Selection Panel Review Summary

Proposal No.: 021
Proposal Title: If we build it, will they come?: Identifying habitat characteristics that support native fish in the Delta and Suisun Marsh
Principal Investigator: Dr. Peter Moyle (UC Davis)
Amount Requested: $1,152,195
Recommended Amount: $1,152,195

Summary: The purpose of this research is to develop a better understanding of how physical habitat, flow, and other factors interact to maintain assemblages of native and non-native species in an environmental gradient that supports populations of most of the native fishes in the upper estuary. By documenting how native and alien fishes use habitat around Suisun Marsh, Sherman Island, and the Cache Slough complex, insights can be gained and hypotheses tested that will aid the recovery of at-risk native species, inform flow and habitat management decisions, and allow for better adaptation to climate change.

Assessment: The Selection Panel was favorable about the ideas in this proposal. They found the goals to be excellent, and the conceptual model and performance plan good. The project team is highly qualified. The proposal focuses on key species and is located in a high priority region. Overall, the Selection Panel felt this proposal would we helpful with restoration and management decisions. However, the description of work needs to be strengthened, as it was very general with insufficient linkages between hypotheses and approaches and it needs to integrate with other ongoing work.

CALFED Ecosystem Restoration Program
External Scientific Review Form

Proposal Number: 021

Proposal Title: If we build it, will they come?: Identifying habitat characteristics that support native fish in the Delta and Suisun Marsh

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:

   This proposal is highly relevant to the ERP goals of habitat analysis and restoration and other ecosystem processes descried in DRERIP analysis of the (2009) of the BDCP. In particular, the focus on resident Delta species or those that spend significant portions of their life in this habitat, is commendable. In addition, the focus on connectivity addresses one of the central problems in restoration of ecosystem processes in the Delta, namely restoring historical flow patterns which will mitigate losses due to diversion and "ecological traps"

   I was surprised that the conceptual model (Fig. 1) didn't include "overall flow" (i.e., high water year, low water year, etc) which affect almost everything depicted. In addition, several of the figures were difficult to interpret (Fig. 2) or had missing information (the x-axis on Fig. 3 is unlabeled but presumably is "days".

   The PI's assume that all flow-biology relationships are linear when in fact they may be non-linear (e.g., Hypothesis 1). I'm not an expert on Delta hydrology, but isn't it possible that high flows "seal" some small or "dead end" sloughs which would produce a high residence time that was positively correlated with flow over some portions of the flow-residence time distribution?

In addition, is high residence time always good -- couldn't nuisance algal blooms develop in these areas if conditions were right? There already are cyanobacteria blooms in the Delta? Consequently, although I largely agree with the conceptual approach of the proposal it contains some generalizations that warrant revision.

Rating: **Excellent** conceptually, **good** based on the details

2. <u>**Approach.**</u> 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:

Kudos for acknowledging that historical data are biased because the sampling design was for pelagic species alone.

Some of the hypotheses are complex and not really amenable to simple testing. For example, take

> *Hypothesis 2*: Phytoplankton, zooplankton, and benthos abundance will be highest in areas of high water residence time, hence areas of high shoreline development and high network complexity.

> *Hypothesis 3*: Native fishes will be more abundant in areas with high zooplankton and benthos abundances and low abundances of most alien fishes.

> *Hypothesis 4*: Native fishes will be more abundant in areas with more variable salinities, cooler summer temperatures, and lower submersed aquatic vegetation (SAV) density.

These all contain multiple factors/processes, many of which are correlated, and there really is insufficient description on how they will be tested/untangled even with the complicated statistical tests described. An example really would have helped here. In addition, there is substantial debate over the use of Bayesian Analysis in this context, especially if you have no biological basis to set the priors. Of course you can set all priors to be equal but if you don't have realistic priors why are you using a Bayesian approach? Analysis/testing of some of these hypotheses might be much more fruitful if an information theoretic approach was used. Clearly Moyle knows what he is doing biologically, yet in some ways this proposal made me think that this is a "measure everything and let the statistics sort it out" approach rather than a more tightly focused proposal that dealt with fewer potential drivers. In one sense this criticism is unfair because there is so much that is unknown, but I think that the experimental design/statistical sections of the proposal could certainly been more focused perhaps with examples of the utility of the complicated statistical approaches using the data from past studies.

I was puzzled by the statement on p. 16 "The relationship among water quality characteristics, plankton, and fish species abundance will be modeled using PCA to produce a variance-covariance matrix." PCA really isn't a modeling technique and PCA uses either a variance-covariance or a correlation matrix for its solution, but my guess is that a simple statistical package could create a variance-covariance matrix independent of PCA. Further on in

this paragraph we are told that a hierarchical model is necessary because of repeated sampling, but this requires a repeated-measures analysis, not necessarily a hierarchical analysis. In addition, we are told that the models will be "compared with AIC" but surely the PI's mean the models will be evaluated using AIC. Clearer writing would have helped here and the lack of clarity gave me concern given the comments above regarding the statistical fishing expedition mentioned above.

There really is insufficient detail for both the nutrient and zooplankton methodologies, and I didn't see any description of the methods, sample sizes, etc to be used in the tagging studies. That is very disappointing and leaves a reviewer in a quandary.

The data for Sr isotopic analysis may have some shortcomings because some areas in the Delta have similar Sr ratios (e.g. Cache Slough and the Napa River, Fig. 3). If the PI's are going to track habitat use and connectivity via these ratios, how will they distinguish between areas with similar ratios? In addition, there is little information on seasonal or annual variation of these ratios. If this variation exists it will greatly complicate the ability of the authors to quantify connectivity.

One problem with the split-tail movement work (bottom p. 13) is that unless there is substantial variability in flow during the study years the PI's may not obtain data for the range of flow conditions naturally present. Although no PI can control this at least it should be mentioned and perhaps contingencies developed.

From the otolith depicted in Fig. 5 it appears that aging of adults will be possible as long as this is a typical specimen rather than an unusually good one. Did the PI's examine other possible structures for aging such as fin rays, or opercular bones? They might yield a clear picture.

I notice that trawling will be used as a sampling method and that specimens will be released after examination. Normally this is a sampling method that produces high mortality – do the PI's have any data on survivorship of released specimens?

Clearly the Moyle model for splittail needs to be updated given the genetic information.

I am curious why hydrologic data aren't being used to quantify connectivity of these habitats. Are there insufficient small scale hydrologic data or would they be too difficult to collect? Obviously connectivity measured by physical properties would yield a clearer picture. In addition, there are a number of hydrologic models for the Delta, yet I didn't really see these models utilized to address connectivity issues. Obviously larval fish and non-demersal eggs are at the mercy of the currents, so it would seem that information from these models would help the PI's design their studies.

Rating: **Excellent** conceptually, **good** based on the details

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:

There really aren't any major problems with the feasibility of the proposed research other than the multiple specific issues described above (e.g., statistical analyses, Sr analyses), but some aspects of this proposal read as if it were hurriedly prepared.

Rating: Ultimately the feasibility is **very good** although I suspect that the final papers will be different from what is described herein.

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: See Section 1 above.

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: Yes

Rating: **Excellent**

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:

There is no doubt that the project will yield high value products that will greatly aid in management of the Delta. Given that this is a highly dynamic system, the generality of these products will depend on the hydrologic regime of the Delta in the years that the PI's sampled, and it is a bit disappointing that there are few comments in the proposal that address this. In addition, given that past studies have documented both long and short-term variation in Delta fish assemblages, why rush into print with only two years of data (see "Schedule of Deliverables)? If years 1 and 2 are similar hydrologically and year 3 is different, it seems likely that the final papers may have different results than those from years 1 and 2. Moyle certainly doesn't need to pad his vita, although if this is being done by graduate students it is a different situation. Nevertheless, it's not great for a student's final chapter to differ from an earlier only because of trying to get work out quickly. Yes I know that the PI's are limited by the funding cycle but it would be nice to acknowledge that biology doesn't necessarily fall into discrete two or three year intervals. It would be good if either CALFED or DSC would have a separate program for long-term studies. Finally, surely they will get many more papers out of this work than the 3 specified in the Deliverables section. Three papers for 1+ million dollars is not a very good cost/benefit ratio for this type of work, especially when PI's salaries are involved.

Rating: **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:

    No duplication -- this builds on a great data base collected by Moyle, his students and collaborators. If this proposal is funded, and I think it should be (although it would be good to get a response to some of these comments) CALFED should coordinate with DSC to ensure that the proposal that the latter group has funded compliments this proposal and vice versa.

    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:

    This is a highly qualified group of PI's – I can think of no one better qualified to conduct studies of this type in the Delta.

    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:

    See comments above in Section 6 – three publications from this budget represents a low cost/benefit ratio, especially given all of the previous studies which should allow for greater synthesis. This is especially true given the high amount of money devoted to co-PI and technician salaries. Greater productivity should be expected.

    Rating: **Good**

Additional comments:

    I would really like to see this funded especially because the Delta is in such bad shape, but am conflicted by the fact that the proposal is lacking in a number of critical details.

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/Adequate**

Please provide a brief explanation of your summary rating:

This proposal is difficult to evaluate given the fact that the ideas are excellent and the need is high but there are many missing details. Conceptually I would give it a Superior but the closest ranking methodologically is Adequate.

CALFED Ecosystem Restoration Program
External Scientific Review Form

Proposal Number: 021

Proposal Title: If we build it, will they come?: Identifying habitat characteristics that support native fish in the Delta and Suisun Marsh

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:

The upper San Francisco Estuary is incompletely sampled regarding principal concepts relating flow, water quality, nutrients, and population life cycles in key habitat types – littoral zone and shoal waters. Species of concern are in decline according to the "pelagic organism decline" in open waters, yet these species may be buffered against decline if more productive and under-represented habitats serve as important production sources. The goal of the proposal is to provide 4 years of sampling in Sherman Island and Cache Slough complex regions and contrast this with a reference system, the Suisan Marsh for which the PIs maintain a long term survey program and associated database on living resource species. Further, they will compare assemblage patterns against modeled hydrology and measured water quality. *A posteriori* analyses will seek to relate flow and water quality to assemblage data. A second analysis will examine otolith microchemistry of a species of concern, Sacramento splittail: a species which traverses study habitats and other seminal regions of the upper San Francisco Estuary.

The proposal is linked to ecosystem changes related to invasive food web impacts, water regulation, and a conceptual model that relates hydrology to foodwebs and living resources. Connectivity is defined in terms of the role of littoral and other shoal habitats in overall ecosystem productivity and life history movements of splittail and other living resource species. Hypotheses were mostly exploratory in nature and some review of past analysis of the Suisan March assemblage, water quality and spatial-temporal changes would have permitted a more informed evaluation on the feasibility of hypothesis testing in this project.

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:

There is a lot of promise in this study, but the central nexus, the survey design, was completely lacking in the proposal – how many sites? How stratified? How are strata, degrees of freedom related to hypothesis testing? The proposal was heavily weighted towards background, previous work, but in fact no assemblage analysis was presented to show the feasibility of relating assemblage trends with hydrology, water quality, geomorphology, etc. The statistical analyses were appropriate for *posthoc* testing of data from a survey not designed with a purpose in mind (e.g., to test a conceptual model, or series of hypotheses). Linking essentially cross-

sectional data (assemblage/water quality metrics) to hydrology requires careful design – the presumption that emergent relations will in fact emerge is inefficient, likely leading to a cycle of over/under sampling and ambiguous results. I was also concerned about using electrofishing, which is a non-quantitative approach – i.e., difficult to standardize. Similarly trawling can be similarly difficult to standardize in terms of catchability, which is likely to co-vary with region, season, flow and other variables. It was disappointing that no text was devoted to justifying the representativeness of gear types in sampling shoal and littoral zone assemblages.

Similarly, there is no presentation of how otolith profiles will be summarized and tested against presented hypotheses. Similar issues of relating longitudinal data to classes of effects hold as above. The use of rare isotopes of Sr in tracing movements within the San Francisco Estuary is a truly innovative and important tool specific to this system, yet here again I have concerns that it is being employed in an unstructured way. For instance, why 200 specimens?

Rating: 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:

Given the long experience this group has in sustained sampling of the Suisan Marsh system, and team strengths in statistics, hydrology, and management links, field sampling and otolith chemistry applications are feasible. The principal concern is lack of experimental design in the two principal aspects of the study. The link between hypotheses and approach was not clearly established based upon presented methods and statistical analyses.

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 found the conceptual model compelling in relating residence times to wetland connectivity and water quality. The food web – native fish abundance component was a bit ambiguous, but I thought the classification scheme, drivers, local filters, and outcomes had strong promise in relating monitoring, science, and management in an adaptive framework. Further the PIs have a strong track record in this regard. The conceptual model for splittail was also well laid out. These models led to the set of hypotheses testing, but as indicated above hypothesis testing lacked feasibility due to experimental design limitations.

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:

This is a strong team and expertise is well sourced for the proposal tasks. Deliverables and assigned tasks and coordination meetings seem well matched against proposal goals.

Rating: Very good

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:

Table 1 presented a nice series of papers and other products. I particularly was impressed with past products intended for a management/policy audience which bode well for similar products for this project.

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:

The leverage of historical data on Suisan Marsh living resource monitoring is a strong element of the proposal as are the long history of some PIs in addressing San Francisco Ecosystem issues. Important publications related to long term changes in the ecosystem are relevant to the proposed work.

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 is a very strong team. Inclusion of hydrology and biostatistical expertise is particularly critical to this project.

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 inclusion of otolith microchemistry aspect was a nice element and the PIs did a nice job linking it to an apparent separate element – the survey and assemblage analysis. Including both elements however did make this an expensive proposal.

Rating: 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:

A good proposal, but unevenly prepared with too much background and insufficient linkage between hypotheses and approach. No survey or experimental design was presented. With this important omission I could not give this a higher rating despite a strong team and past record of achievement.

CALFED Ecosystem Restoration Program
External Scientific Review Form

Proposal Number: 021

Proposal Title: If they build it will they come? Identifying habitat characteristics that support native fish………

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 goals, objectives and hypotheses are clearly stated and the proposal clearly describes the ecosystem goal it is designed to address. The problem to be addressed by this research is adequately described by the proposal. This proposal intends to identify habitat characteristics that support native fish in portions of the Delta and Suisan marsh through field sampling with trawls, electrofishing, fish tagging and otolith chemistry as well modeling of the observed patterns in relation to key environmental variables that include hydrodynamic patterns. The concept is to identify patterns as a basis for establishing guidelines for habitat restoration that will benefit native species over exotics. A central focus is the connectivity among habitats believed to be especially important to native species. They propose to focus on the Cache slough and Sherman lake areas and compare the results to ongoing studies in Suisan marsh. They state that residence time, connectivity and water quality have an important effect on native fish abundance and emphasize that restoration activities of these habitats should center on consideration of these issues in the context of climate change. Establishing better habitat preference data for native species are likely to aid in those considerations.

    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 investigators adequately describe much of their approach, and as proposed, the research would contribute to the knowledge base of the system. There is a lot to like about the proposed approach especially considering the cumulative and overall experience and knowledge of the team that can interpret it in the context of what is now known. I am compelled to point out that there may be better methods than what are proposed here that could be used to learn about movement within the system, especially for splittail. Every approach has strengths and weaknesses and these choices may be a compromise in approach to suit a multispecies large scale effort. Nevertheless, the use of otolith chemistry is ok but much better resolution could be attained to track migration patterns by sonic tagging and the PIs know it works for splittail because they've collaborated on similar research. Although both approaches are feasible in this system and both are relatively expensive, sonic tagging can provide greater information at more refined spatial scales with no chance of ambiguity in the interpretation of the data. The major drawback of sonically tagging specimens is that the numbers tagged will be low and individual behaviors may over-represent population level behaviors. There may also be issues with receivers in the area of the proposed study. In contrast it is important to note that when the study areas are dominated in part or whole by high salinity, the interpretation of movement determined by otolith strontium isotope signatures will become obscured regardless of the species because of the lack of a sufficient signature differences at high salinity. Since the project focuses on the issue of connectivity the blanket statement that the otolith chemistry has been established for splittail doesn't mean it will work under all flow scenarios for tracking movements. The traditional tagging will provide complimentary movement data for a variety of species but the PIs do not describe the specific types of tagging research, the tags to be used nor do they propose any effort to establish tag retention rates and tagging mortality rates. They mention 5000 tags at $1 per tag in the budget.

   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:

   The project is feasible as proposed and the scale of the project is consistent with objectives.

   Rating: Very 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:

The conceptual model is satisfactory. The one area that always puzzles me about conceptual models in the Delta is the issue of what drives fish production in this system. Habitat is where the fish live but it does not drive secondary production per se. Primary production drives secondary production in almost all aquatic ecosystems (deep sea vents are an exception). Allochthonous inputs may be important in parts of the system but it is likely to be a distant second to PP. It seems that in the Delta all the primary production is never accounted for, especially in these shallow systems that may have some benthic algal production, epiphytes on marsh and SAV and re-suspended PP during wind events that are more significant for shallows system compared to deeper high energy channels of the main rivers and estuary. If I were looking at these shallow systems I would be looking at some of those pools of energy flow as potentially important drivers of differences between main channel and shallow system ecology.

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:

The project is research and monitoring with adequate reporting to assure that managers and other have access to data as it is developed. I am certain that if this project is funded the results will be incorporated into future considerations of restoration projects.

Rating: Very good

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:

Given the UC Davis team and Moyle's track records, I believe the results of this study will be widely available to all those that might want to use the results. Valuable outcomes are likely from components of the project if not all parts are funded. They intend to identify restoration actions that will improve these habitats for native fishes although no restoration will take place as part of this project.

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:

Moyle has many years of research on habitat use by fishes in this ecosystem and that is a great advantage in sample design and execution of the proposed studies. It also means that the proposed studies would be well integrated with previous and other ongoing efforts. This proposal starts out talking about general studies of habitat use patterns of fishes in shallow waters (splittail, Chinook, delta smelt) of the water shed but focuses much of the effort on splittail. Some of the investigators on this proposal have been involved with specific aspects of previous splittail research such as the otolith chemistry. The proposal mentions some of this splittail research but does not go into detail about how other previous or concurrent efforts on splittail research might compliment or duplicate previous efforts except to say that they will be complimentary and taken into account for this study. The awkward part of this is the distinctly different approaches being used in those studies vs those proposed here. Feyrer has tagged splittail with sonic tags along with determining that genetically distinct subpopulations of splittail exist. Moyle et al acknowledge and discuss this in this proposal but are not explicit how they will reconcile their results with the reality of two subpopulations that inter-mix to some extend at various life stages nor how their sampling and analysis of movement patterns will be reconciled with those produced by sonic tagging.

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 is a highly qualified team with the lead PI especially well versed in the research, the fish fauna and the ecology of the Bay-Delta ecosystem. Moyle knows the fishes of this system as well or perhaps better than anyone. The team's infrastructure also seems to be good for accomplishing the proposed research and monitoring.

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:

This is a large budget but reasonable for the number of PIs and the proposed effort.

Rating: Excellent

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 team has made a case for a more extensive study of the connectivity between areas of the Delta that appear to support native fishes better than exotics. Their proposed research should provide an opportunity to use the information they gather to manage the system in a way that restoration efforts and water management might be able to favor native species over exotics if a better understanding of the observed patterns can be established.