performance.

   Rating: Not Rated

6. **Expected Products/Outcomes.** Are products of value likely from the project? Are products of value also likely from the individual components of the project? Will the results of this study be readily accessible?

   Comments:

   Tool development and publications seem to be principal outcomes. Additional consideration might have been given to how to get relevant results into stakeholder's hands.

   Rating: Very Good

7. **Previous Related Work.** Does the proposed project continue past work or include any work that could be considered a duplication of work previously done or currently being done by others?

   Comments:

   PI May has an extensive history of doing high impact genetic/ecological work in the Central Valley.

   Rating: Excellent

8. **Qualifications.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project? Do they have working knowledge of California streams and rivers?

   Comments:

   Expertise and qualifications have been well established for genetics and sampling design and field work. Given shortfalls and lack of publications on bioenergetic modeling by relevant scientists, I did have concerns on this component of the work.

Rating: Very Good

9. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed? If the budget is considered to be excessive or inadequate for the work proposed, please highlight areas of the budget that may be of concern.

   Comments:

   The budget seems reasonable for this type of project.

   Rating: Very Good

Additional comments:

None.

Overall Evaluation Summary Rating

In the space below, please provide an overall rating of the proposal using one of the following categories:

- **Superior:** Outstanding in all respects with superior technical and scientific value and no significant concerns. Expected to add substantial new thinking/concepts to our knowledge/understanding of the topic proposed.
- **Above Average:** A very good proposal with at least high technical and scientific value and no significant concerns. Will add solid basic knowledge/understanding of the topic proposed.
- **Adequate:** A reasonable proposal without serious technical deficiencies and at least adequate value scientifically. Will add some useful knowledge to the topic proposed.
- **Inadequate:** A technically deficient proposal and/or one with low value, serious impediments or concerns. Will not likely change our basic knowledge/understanding of the topic proposed.

Rating: **Superior-Above Average**

Please provide a brief explanation of your summary rating:

This is an innovative proposal that could greatly expand understanding of the role of predation in the decline of native fishes in the Central Valley. The emphasis on bioenergetic modeling in the proposal was a valiant effort in scaling results up to system scale inferences, but is probably not feasible. The proposal still was compelling in addressing incidence of predation in the context of functional responses. I found the proposal well prepared in presenting the inter-relationship of all study elements and their link to an interesting and important set of questions.

Proposal Number: 022

Proposal Title: Linking habitat and spatial variability to native fish predation

Reviewer: #3

Conflict of Interest Statements:

I have no financial interest in this proposal (please mark correct response).

- Correct XX
- Incorrect

General Review Questions:

Along with your written observations in response to the questions below, please rate each using the following criteria:

Excellent: Outstanding in all respects
Very Good: High quality in nearly all aspects
Good: Quality work, but with some deficiencies
Fair: Lacking in one or more critical aspects
Poor: Serious deficiencies

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives, and hypotheses clearly stated and internally consistent? Does the proposal describe the ecosystem goals it is designed to address (link to ERP goals)?

Comments:

The proposal adequately describes the problem that the project is designed to address throughout the proposal. In concept the problem is relatively simple. In practice it is a complex problem to resolve and the issue of predation on native species under population stress has been an issue managers within the Delta have wrestled with for awhile.

The goal of this research proposal is to link predation rates of important predators to habitat specific conditions including prey density, temperature and other important variables that might influence predation rates. Emphasis is on relatively rare but important emigrating Chinook salmon and other potentially important environmentally sensitive species such as Delta smelt. The effort seems to be highly focused on Chinook but the study as proposed will provide ancillary information on the incidence of other species within the stomach contents of key piscivorous predators in the bay-delta system. In my reading of the proposal the investigators intend to focus the bioenergetics model primarily on impacts to Chinook as they out-migrate but the approach should be adaptable to look at impacts to other species.

Rating: Excellent

2. **Approach.** Does the proposal clearly describe its approach (including study design and methods, if appropriate)? Is the approach well designed and appropriate for meeting the

objectives of the project as described in the proposal? Will the proposal contribute to our knowledge base?

Comments:

The methods and some of their potential limitations are clearly spelled out in the proposal and it is apparent that this group has given this problem a fair bit of thought and in fact they have some experience with this approach in research utilizing these same methods to study the predation on early life history stages of other species (delta smelt). Regardless of the final outcome of this proposed effort it should add to the knowledge base for understanding predation for the prominent predators targeted in this study. I believe the molecular approach outlined in the proposal is a good one but I am compelled to point out some of the limitations to the approach not emphasized in the proposal. The major one that is pointed out in the proposal is that much of the outcome of the modeling will be based upon assumptions about precise rates of contact between predators and prey that cannot be verified. One of my concerns is that outmigration of the species of concern can be highly synchronous at times in response to temperature or flow events and these episodic events may be relatively short in duration but moving a large number of the target species through the predation gauntlet. How will the predator sampling take this into account? Modeling for these different scenarios might provide some insights into the possible outcomes but no effort along these lines was proposed.

One part of the methods where the rationale is not clearly stated is the estimation of the gut retention times that allow detection of a prey item. The rate of digestion of prey will be temperature sensitive and is also likely to be affected by ingestion rates and activity of the predators but they proposed to establish it at only a single relatively low temperature of 12C in tanks. I'm guessing they are establishing a maximum length of time but I am not certain. If this were my study I would run it at least two temperatures. I would also photograph remains over time and use that in my field studies to help detect prey types. If a prey item is recognizable in a stomach why not use that along with the DNA typing? After all, the DNA assays will only provide a positive for the species tested by the assays. It would be valuable to know if other DNA is showing up in stomach contents and if identifiable visually why not ID it and look for patterns?

Although stomach analyses are tedious and have limitations why not get something out of them if you can such as numbers of prey and relative sizes to support the modeling. Obviously previous diet studies were able to identify remains. This added information would improve the basis for modeling. These data along with the molecular methods would strengthen the overall knowledge base and accuracy of the modeling.

One issue not addressed in the proposal and as far as I can tell not given much thought with regard to the predation issue in the delta is the probability that some level of cannibalism takes place for prominent predators and that piscivorous predators will occasionally eat each other's young stages. It seems to me that this is an important issue to consider if as a possible management action you intend to try to manipulate the abundance of predators in the system. Manipulating a specific predator in order to reduce its abundance may lead to unintended consequences of that management action.

The other issue this proposal does not or cannot address is the issue of turbidity within the Delta which may be an important issue that has changed the relative predation rates of different predators and has some importance consequences for modeling habitat specific predation rates. No mention is made of this issue in the proposal but for large predators turbidity and light are very big issues when it comes to reactive distances and predation and has probably affected predation dynamics in the Delta as the water has become less turbid.

It is not clear how the relative abundances of the predators to be tested were selected. Emphasis for both modeling and the bioenergetics is on striped bass and Chinook as prey. It seems that the PIs believe this is the most important biotic interaction with regard to predation of Chinook but by taking vastly different numbers of predators it may be more difficult to compare results by species.

Rating: Very Good

3. **Feasibility.** Is the proposed project's approach fully documented and technically feasible? Can the project be completed within reasonably foreseeable constraints (e.g., acquiring permits, construction, weather, etc…)? Does the proposal thoroughly address requirements such as environmental compliance and permitting? Is the scale of the project consistent with the objectives?

Comments:

This is a research not a restoration project but a research project in support of potential restoration efforts. This research is highly feasible and the scale is reasonable for the objectives and requested dollars. They have some permits in place and the others they require that should not be a problem given their previous similar projects and their knowledge of the Federal and state permitting process.

Rating: Excellent

4. **Conceptual Model.** Does the proposal provide a conceptual model that describes the interconnections among the key ecosystem components relevant to the action(s) being proposed? Does the conceptual model clearly explain the hypotheses it is testing?

Comments:

The conceptual model could have been better developed by making specific hypotheses on the likely seasons and places within the Delta where significant interactions of predators and prey of special interest are likely to take place. The conceptual model also ignores important issues related to the predation process (see approach section for specifics).

Rating: Good

5. **Performance Evaluation Plan (Monitoring Plan and Performance Measures).** Does the proposal include a plan for project performance evaluation (monitoring to assess results and evaluate assumptions and hypotheses)? Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Will future studies or restoration projects be able to incorporate the information from this project?

Comments:

This is not a restoration proposal so no post project monitoring is required. The data should be available to future restoration efforts but no easily identifiable performance measures are identified in the proposal. Deliverables are indentified as peer reviewed scientific papers and report as a measures of success.

Rating: Fair

6. **Expected Products/Outcomes.** Are products of value likely from the project? Are products of value also likely from the individual components of the project? Will the results of this study be readily accessible?

Comments:

The predation issue seems to be something that is a lingering concern within the Delta ecosystem and a difficult one to quantify with certainty. Any progress is likely to be valuable to managers. Given the proposed reporting and publications the data is likely to be accessible.

Rating: Very good.

7. **Previous Related Work.** Does the proposed project continue past work or include any work that could be considered a duplication of work previously done or currently being done by others?

Comments:

As stated earlier, members of this team have a current project underway using DNA methods to identify predators on young delta smelt. This effort will not duplicate that effort, but previous efforts will help provide a better basis for identifying a wider array of prey in the stomachs of large piscivorous predators.

Rating: Very Good

8. **Qualifications.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project? Do they have working knowledge of California streams and rivers?

Comments:

This team is highly qualified and especially well-suited to this particular research effort. The team has a very good working knowledge of the ecosystem, experience and interest in the predation issue, and they possess the working knowledge and the infrastructure to accomplish the research goals.

Rating: Excellent

9. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed? If the budget is considered to be excessive or inadequate for the work proposed, please highlight areas of the budget that may be of concern.

Comments:

The budget seems reasonable for a multi-institutional and interdisciplinary team.

Rating: Very Good

Additional comments:

None.

Overall Evaluation Summary Rating

In the space below, please provide an overall rating of the proposal using one of the following categories:

- **Superior:** Outstanding in all respects with superior technical and scientific value and no significant concerns. Expected to add substantial new thinking/concepts to our knowledge/understanding of the topic proposed.
- **Above Average:** A very good proposal with at least high technical and scientific value and no significant concerns. Will add solid basic knowledge/understanding of the topic proposed.
- **Adequate:** A reasonable proposal without serious technical deficiencies and at least adequate value scientifically. Will add some useful knowledge to the topic proposed.
- **Inadequate:** A technically deficient proposal and/or one with low value, serious impediments or concerns. Will not likely change our basic knowledge/understanding of the topic proposed.

Rating: **Above average**

Please provide a brief explanation of your summary rating:

This is a difficult problem to address, but this effort is superior to many of the other approaches I have seen proposed in the past to look at the predation issue within the delta. Nevertheless, I believe many improvements could be made to this effort to get the most return for a buck. I have mentioned some of these above but there are other things that could be done to learn more about predators and prey dynamics in the system as part of this study. For example, many of the young Chinook from hatcheries end up being tagged with coded wire tags. It would add to the knowledge base if positives in predator stomach (i.e. presence of Chinook) could be scanned for tagged fish because it would be a known release time/place and if Chinook positives are significant in numbers within predator stomachs during release periods or known outmigration periods, then tagged vs untagged ratios could be estimated and used by management. The discovery of a few tagged fish could also help verify the methods in the field.