

Proposal Reviews

#79: Behavioral and Genetic Variability of Wild and Hatchery Juvenile Chinook Salmon in the Tuolumne River

H. T. Harvey & Associates, U.C. Davis

Research and Restoration Technical Panel Review

San Joaquin Regional Review

External Scientific Review #1
#2

Environmental Compliance

Budget

Research and Restoration Technical Panel Review:

CALFED Bay-Delta 2002 ERP PSP Research and Restoration Technical Panel Review Form

Proposal Number: 79

Applicant Organization: H. T. Harvey & Associates, U.C. Davis

Proposal Title: Behavioral and Genetic Variability of Wild and Hatchery Juvenile Chinook Salmon in the Tuolumne River

Review:

Please provide an overall evaluation summary rating:

Superior: outstanding in all respects;

Above Average: Quality proposal, medium or high regional value, and no significant administrative concerns;

Adequate: No serious deficiencies, no significant regional impediments, and no significant administrative concerns;

Not Recommended: Serious deficiencies, significant regional impediments or significant administrative concerns.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Superior	The coincident concerns of a regional reviewer and an external reviewer make us believe that this project would be unlikely to deliver useful information. The
-Above average	genetics work would be interesting, but it is unclear what value results from the genetics work might have in the absence of useful information on migratory behavior. Valid hatchery/wild comparisons seem ruled out for two reasons:
-Adequate	non-local stock and unpredictable stocking practices. Panel discussions suggested that the minimum size for effective application of radio tags is about 120 mm rather than the 100-120 mm range suggested in the proposal. The
XNot recommended	proposal did not present the kind of obvious background data (size ranges of juveniles at time of migration, racial origin and regularity of hatchery releases) that could possibly have alleviated the concerns expressed above.

1. **Goals and Justification.** Does the proposal present a clear statement of goals, objectives and hypotheses? Does the proposal present a clear justification and conceptual model for the project?

There is no question that there are substantial legitimate concerns regarding survival of downstream-migrating juvenile chinook in the Tuolomne River. This proposal suggests that at least some of the variability on performance (survival) may be attributable to variability in juvenile migration behavior that may have a genetic basis. Such genetically-based variation might also explain differences in performance of hatchery and wild fish.

2. **Likelihood of Success (Approach, Feasibility, Capabilities and Performance Measures).** Is the project likely to succeed based on the approach, feasibility and project team capabilities? Are

the proposed performance measures adequate for measuring the project's success?

Assessments of likelihood of success were mixed for this proposal. One external review was favorable (good), but did indicate lack of expertise wrt whether or not sizes of fish to be radio-tagged were reasonable. Other external reviews were negative (poor) and did not believe that the proposed project could deliver useful results. Particular concerns were raised regarding at least (a) small sample sizes for radio-tagging, (b) size-biased tagging due to requirement that radio-tags be applied only to largest juveniles, (c) possible behavioral and/or mortality impacts due to tagging process; and (d) unpredictable presence of hatchery fish that are not, in any event, of Tuolumne stock origin, thus frustrating any meaningful comparison between wild and hatchery fish.

3. **Outcomes and Products.** Will the project advance the state of scientific knowledge in general and/or make an important contribution to the state of knowledge of the Bay-Delta Watershed? For restoration proposals, is the project likely to contribute to ecosystem restoration or species recoveries in a significant way? Will the project produce products useful to decision-makers and scientists?

The project might produce some extremely interesting findings wrt relatedness of juveniles at different sites, but it is unclear whether such genetic findings could be related to migratory behavior and/or survival. Other project outcomes seem unlikely to produce useful products.

4. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

This project seems extremely expensive given the very small number of fish proposed for radio-tagging

5. **Regional Review.** How did the regional panel(s) rank the proposal (High, Medium, Low)? Did the regional panel(s) identify significant benefits (regional priorities, linkages with other activities, local involvement) or impediments (local constraints, conflicts with other activities, lack of local involvement) to this proposal? What were they?

The single regional reviewer of this proposal gave it a low review for reasons that coincided with one of the external reviews. Namely, concern was expressed regarding the large size (100-120 mm) required for radio-tagging, possible behavioral changes due to tagging, and uncertainty in availability of hatchery fish.

6. **Administrative Review.** Were there significant concerns about the proposal with regard to the prior performance, environmental compliance and budget administrative reviews? What were they?

No concerns were raised.

Miscellaneous comments:

Genetics studies concerning relatedness of individuals at different locations would be of interest, but it is unclear how possible differences in migratory behavior of juveniles between sites could be attributed to genetics rather than environment (where parents spawned and juveniles reared).

San Joaquin Regional Review:

Proposal Number: 79

Applicant Organization: H. T. Harvey & Associates, U.C. Davis

Proposal Title: Behavioral and Genetic Variability of Wild and Hatchery Juvenile Chinook Salmon in the Tuolumne River

Overall Ranking: Low Medium High

Provide a brief summary explanation of the committee's ranking:

The information from this proposed project would be very useful in understanding the movement of juvenile chinook and the affect of restoration projects on said movement. However the committee felt that there were too many uncertainties, such as the absence of evidence for the genetics vs. timing of movement, and a number of implementation difficulties, such as the large size that was required for test fish.

1. Is the project feasible based on local constraints?

Yes No

How?

The basis for the project are uncertain aspects of Chinook life history in this basin, such as if the spawning occurs later in one part of the river do those juveniles outmigrate later. This makes it difficult to ensure success of the project as these attributes may not exist in the chinook juveniles. Also the Chinook juveniles in the San Joaquin do not consistently wait to outmigrate until they are 100 mm and it is not clear what the data from fish greater than 100 mm would mean for the average chinook juvenile in the system. It is not known if hatchery fish will be in the river in any particular year.

2. Does the project pursue the restoration priorities applicable to the region as outlined in the PSP?

Yes No

How?

The project would provide information on an at risk species. This meets priorities 4 in the San Joaquin and 6 in the Multi-Region.

3. Is the project adequately linked with other restoration activities in the region, such as ongoing implementation projects and regional planning efforts?

Yes No

How?

If the project worked it would provide additional monitoring information for channel restoration projects in the river. There is a need for additional coordination to make the data useful for project monitoring.

4. Does the project adequately involve local people and institutions?

Yes -No

How?

Most organizations working on the river have had an opportunity to give input to the project and will have an opportunity to participate at some point.

Other Comments:

If the behavior and genetics behave in the manner proposed in this proposal that fact would be very useful to know. Restoration of salmon populations is dependent on successful outmigration and yet there are many gaps in that knowledge. The committee felt that there were temperature related issues that should have been dealt with in the proposal, since some fish were to be moving in the system early in spring and some were to be moving later.

External Scientific: #1

Research and Restoration External Scientific Review Form

Proposal Number: 79

Applicant Organization: H. T. Harvey & Associates, U.C. Davis

Proposal Title: **Behavioral and Genetic Variability of Wild and Hatchery Juvenile Chinook Salmon in the Tuolumne River**

Conflict of Interest Statements:

I have no financial interest in this proposal.

Correct
 Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

I am a researcher at UC Davis, but in a different department than Drs. Klimley and May

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent	The proposed research will monitor juvenile salmon migration in greater detail than has previously been possible, and test for genetic correlations of behavior. In addition, predation risks associated with environmental factors can be estimated, and comparisons made between wild and hatchery fish. These are excellent and useful questions.
<input checked="" type="checkbox"/> Good	
-Poor	

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

The goals of this research are to link migratory patterns and genetic structure of chinook salmon in the Tuolumne to fish mortality and to compare observations from wild fish to hatchery raised fish. The research on migration patterns and mortality, and possible underlying genetic correlations is timely and important for the recovery of endangered runs of fish.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project

justified?

Detailed knowledge of real-time, in-the-river individual fish migration is lacking - this project proposes to use state of the art electronic monitoring to get at this knowledge. The conceptual models is detailed (in Fig. 1) and clearly related to the hypotheses they propose to test. This is properly a research project. I don't see a strong link to adaptive management.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

The approaches are well designed and use tagging and electronic tracking to monitor fish movement, analyze predation, and compare behavior of wild vs hatchery raised fish coupled with genetic analysis of the tagged individuals. This project is likely to generate new insights on salmon migration behavior and the importance of genetics on this behavior. However, I don't know whether 200 fish is an adequate tagging effort, or whether the size chosen (100-120 mm) is the appropriate sized fish.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

Approaches are detailed, well documented and the molecular techniques have been used in other studies on Salmonids (including a CalFed funded proposal on Salmonid genetics awarded to one of the investigators)- which should translate into a high likelihood of success

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

Specific performance measures are not listed. In general, they will report annual reports of their progress to CalFed. A work schedule provides a few more details.

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

Products include written reports to CalFed and the Tuolumne Technical Advisory Committee, conference presentations, and scholarly publications.

7. **Capabilities.** 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?

The research team is highly qualified to perform this research. Dr. Klimley has extensive experience in electronic tracking and has published extensively; Dr. Quinn will lend his expertise in handling Salmonids; and Dr. May has already developed a suite of microsatellite for chinook Salmon in another CalFed funded research effort

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

**About 2/3 of the \$655K budget is for tagging and tracking. I don't know if this is economical.
The predation and genetic analyses seem reasonably priced.**

Miscellaneous comments:

External Scientific: #2

Research and Restoration External Scientific Review Form

Proposal Number: 79

Applicant Organization: H. T. Harvey & Associates, U.C. Davis

Proposal Title: **Behavioral and Genetic Variability of Wild and Hatchery Juvenile Chinook Salmon in the Tuolumne River**

Conflict of Interest Statements:

I have no financial interest in this proposal.

Correct

Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

none

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent	I was extremely disappointed with this project. With Tim Quinn on board, I had expected some quality science to be proposed, but I do not recommend that the submitted proposal be funded.
-Good	
XPoor	

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

Perhaps 6-8 (maybe more!) years ago I was invited to participate in a workshop on estimation of mortality to chinook salmon smolts in the Tuolumne River and I am aware that there are substantial concerns regarding predation in previously "mined" areas and that there has been ongoing consideration of constructing a hatchery directly on the Tuolumne to aid in restoration of these fish. Given this background, I suppose that the objectives of this research (to learn about genetic and behavioral variation in wild Tuolumne fish and to compare them with hatchery fish) may be of interest. I do not believe that the genetic hypotheses presented on page 3 are of immediate importance for restoration, however.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

The proposal provides no arguments in support of the contention that variable performance of juveniles from different study reaches might reflect genetically inherited traits at the level of "family" (progeny of individual female) and race (fall vs late fall). Even if the study were to discover greater relatedness of individuals within reaches than between and between reach differences in migratory behavior, it seems to me that such differences could in large part be a reflection of environment. Only if female parents with substantially different genetic makeup spawned in the same location and had different migratory behavior would it be possible to generate the kinds of inferences implied in this proposal.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

As developed, I believe that this proposal has numerous flaws that diminish its potential usefulness. First, as noted above, I believe that one could argue that migratory behaviors of juveniles may in large part reflect their rearing environments. If this (alternative) hypothesis (not mentioned) were true, then one might expect very similar outcomes as those expected under their apparent "null" hypotheses presented on page 3. Second, unless things have changed recently the hatchery fish that are released in the Tuolumne are from Merced River fish facility, the smallest propagation facility operated by CDFG in the CV. Thus, these fish are not currently native to the Tuolumne and may not even be closely related to them. Thus, a comparison of "wild fish" with "hatchery fish" is confounded in a manner similar to the confounding between genetics and reach identified above. Also, because the Merced facility is small, genetic diversity among Merced hatchery fish is likely much smaller than at the more typically large-scale CV salmon hatcheries such as Nimbus, Coleman, etc. Third, the device of relying primarily upon following radio-tagged fish means that sample sizes are extremely small - just 40 fish tagged in each of 4 areas for a total of 160 radio tags. Fourth, the authors state that the radio tags require that fish are in the range of 100-120 mm fork length. I cannot believe that average-sized chinook juveniles reach such a size prior to outmigration in March-May. If only the larger fish are radio-tagged, then clearly inferences regarding behavior will be seriously biased.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

Based on reasons elaborated above in the Approach section, I believe that it would be unlikely for this project to deliver results that would provide support for or against the hypotheses presented in this proposal. I'm sure that with Tom Quinn helping out that there would be few problems wrt collecting data on radio-tagged fish. The problem would be interpreting collected data. For example, fish are to be tagged just before outmigration takes place. These tags have small "antennas" that trail behind the fish. Does this alter behavior of such small fish? Does it cause mortality due to predators to be greater than what it might be for a fish that does not have such implanted tags. Etc., etc..

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

no comment on this topic.

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

The tasks identified under genetic studies might produce some extremely interesting data regarding relatedness of juveniles at different sites. I have no idea how one might use such information for future restoration, etc., however. It seems of more basic scholarly interest. The numbers or radio-tagged fish seem much too small to provide much of value concerning predation

7. **Capabilities.** 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?

Klimley appears to have had extensive experience with tagging of white sharks, but it is not so clear to me that his experience translates well to working with juvenile salmonids. Tom Quinn (subcontracted from UW) is a world-class fishery scientist and a recognized expert on migration of salmonids, but his role in this project seems very modest. Frankly, the submitted proposal makes me think that Klimley has not done his homework on juvenile chinook. For a radio-tagging study of juveniles, it would be important to establish that essentially all juveniles exceed the minimum size required for tagging prior so that sizes of fish could be "random" or stratified to ensure that all sizes of fish were tagged. Such an obvious and critical requirement is not even discussed in this proposal.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

I think that this project is extremely expensive given the very small numbers of fish that would be radio-tagged.

Miscellaneous comments:

Environmental Compliance:

Proposal Number: 79

Applicant Organization: H. T. Harvey & Associates, U.C. Davis

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1. Are the legal or regulatory issues that affect the proposal identified adequately in the proposal?

-Yes No

If no, please explain:

The Tuolumne River is considered critical habitat for steelhead. ESA compliance and consultation with USFWS and NMFS is required for incidental take of steelhead.

2. Does the project's timeline and budget reflect adequate planning to address legal and regulatory issues that affect the proposal?

-Yes No

If no, please explain:

No time or money are allocated for compliance with ESA.

3. Do the legal and regulatory issues that affect the proposal significantly impair the project's feasibility?

Yes -No

If yes, please explain:

They will need to budget for ESA compliance.

Other Comments:

Budget:

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Proposal Title: Behavioral and Genetic Variability of Wild and Hatchery Juvenile Chinook Salmon in the Tuolumne River

1. Does the proposal include a detailed budget for each year of requested support?

Yes No

If no, please explain:

2. Does the proposal include a detailed budget for each task identified?

Yes No

If no, please explain:

3. Does the proposal clearly state the type of expenses encompassed in indirect rates or overhead costs?

Yes No

If no, please explain:

4. Are appropriate project management costs clearly identified?

Yes No

If no, please explain:

5. Do the total funds requested (Form I, Question 17A) equal the combined total annual costs in the budget summary?

Yes No

If no, please explain (for example, are costs to be reimbursed by cost share funds included in the budget summary).

There is a dollar difference, which appears to be a carrying forward error.

6. Does the budget justification adequately explain major expenses?

Yes No

If no, please explain:

7. Are there other budget issues that warrant consideration?

-Yes No

If yes, please explain:

Other Comments: