Independent Scientific Review Panel
for the Northwest Power Planning Council
851 SW 6th Avenue, Suite 1100
Portland, Oregon 97204
isrp@nwppc.org

Final Review of Proposals Submitted in Response to Bonneville Power Administration's March 14, 2003 Request for Studies for Reasonable and Prudent Alternative Actions 182 and 184 of the 2000 Federal Columbia River Power System Biological Opinion

(Final ISRP Review of RFS Proposals and Responses)

Chuck Coutant
Susan Hanna
Nancy Huntly
William Liss
Lyman McDonald
Brian Riddell
William Smoker
Richard N. Williams

Jack Griffith, PRG
Eric Loudenslager, ISAB-PRG

ISRP 2003-9
June 27, 2003

Final ISRP Review of RFS Proposals and Responses

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Introduction

This report contains the ISRP's final recommendations and comments on 15 proposals submitted in response to Bonneville Power Administration's March 14, 2003 Request for Studies (RFS) intended to address research, monitoring, and evaluation requirements under Reasonable and Prudent Alternative (RPA) Actions 182 and 184 of the NOAA Fisheries' 2000 Biological Opinion (BiOp) on the Operation of the Federal Columbia River Power System (FCRPS). The Request for Studies (RFS) was needed to fill research needs for the BiOp that are not actively being addressed through the Bonneville Power Administration's current implementation program. On April 25, 2003, the ISRP issued a presumably final review of the 15 proposals (ISRP 2003-7), but suggested that a response review be provided for a number of reasons, foremost of which were to ensure that the proposals adequately addressed the gaps identified for the BiOp and to provide the same opportunity for a response loop as was provided for proposals submitted in the Mainstem/Systemwide project selection process. Given the overall quality of the responses, the ISRP feels this was a useful exercise that should provide BPA and the Council more confidence in selecting proposals to meet the RPA Action items.

The RFS covered the following three topics relevant to the Reasonable and Prudent Alternative Actions:

1) **Action #184**: Synthesis of Existing Analytical Approaches, or Development of a New Analytical Approach, for Determining the Effects of Hatchery Reforms on Extinction Risk and Recovery (2 proposals received);
2) **Action#184**: Reproductive Success of Natural-Origin, Hatchery-Origin, and Reconditioned Kelt Steelhead (4 proposals);
3) **Action #182**: Studies to Determine Reproductive Success of Hatchery Spawners (9 proposals).

A majority of ISRP members and two Peer Review Group members participated in the initial review and the response review, shared comments, discussed the proposals and responses in detail, and reached the recommendations in the report by consensus. As with all ISRP proposal reviews, proposals were reviewed in the context of the Council's program and in regard to whether they:

1. are based on sound science principles;
2. benefit fish and wildlife;
3. have clearly defined objectives and outcomes; and
4. have provisions for monitoring and evaluation of results.

In addition to the standard ISRP criteria, the ISRP reviewed each proposal according to the specific criteria and questions provided in the RFS. In this report, ISRP final recommendations and comments are provided for each proposal and grouped by the three RFSs. General comments on the three sets of proposals are provided at the beginning of each RFS section. Preliminary comments for each of the proposals, which follow the ISRP final recommendations and comments, are left in the report for ready reference to the issues that were addressed (or not addressed) in the response review.

We look forward to a continued role in this process and joining the Independent Scientific Advisory Board in a likely review of the RME Framework document. This regional effort to establish a scientifically sound, cost-effective, and long-term RME plan for the Columbia River Basin is an endeavor of paramount importance.

FCRPS BIOP Action #184: Synthesis of Existing Analytical Approaches, or Development of a New Analytical Approach, for Determining the Effects of Hatchery Reforms on Extinction Risk and Recovery

This RFS solicited proposals from qualified individuals or groups to develop a standardized analytical approach for synthesizing the results and detecting the effects of a myriad of hatchery reforms. The synthesis was to be on extinction risk and recovery at the population and Evolutionarily Significant Unit (ESU) levels, consistent with the purposes of Action 184. Specifically the solicitation calls for studies to determine the efficacy of hatchery reforms in reducing extinction risks and/or contributing to recovery goals. RPA Action 184 calls for research to assess: (1) the efficacy of hatchery reforms in reducing extinction risk, and (2) the efficacy of conservation hatchery activities in contributing to recovery. This RFS is focused on the first of these two topics. The cost for a 9-month project to fulfill this RFS was estimated to be from $100,000 to $150,000. Two proposals were received in response to this RFS.

The ISRP was surprised at the content and approach of these two proposals, and that there were only two proposals received. We wonder if the short timeline on the request worked against both broader response and more in-depth attempts to synthesize data at large scales. The RFS emphasized the latter point, yet neither proposal adequately addressed the issue of large-scale synthetic analytical tools and approaches. However, the problem appears to be that what truly is needed is data. The ISRP pointed out the potential lack of data in our preliminary review of the requests for studies and both proposals confirm our evaluation. We can recommend proposal #10 for development of an evaluation model based on expert opinion (specific review comments included below) as a stopgap measure in the short term. In the long term, only collection of better monitoring data as proposed in some of the other proposals in this solicitation, and as will be identified in the course of development of an expert system, will allow detection of the effects at the population and ESU levels of a myriad of hatchery reforms on extinction risk and/or recovery.

Proposal 10: A Tool for Evaluating Risks and Benefits of Reform Actions in Hatchery Programs (WDFW)

ISRP Final Recommendation:
Fundable at low priority.

The response provides reasoned answers to each question and good justification for the use of expert opinion as a short-term step in the risk analysis, although it is weak in describing the process of generating expert opinion in a way that would ensure consistency and usefulness. The proposed effort is useful as a stopgap measure until data are available, but the generation of “best guess” information from sources that have many potential biases is a far cry from the collection of actual data.

The response acknowledges that expert opinion is a second best alternative to having good data, but argues that in the absence of data, expert opinion is useful. The ISRP agrees that a model based on expert opinion will be useful. Further, a secondary benefit will be further identification of significant data gaps that limit the ability to meet the full intent of FCRPS BIOP Action #184: Synthesis of Existing Analytical Approaches, or Development of a New Analytical Approach, for Determining the Effects of Hatchery Reforms on Extinction Risk and Recovery.

The use of expert opinion as an approach to generating synthesized results is a matter of process, but the response does not provide specifics about that process. The authors assert that variability in expert opinion can be used as a proxy for parameter variability. This seems a tenuous assumption when considering the factors that contribute to variation in expert opinion: range of knowledge, context-specific information, vested interest, and selection bias. The response recognizes these sources of variation but says only that the solution is to guard against them where possible. How would this be done? The response does not thoroughly describe a process by which a group will develop probabilities of certain hazardous events, or quantify the magnitude of the impact (cost) of those events. The response refers to methods available to assess uncertainty in responses but does not elaborate on them. Additionally, the response does not indicate how, specifically, the use of expert opinion would be transparent.

Reviewers were surprised that the sponsors did not have suggestions on how to further test their proposed risk consensus model. The model could be used in a sensitivity analysis of a set of predictions, these could be the basis for new research or to find studies (of which there are several in this package) that could be used in these tests. If the consensus model is a short-term process, then the authors should be able to suggest means to develop the longer-term procedures. The recommendation to evaluate the quality of the consensus model is discounted as impractical.

Regarding the response to question 3 (continuous up-dating of information), the question posed by the ISRP and their response are both reasonable, everyone is acknowledging that we currently do not have adequate data for more quantitative models, so this is another process and we need to ensure that funding agencies and decision makers understand the dynamic nature of this work over the next 10+ years. Their adoption of "broader view of risk" as supported by the NRC (1996) report is good; this is a very readable and informative report. However, the response downplays the likelihood that new information will be different enough to change the basic conclusions of the initial expert opinion exercise. The ISRP continues to believe that usefulness of an expert opinion model will require periodic updates as new data are generated. Intuition of experts tends to change immediately upon learning new facts.

A point for clarification, Ken Currens is identified as a Co-PI but is not listed in the budget.

ISRP Preliminary Recommendation:
Fundable at low priority subject to addressing minor criticisms below.

We can recommend proposal #10 for development of an evaluation model based on expert opinion as a stopgap measure in the short term. In the long run, only collection of better monitoring data as proposed in some of the other proposals to satisfy RPA Actions #184 and #182, and as will be identified in the course of development of an expert system, will allow detection of the effects at the population and ESU levels of a myriad of hatchery reforms in terms of their effects on extinction risk and/or recovery. Overall, the proposal is weak in directly addressing needs described in the RFS.

Direct answers to RFS questions.

· *Would the study result in the development of a standardized analytical approach for synthesizing the results and detecting the effects at the population and ESU levels of a myriad of hatchery reforms in terms of their effects on extinction risk and/or recovery?*

The proposed approach is standardized but derived data will be mainly qualitative (subjectively determined by expert opinion) and in this sense, not strictly quantitative and analytical. The ability to apply the approach to a given hatchery and hence compare the different hatchery reforms in terms of

effects on extinction risk and/or recovery at the population or ESU levels will vary with the data available. Fewer data means more reliance on expert opinion.

· *Will the study provide documentation and/or explanatory text for the analytical approach sufficient to allow other entities to readily use it to evaluate potential effectiveness of hatchery reform measures?*

Yes and no. Others will be able to use the method, but use will depend on the availability of data and experts. It is likely that the work would provide documentation and explanatory text sufficient to support use by others, as the main players in the proposed effort have a track record of making information and approaches available to others. However, the outcome will be an expert system subject to continuous restructuring as new empirical information comes available or it would simply freeze today's admittedly inadequate information as to outcomes of new approaches (reform). This would seem to make the project continuing, rather than a nine-month project with a clear useful end-product. The funding agency should carefully consider whether or not they wish to start what may turn out to be a 10-12 year project.

Primary Review Comments and Questions for Improvement of the Proposal

The authors recognize that, too few data are available to provide case-specific assessments. They propose an expert system approach that can use whatever data exist from ongoing processes, such as HGMPs, the APRE databases, and EDT analyses, to qualitatively assess risks and benefits. We agree that an expert system such as proposed is probably the only way to provide qualitative risk assessments of hatchery practices. The alternative is to wait 10-12 years for the completion of rigorous studies on the reproductive success of hatchery and wild fish to obtain more quantitative assessments.

The proposed approach is founded on the principles of probabilistic risk assessment and attempts to estimate risk as a function of the subjective probability that different events might occur and their possible consequences. The principal investigators propose to use "expert opinion" to derive lists of hazards, weighting factors for hazards, variables contributing to risk, and distributions of potential consequences. However, the proposal continues to use the term "risk" in other ways. To further confuse the terminology, it uses "hazards" interchangeably with "risks". It refers to "risk-benefit" analysis without explicitly addressing benefits. These issues and definitions should be addressed and made clear before the project is funded.

It is an act of faith to assume that a consensus model based on expert opinion will in fact be a good model. The proposal should include methods for evaluating the quality of the resulting consensus model before funding. An itemized budget should be provided.

Proposal 13: Analytical Approach for Determination of Effects of Hatchery Reform on Extinction Risk and Recovery of Salmon and Steelhead (CRITFC)

ISRP Final Recommendation: Not Fundable. A response was not requested. Unfortunately, the region lacks the data needed to successfully implement this project. The work would seem to require information that could only be gathered with a longer-term and larger-scale study than is described or that fits the 9-month timeframe specified in the RFS.

Direct answers to RFS questions.

· *Would the study result in the development of a standardized analytical approach for synthesizing the results and detecting the effects at the population and ESU levels of a myriad of hatchery reforms in terms of their effects on extinction risk and/or recovery?*

Unfortunately no, because of the region lacks adequate data.

· *Will the study provide documentation and/or explanatory text for the analytical approach sufficient to allow other entities to readily use it to evaluate potential effectiveness of hatchery reform measures?*

Unfortunately no, because the region lacks adequate data.

Primary Review Comments and Questions for Improvement of the Proposal

The proposal should have a more clear and definite statement of tasks and methods to accomplish the tasks and objectives.

This proposal would take a synthetic approach to assessing the effect of hatchery reforms on risks for wild salmonids. It provides an extensive literature review of the issues related to reproductive success, artificial propagation, conservation goals, risks of hatchery operations, and biological impairment from hatchery wild interbreeding. The idea is to link hatchery practices with variation in traits that determine fitness. The idea of developing a statistical framework to assess these linkages and to use this framework to guide hatchery practices seems like a good one. Unfortunately, empirical work and data would need to be greatly expanded to relate the proposed genetic approach to indexing outcomes of hatchery practices to an assessment of risk at the population or ESA level.

The proposal fails to provide convincing linkage of the small-scale and local measures that will be taken to population- or ESA-level demographic responses. The proposal assumes that simple gene expression, as indexed with arbitrary microarray analyses, will translate simply into population-level fitness and thus can be used to assess risk resulting from hatchery protocols. This is an untested assertion and cannot be assumed to be correct. The proposal needs a strong way to test this assumption.

The short section on their statistical model was unclear on what dependent variables are to be modeled, what independent variables are to be included, and where the data will come from. Examples of the computation conducted on an example using real data (or hypothetical data) would have been helpful.

FCRPS BiOp RPA Action#184: Reproductive Success of Natural-Origin, Hatchery-Origin, and Reconditioned Kelt Steelhead

Background
This RFS solicited a study of the relative reproductive success of reconditioned steelhead kelts to provide information critical for evaluating the potential benefits of enhancing iteroparity. Specifically, the study should determine the relative reproductive success of reconditioned steelhead kelts spawning in the wild compared to natural-origin adults, hatchery-origin adults, and cross matings of these three variants, in one or more populations. The study should be directly applicable to one or more of the following listed ESUs: Upper Columbia, Mid-Columbia, and Snake River steelhead. The cost for a study at the general desired level of effort was estimated at $400,000 to $650,000 per year.

ISRP General Comments after Preliminary Review
In their initial presentation, none of the four proposals adequately addressed all the questions posed in the RFS. With regard to reproductive success of kelts, reviewers and proposers seem concerned about the genetic fitness issues associated with re-introducing the iteroparous life history trait back into the natural populations.

The ISRP felt there were three aspects of kelt reconditioning that merit study:

1) The physiological reconditioning of the fish and the quality of the gametes produced (egg number, size, quality) and the timing of maturity (is it coincident with the wild?) …i.e., the phenotypic expression of maturity

2) Whether there are genotype x environment interactions in the reconditioning of kelts that may lead to a non-random contribution of genetic material in the next generation?

3) What are the longer term genetic effects of re-introducing repeat spawning into depressed populations … is the demographic boost balanced by changes in the genetically effective population size?

With respect to assessing fitness effects, there are three points to consider further:

1. Measuring immediate spawning success is not a sufficient assessment of reproductive fitness of kelts.

2. The assessment of reproductive fitness and genetic effects of this life history trait is inherently long term and will not be answered in the next 3-5 years. It will require much longer commitments and funding to answer this question.

3. At present, we may not have adequate genetic tools to study the genetic basis of this trait.

An assessment of the genetic risk associated with kelt reconditioning could be generated by monitoring the pedigree of steelhead that become successfully reconditioned spawners, and whether they represent a subset of the lineages in a population or are a random sample of the population (a test of point 2). But other than a genetic basis for differential success of certain kelts, what other genetic concerns would there be? Remember that a kelt has already been successful to the extent that

it survived to maturity and spawned, most are female so that successful mating is very likely (assuming that the timing is correct and eggs are viable), and spawning likely occurs after 1 to 2 years of conditioning and should avoid mating with immediate siblings etc.

Proposal 4: Reproductive Success of Natural-Origin, Endemic Hatchery Origin, and Reconditioned Kelt Summer Steelhead in the Tucannon River (WDFW)

ESU: Snake River Steelhead

ISRP Final Recommendation:

Fundable for the limited scope the proposal offers, but it does not address all the criteria laid out in the RFS. The proposal is asking a very narrow question and if the question is important it may be more efficient for another broader proposal to address the question within its design and implementation than to separately fund this project. Consequently, this is the lowest priority of the three "fundable" kelt proposals.

The sponsor's reply concerning the smolt trap was reasonable, and their comments on costs for DNA analysis are worth keeping in mind. The remainder of their comments to questions were reasonable, including our questions concerning measurements in F_2 and F_3 generations and use of controls. The appropriate measure of reproductive success should be within their natal environment and competing with these local fish. The use of controls is more associated with longer-term studies and they are investigating the use of Asotin Creek.

ISRP Preliminary Recommendation:

Fundable at medium priority, contingent on adequate response to unaddressed RFS questions and ISRP concerns. Many of the RFS questions were not addressed. Priority for the project would be higher if a smolt trap was available for this project, and evidence existed that the process of reconditioning kelts had been successfully undertaken.

ISRP Review Comments:

The Tucannon situation is directly applicable to a listed ESU and presents an important opportunity for satisfying the requirements of the RFP even given the difficult sampling problem of finding offspring of wild fish with which to estimate reproductive success. The project geneticist is well qualified. However, the questions about genetic consequences in the RFP aren't addressed in the proposal. The proposal does not describe a plan to answer the questions or address the potential long-term genetic risk of kelt reconditioning. A smolt trap would be a better approach, although they are already likely getting some of their sampling with ongoing monitoring via electrofishing.

The proposal is not thoroughly written, is a bit disorganized, and seems to place little emphasis on examining possible risks of kelt reconditioning. "After spawning, fish will be PIT tagged for unique identification and transported to a 20' circular rearing tank at LFH, and the reconditioning process will begin." Shouldn't the fish be marked and identified earlier so that fitness of their offspring reared in the hatchery can be compare to the offspring produced from the reconditioned adults? Maybe they are doing this but it is not clear. Will each individual female be incubated separately at LFH the first year so that results can be compared to results after reconditioning.

Are tissue samples needed from both the juveniles (Task 2b) and smolts (Task 2c- costs are not included because the trap is proposed in another study and may or may not be funded) if the smolt trap is funded. Which is best?

"Task 2d. Collect DNA tissues from all natural origin returning adults at the TFH adult trap from study year fish to conduct pedigree analysis from adult to adult." Shouldn't they also collect DNA tissues from hatchery produced adults that are allowed to spawn naturally?

Following fitness of the various crosses to F2 and F3 is not mentioned. Are control streams needed to follow fitness over time without hatchery and reconditioned steelhead?

RFS Review Criteria:

- *Will the study determine the relative reproductive success of reconditioned steelhead kelts spawning in the wild compared to natural-origin adults, hatchery-origin adults, and cross matings of these three variants, in one or more populations?*

Yes, in the Tucannon River population of steelhead, if successful in each of several components—reconditioning kelts, sampling offspring successfully (will a representative sample of natural-origin parr be obtained from eight 'Index Sites' in the Tucannon R? How will additional sites be chosen? The proposal is a bit vague. For example, on whether or not funds have been included for DNA tissue analysis from the parents of the hatchery origin adults – or, is this a good idea?

- *Does the proposal employ the use of microsatellite DNA analysis in order to ascertain the pedigree of resulting progeny and subsequent returning adult steelhead. If not does the method proposed provide quantification of reproductive success of equal of better power than microsatellite DNA analysis?*

Yes

- *Does the study include analysis of the potential genetic consequences of repeat-spawning steelhead on small populations?*

Not included in this proposal.

Other research topics, which should be addressed in the proposed study if possible, include:

- *How increasing iteroparity might increase inbreeding in the target population, particularly if it is small?*

Not in the proposal.

- *How reconditioning kelts might increase domestication selection in the target population?*

Not addressed well in the proposal.

- *How the reconditioning program might alter age structure and life history structure in the target population.*

Not addressed well in the proposal.

- *Does the research site(s) offer the ability to capture and sample sufficient outmigrating offspring and all, or nearly all, returning adult steelhead.*

Yes for adults, but not for outmigrating smolts unless the smolt trap is funded in a chinook study. Electrofishing for juveniles might not give adequate sample size and might not be completed. Smolt monitoring might be less than ideal unless a new trap is installed.

- *Is the proposed study directly applicable to one or more of the following listed ESUs: Upper Columbia, Mid-Columbia, and Snake River steelhead.*

Yes.

- *Cost-effectiveness (e.g., the ability to take advantage of existing fish production, research, monitoring or evaluation activities) will be an important consideration in the proposal selection process.*

Potentially highly efficient by adding marginal effort to other projects. Budget seems sketchy but suggests large effort devoted here. ~$140K (only adult sampling and without juvenile sampling) and ~$260K (with juvenile sampling) seems reasonable. Would the $140K include funds for sampling smolts if the smolt trap is approved?

Proposal 5: Assessment of the Reproductive Success of Reconditioned Kelt Steelhead with DNA Microarray Technology (Battelle)

ESU: Mid-Columbia River Steelhead
ISRP Final Recommendation:
Do not fund. A response was not requested. This is not tied directly enough to the Kelt RFS.

The ISRP notes however that this is a very well written and technically qualified proposal that may produce an important technique (microarray analysis) in the genetic studies of steelhead and other fishes. The proposal did outline an interaction with the kelt reconditioning study in the Yakima basin, included a proof of principle of this technique by comparing it directly with DNA micro-satellite studies, and a goal to transfer this technology to regional laboratories. New tools that could allow researchers to more finely resolve genetic variation between individuals and populations will continue to be important, but this tool may not be necessary or available in the timeframe to address these RPA issues associated with reproductive success.

RFS Review Criteria:

- *Will the study determine the relative reproductive success of reconditioned steelhead kelts spawning in the wild compared to natural-origin adults, hatchery-origin adults, and cross matings of these three variants, in one or more populations?*

Yes, but measurement of the fitness of the various groups is not discussed in detail.

- *Does the proposal employ the use of microsatellite DNA analysis in order to ascertain the pedigree of resulting progeny and subsequent returning adult steelhead. If not does the method proposed provide quantification of reproductive success of equal of better power than microsatellite DNA analysis?*

They propose to further develop and test DNA Microarray Technology. Are these the same? Is the DNA microarray technology needed?

- *Does the study include analysis of the potential genetic consequences of repeat-spawning steelhead on small populations?*

Not explicitly, although perhaps implied by cooperation with the Yakima Fisheries Projects.

Other research topics, which should be addressed in the proposed study if possible, include:

- *How increasing iteroparity might increase inbreeding in the target population, particularly if it is small?*

  Not explicitly, although perhaps implied by cooperation with the Yakima Fisheries Projects.

- *How reconditioning kelts might increase domestication selection in the target population?*

  Not explicitly, although perhaps implied by cooperation with the Yakima Fisheries Projects.

- *How the reconditioning program might alter age structure and life history structure in the target population.*

  Not explicitly, although perhaps implied by cooperation with the Yakima Fisheries Projects.

- *Does the research site(s) offer the ability to capture and sample sufficient outmigrating offspring and all, or nearly all, returning adult steelhead.*

Not explicitly, although perhaps implied by cooperation with the Yakima Fisheries Projects.

- *Is the proposed study directly applicable to one or more of the following listed ESUs: Upper Columbia, Mid-Columbia, and Snake River steelhead.*

Marginally.

Cost-effectiveness (e.g., the ability to take advantage of existing fish production, research, monitoring or evaluation activities) will be an important consideration in the proposal selection process.
No. At $462k/year over 4 years or $1.848 million total it is on the top end.

Proposal 9: An Evaluation of the Efficacy of Steelhead Kelt Reconditioning to Address Biological Opinion Action 184b: The Reproductive Success of Hatchery-Origin and Wild-Origin Repeat Spawners (USGS)

ESU: Lower Columbia River Steelhead
ISRP Final Recommendation:
Fundable at medium priority. The justification for this research is compelling, even if not for an RFS-specified ESU. The response was thorough and well thought out.

This is a good, thorough response to the ISRP questions. Question 1 refers again to following fitness in F_2 and F_3 generations and use of controls. In terms of the immediate value of the work, the F_1 is the critical first step (we agree with the project sponsors) and then fitness can be tracked the over time. At this longer timeframe, the use of controls does become appropriate as the temporal effects become more important to the evaluation. However in the short-term, the important question is the effectiveness of repeat spawners and their reproductive success in the open natural environment.

Their reply on pooling milt is incorrect. There have been two detailed studies comparing pooling milt and spawning females with individual males. The motility of sperm is variable and pooling milt generally results in a couple of males fertilizing all females … the opposite of what the proposers seem to be saying. The performance of males varied between groups that were pooled. To conduct the study on female re-conditioning, the proposers should conduct matrix spawning of females with multiple males … but use the same males over several females. Pooling males does not protect against "differences in sperm motility or viability of individual males".

The response to the assessment of low priority due to lack of direct application to an ESU is persuasive in arguing that this research would generate information about how processes of reconditioning work, and that this process information would generalize to all ESUs. The question seems to be on the interpretation of "directly applicability" to the named ESU's: does it mean only projects occurring in an ESU or does it allow scope (as does RPA 182) for studies not occurring in an ESU but having clear applicability? The response provides reasonable justifications for the laboratory work and for the benefits of the natural conditions of Kalama River steelhead, particularly in improving the power of statistical tests.

Regarding the second point presented by H/H group, we strongly disagree with their point, and agree with the proposal authors.

ISRP Preliminary Recommendation:
Qualified fundable for phase 1, contingent on adequate response to unaddressed RFS questions and ISRP concerns. This proposal is of the lowest priority of the three reconditioning (#4, #9, and #14) proposals because it does not have as direct application to the ESUs.

A strength of this proposal is that it looks at the early lifestage of the steelhead. They are proposing in Phase I to see if kelts can be reconditioned and produce offspring approximately equal to a virgin spawner in a controlled environment. Phase II will only be conducted if Phase I is successful. Methods for design or analysis of Phase II are not given. The very brief discussion of work to potentially follow Phase I and II may be the most unique part of the study, namely comparison of the reproductive viability of gametes and progeny from reconditioned spawners (i.e., those retained in a hatchery environment) relative to naturally returning repeat spawners. Reviewers also assessed a lower rank to this investigation as it may not constitute an adequate test of gamete viability when tested only within the controlled environments. Reproductive success may be as much a function of spawning behavior, synchrony of gamete development, and local stresses, which cannot be naturally controlled in the laboratory environment.

Following fitness of the various crosses to F2 and F3 is not mentioned. Are control streams needed to follow fitness over time without hatchery and reconditioned steelhead?

Why pool milt—is intent to examine variability of potency? H s W?

RFS Review Criteria:

- *Will the study determine the relative reproductive success of reconditioned steelhead kelts spawning in the wild compared to natural-origin adults, hatchery-origin adults, and cross matings of these three variants, in one or more populations?*

Yes, if Phase I to recondition kelts seems to be working. Directly studies components of fertility of reconditioned kelts; studies reproductive success as well at maturity of offspring.

- *Does the proposal employ the use of microsatellite DNA analysis in order to ascertain the pedigree of resulting progeny and subsequent returning adult steelhead. If not does the method proposed provide quantification of reproductive success of equal of better power than microsatellite DNA analysis?*

Yes.

- *Does the study include analysis of the potential genetic consequences of repeat-spawning steelhead on small populations?*

No.

Other research topics, which should be addressed in the proposed study if possible, include:

- *How increasing iteroparity might increase inbreeding in the target population, particularly if it is small?*

No. It describes effect qualitatively, doesn't propose to study immediately.

- *How reconditioning kelts might increase domestication selection in the target population? And*
- *How the reconditioning program might alter age structure and life history structure in the target population.*

No.

- *Does the research site(s) offer the ability to capture and sample sufficient outmigrating offspring and all, or nearly all, returning adult steelhead.*

Yes, apparently.

- *Is the proposed study directly applicable to one or more of the following listed ESUs: Upper Columbia, Mid-Columbia, and Snake River steelhead.*

No

- *Cost-effectiveness (e.g., the ability to take advantage of existing fish production, research, monitoring or evaluation activities) will be an important consideration in the proposal selection process.*

Yes, at ~$160 to $300 the budget is in line with others.

Proposal 14: Proposal to Evaluate Reproductive Success of Natural-Origin, Hatchery-Origin, and Kelt Steelhead in the Columbia River Basin (CRITFC)

ESU: Multiple candidate ESUs

ISRP Final Recommendation:

Fundable, highest priority of the set.

This is a thorough response that addresses both the general and specific ISRP review comments. A revised sampling scheme is presented that makes explicit the methods, time line, and numbers of fish sampled. The work is both scaled back (as recommended) and "expanded" in the sense of adding temporal and spatial controls. The proposers have specified sites, as requested, and these sites seem appropriate. They seem to know what they are doing, and have thought through the complexities of enhancing second spawning after reconditioning in a hatchery environment. The ESUs are relevant. Controls have been introduced.

CRITFC obviously considered the ISRP comments thoroughly and responded in kind demonstrating a more in-depth consideration of kelt reconditioning and our past discussions.

ISRP Preliminary Recommendation:

Fundable, contingent on adequate response to unaddressed RFS questions and ISRP concerns. This proposal received the highest ranking of the four proposals submitted for the RFS because it offers the most comprehensive application to the priority ESUs listed in the RFS. However, the proposal needs to be scaled back.

They propose to apply kelt reconditioning methods developed by project 2000-017 with geographic replication for evaluation of fitness of various crosses across all ESUs of interest in the Request for Studies. Replication over space is highly desirable, however it is unclear if they are replicating each stream in time. They mention following the 2004 spawning class for reproductive success, but what about 2005 and beyond. Resident trout are included, a study component mentioned in many of the proposals.

The theoretical risk associated with the kelt reconditioning projects is of increasing inbreeding by using the reconditioned kelts in a small population. The implicit argument is a demographic boost is needed to a greater extent than the potential risk of inbreeding depression. The CRITFC kelt proposals seem be overly optimistic about the potential benefits of kelt reconditioning. The streams chosen are primarily upstream of many dams and will likely need to be continually propped up as the kelts will likely not naturally be successful given the migratory conditions. This proposal would start a fairly aggressive program before the benefits and risks are determined. The proposal seems to place little emphasis on examining possible risks of kelt reconditioning. Consequently, we recommend that the number of streams need to be scaled-back to less than five to reflect the fact that this a research project. This seems to be an effort to drastically expand the implementation of kelt reconditioning, without first evaluating the possible undesirable consequences, as called for in the RFP.

Following fitness of the various crosses to F2 and F3 is not mentioned. Are control streams needed to follow fitness over time without hatchery and reconditioned steelhead? The study should pay more attention to gamete quality. Where are the controls in the study?

RFS Review Criteria:

- *Will the study determine the relative reproductive success of reconditioned steelhead kelts spawning in the wild compared to natural-origin adults, hatchery-origin adults, and cross matings of these three variants, in one or more populations?*

Yes, with replications. Cross matings are not mentioned, but we assume they would be studied.

- *Does the proposal employ the use of microsatellite DNA analysis in order to ascertain the pedigree of resulting progeny and subsequent returning adult steelhead. If not does the method proposed provide quantification of reproductive success of equal of better power than microsatellite DNA analysis?*

Yes.

- *Does the study include analysis of the potential genetic consequences of repeat-spawning steelhead on small populations?*

No. Not specifically addressed.

Other research topics, which should be addressed in the proposed study if possible, include:

- *How increasing iteroparity might increase inbreeding in the target population, particularly if it is small?*

Not specifically addressed.

- *How reconditioning kelts might increase domestication selection in the target population? And*

Not specifically addressed.

- *How the reconditioning program might alter age structure and life history structure in the target population.*

Not specifically addressed.

- *Does the research site(s) offer the ability to capture and sample sufficient outmigrating offspring and all, or nearly all, returning adult steelhead.*

This is a criterion for site selection.

- *Is the proposed study directly applicable to one or more of the following listed ESUs: Upper Columbia, Mid-Columbia, and Snake River steelhead.*

Yes, in all three.

- *Cost-effectiveness (e.g., the ability to take advantage of existing fish production, research, monitoring or evaluation activities) will be an important consideration in the proposal selection process.*

No, at ~$1,017K for year one, this is one of the more expensive proposals. However, the study allows for replication in 5 streams, or about $200K per stream, the standard budget.

Action 182: Studies to Determine Reproductive Success of Hatchery Spawners

Background

This RFS sought proposals from qualified individuals or groups to conduct scientifically sound studies that focus on the biological question(s) to determine relative reproductive success of natural-origin and wild-spawning hatchery-origin anadromous salmonids in the Columbia Basin. Studies must be designed to directly estimate the reproductive success of both hatchery-origin and natural-origin fish spawning naturally in the same population. These studies are needed to address scientific uncertainties outlined in Reasonable and Prudent Alternative (RPA) Action 182. Determining the reproductive success of natural-origin and hatchery-origin fish addresses critical uncertainties regarding population status assessment and recovery planning.

The RFS sought studies directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead, Snake River fall chinook, and Columbia River chum. Proposals were received for all of those listed ESUs except Upper Columbia steelhead. Studies not occurring in those ESUs,

but with clear applicability to those ESUs will also be considered. Several studies with general applicability to steelhead and spring/summer chinook were received. Cost for a reproductive effectiveness study at the general desired level of effort was anticipated to be $200,000 to $300,000 per year. Implementation of four studies was the intent of this solicitation, but this number could vary, based on the final negotiated costs of the selected proposals. Nine proposals were received addressing this RFS.

ISRP General Comments on the Request for Studies for RPA 182

The RME group has not clearly differentiated the contribution that reproductive success evaluations of hatchery-origin and natural-origin salmon and steelhead when spawning naturally make to fulfilling RPA 182 vs. 184. Based on the background documents, the ISRP's understanding is that RPA 182 addresses the problem of calculating lambda for BiOp check-ins in 2005 and 2008 for populations composed of both hatchery- and natural-origin adults. As the reproductive success of the hatchery-origin adults increases, the ESA status of the population may deteriorate because the population is being supported (replaced?) by hatchery fish. In the RFS for RPA 182, the RME group gives priority to state-of-the-art hatchery programs. It is not clear why this should be a priority. The ISRP believes an important priority should be conducting these assessments with those ESUs and populations where hatchery strays are perceived to be a problem for the ESA assessments. In evaluation of these proposals for funding, the ISRP recommends that the funding agencies consider those that test the reproductive success of hatchery-origin adults that are creating the uncertainty, not necessarily selecting a subset of the state-of-the-art artificial production programs within the basin. Parts of the RFS and some of the projects read more like they are evaluating whether hatchery fish can make a contribution to recovery. This is covered under RPA 184. The reproductive success evaluations can make meaningful contributions to both of these RPAs.

These issues need to be considered in deciding which projects are likely to provide the information required to fulfill the RPA 182 and 184 uncertainties. As examples, Proposal 1, an investigation of the reproductive success of hatchery-origin adults in the Deschutes River Basin, does not evaluate "state-of-the-art" fish culture operations. However, it is evaluating hatchery-origin adult strays from variable unknown sources that create the dilemma for the ESA calculations of lambda. Other projects are evaluating hatchery-origin adults that are part of reintroduction programs. These projects appear directed at the question of what contribution hatchery-origin adults can make to recovery, rather than developing the data to adjust the calculation of lambda.

Comments Applicable to the Set of Proposals

For the entire suite of projects to fulfill the RPA, the ISRP believes that it is necessary to consider whether the "natural" fish inhabiting an assessment stream are representative of "wild" fish. The ISRP is concerned about interpreting studies where the extant population has a long history potential interbreeding with hatchery fish. Has the hatchery component been a part of the population for so long that there is little difference between the hatchery and wild? Assessments of reproductive success in control or reference streams with little past hatchery influence may be helpful in sorting out the effects of past interbreeding between hatchery and natural fish. A related issue is that principal investigators of proposals in this section should address how the results will be interpreted if the progeny of HxH, HxW, and WxW crosses have equal fitness in the F_2 and F_3 generations. That is, should one conclude that the genetics of the WxW crosses are so similar to the hatchery crosses that no difference in fitness can be determined, or should one conclude that supplementation and/or straying is working perfectly? It is not clear to the ISRP that available data will be able to distinguish between these two conclusions. That is, is it enough just to compare fitness of HxW and HxH crosses with fitness of WxW in the same stream when the fitness of the WxW may be changing

because of supplementation or straying? This question was asked of all proposals submitted in response to the RFS covering RPA #182.

The RFS requested that proposals address the likely effect of any difference in reproductive success of hatchery and wild fish, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria. Control streams or some other means of assessing effects of out of basin factors on population growth rate and other demographic parameters would be crucial in determining the robustness of inferences about population-level impacts over the long-term.

In general, the initial proposals did not do a good job of assessing the potential effects of resident trout on the results of the steelhead studies. The principal investigators were asked how they will evaluate differences in reproductive success of the various crosses, if there is substantial interaction and interbreeding with resident trout, as might be expected to occur in the Deschutes, for example?

Finally, there are significant data management issues involved in genetic testing, in the sampling protocols, and in the sheer magnitude of data that will be generated by these proposals. Without adequate quality assurance and control, researchers could make subjective decisions that could substantially affect their outcomes. Thus, quality assurances and control plans should be built into the proposals. Addressing QA/QC should be done regionally, as well as for individual projects.

Proposal 8: Evaluating the Reproductive Success of Natural- and Hatchery-Origin Columbia River Chum Salmon (WDFW)

ESU: Columbia River Chum
ISRP Final Recommendation:
Fundable. This is one of the better-designed studies submitted for the RFS.

If this is the only chum hatchery on the Columbia is this a high priority project? In the current chum ESU is there any problem estimating lambda? Does this hatchery create a problem with strays? Does this ESU really need the amount of effort that upriver stocks need? This may be a forward-looking problem and could help the region decide whether to ramp down hatchery chum production. Having some work on chum restoration would likely be worthwhile. The comments help explain the unique aspects of this system. It is not perfect, but good information should come from it, especially after this exchange.

Responses to most ISRP questions were reasonable. A question remains as to whether the Grays River population represents a truly wild population uninfluenced by earlier hatchery introductions. The answer to this question in the sponsor's response is conjectural and not especially compelling. The genetic composition of the Grays River stock should be compared with other Lower Columbia River chum stocks to at least demonstrate whether the Grays River population is representative of other Lower River chum stocks. A solid rationale is presented for conducting the research in the Elochoman rather than the Grays River. Straying of non-project fish into the Elochoman likely would be less than in the Grays and the ability to detect strays would be greater.

The ISRP question concerning genetic background (Q2 in their reply) seems to be a reasonable assumption, but it is an assumption, and they should continue to consider this issue.

Regarding the ISRP's question about fish from the same origin spawning separately vs. fish from mixed origins spawning together, the logic of the method described in the response seems a bit fuzzy and appears to be based on a strong assurance of controlling sampling proportions and then being able to track subsequent genetic transmissions.

Given the ISRP site visit to the Grays River, many of the ISRP questions would not be too practical in that environment. Highly variable flows and an unstable stream channel in the Grays likely would reduce the ability of the investigators to enumerate juvenile chum and could jeopardize the persistence of a weir-controlled project site. They have provided sound answers to our questions.

The sponsors presented evidence from decoded otoliths that the first generation adults collected at Crazy Johnson Creek and West Fork of the Grays would be predominantly of wild-origin and hatchery-origin, respectively. Furthermore, they have developed a method for assessing the percentage of hatchery and wild parents from each site at the time fish are collected for transplantation to the Elochoman.

Dr. Schroder is identified as the project lead. He has extensive experience working with chum salmon – likely the most experience in the Pacific NW.

The response does not address concerns about the size of the budget.

ISRP Preliminary Recommendation:
Fundable contingent on an adequate response to ISRP questions and comments. This proposal is a technically sound investigation, in a "semi controlled spawning habitat" mesocosm, of reproductive success of hatchery-bred and natural origin chum salmon from the Grays River. It will enable researchers to study in detail morphological and behavioral components of fitness of spawning chum salmon and potentially to understand related differences of reproductive success, measured at both fry and adult life stages, between hatchery-origin and natural-origin spawners. The origins of individual parents of the first filial generation would be identifiable from dissected otoliths after spawning and they would be associated with their offspring by their msDNA genotypes. The lineages of later generations would be identifiable by msDNA parentage analysis. It is not clear to reviewers why a direct study of reproductive success in the Gray's River or a weir-controlled tributary of the River would not be more directly applicable to questions about the relative fitness of hatchery-origin chum salmon and of their offspring. Will salmon in a gravel-filled trough exhibit the same array of behaviors that they would in their native habitat? Would a direct study be infeasible?

The proposers report that first-generation returns to the supplementation hatchery on the Grays River last year (2002?) may have comprised the largest spawning population ever observed in the river but that otoliths recovered from spent fish have not been examined and it is not known whether salmon produced by the hatchery contributed substantially to the spawning population. What proportion of Grays River spawners in 2002 were produced by the hatchery? (Surely a small sample of otoliths could readily be examined.)

Assuming that a substantial portion of the Grays River spawners will have been produced from the hatchery, the proposers establish the purpose of their research as discovering whether these hatchery origin spawners are reproductively successful, as successful as wild-origin fish. That is, the rationale for hatchery supplementation depends on the reproductive success of the hatchery-origin spawners without cost to the fitness of the natural-origin component of the population. They point out that the Grays River population is the only Columbia River population with a supplementation hatchery component, thus it offers the only opportunity for comparative study of reproductive success in hatchery- and natural-origin chum salmon. Reviewers ask whether a more appropriate reference would be the reproductive success of natural-origin chum salmon in the absence of hatchery-origin salmon; could research in a 'semi controlled' spawning habitat such as that proposed at Beaver Creek be designed to provide such a comparison? Could comparisons of reproductive success between hatchery-origin rich and natural-origin rich tributaries of the Grays River provide such comparisons? It concerns the ISRP that the proposed research in a 'semi controlled' habitat will not be able to control the proportion of hatchery origin (HOR) and natural origin (NOR) spawners except by choosing the tributary of the Grays River

where the spawners are collected (Crazy Johnson Creek is believed to be rich in natural origin spawners, West Fork in hatchery origin spawners.) What evidence is there (if no otoliths have been examined) that this spawner-selection protocol will provide adequate experimental contrast between the lineages? Without control over the proportions of HOR- and NOR-spawners, will the proposed research detect any frequency-of-HORs effect on the reproductive success of HOR and NOR spawners?

The proposed research is expensive, but it is the only proposed research in the ESU.

Does the study address the following RFS questions:

> *• Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
>
> Yes – assuming that enough salmon survive to be counted and that adequate numbers of HO and NO parents are incorporated in the initial spawning.
>
> *• What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*
>
> Yes – a strength of the proposed research is that behavioral and morphological components of reproductive success of individual spawners will be observed.
>
> *• What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*
>
> The project has the potential to address whether the population grows and how genetic diversity and fitness are affected by hatchery origin spawners, but the proposal does not address the question directly.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

> *• The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*
>
> Yes—it is the only proposed study of reproductive success in the Columbia River chum ESU.
>
> *• The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*
> Yes
>
> *• The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*
>
> More to the point— reproductive comparisons from smolt species may not be pertinent to chum, a fry-emigrating species, so it is arguably important to compare reproductive success in chum salmon.
>
> *• Potential to commit to a long-term study (beyond F2); and*
> Yes - the potential is here.
>
> *• Overall cost effectiveness*
>
> Highest cost of all the proposals in this set.

Proposal 7: Reproductive Success of Hatchery Spawners in the Chinook River, Washington (Sea Resources)

ESU: Lower Columbia River Coho

ISRP Final Recommendation:

Do not fund. The ISRP posed several questions critical to the study design. An ISRP fundable recommendation was contingent on an adequate response to the ISRP's questions. These questions were not addressed in a response from the sponsors. Funding this project without a response would be unfair to other investigators who have prepared thoughtful responses to the ISRP's questions.

ISRP Preliminary Recommendation:

Fundable contingent on an adequate response to ISRP questions and comments. The research would compare reproductive success of hatchery-origin and natural-origin spawners that are both derived from a conventional hatchery program. Because it does not contrast reproductive success of hatchery and natural origin fish in one of the listed ESU's the proposed research would not strictly meet the criteria in the RFS. However, the questions the project would address are significant and address important uncertainties for listed ESU's of other species. The results would be directly applicable to the Lower Columbia River Southwest Washington coho ESU which are a candidate for listing under the Endangered Species Act and which the recent report by the Biological Review Team categorized as endangered.

Models of how interbreeding with hatchery-origin fish may reduce fitness of natural-origin fish aren't developed and the research isn't proposed in context of such models. The hypotheses stated in objectives 2 and 3 are simplistic statistical hypotheses. Mechanistic hypotheses posing possible causal relationships would be more useful and more interesting. For example, what phenotypic traits would be expected from each type of cross, how would they affect fitness, and why would they be expected. If these kinds of hypotheses are not proposed, the applicants should explain why.

In objective 2 the applicants propose to estimate fry and parr survival. How would this be accomplished? If egg-fry survival is to be estimated, how will spawner density and egg production from each type of cross be determined? If the purpose is only to estimate fry produced by each type of cross (fry/spawner), the number of spawners must be determined. How will this determination be made?

"After the third year of the project, we will be able to assign parentage to the adults returning to the weir" –How will strays into the Chinook River be accounted for? Will there be substantial numbers of unidentifiable coho? Will they be prohibited from entering the River?

In general the methods for several of the sub-objectives of objective three are not well explained. For example, how will heritability be estimated (objective a4), why is heritability an important parameter, how will objective a6 be accomplished, what are the direct and indirect methods proposed in objective a7? Objective 3, task a3 contains two different parts -- determining whether random mating occurs and determining whether the different crosses have offspring with similar survival rates. The two Parts of the objective should be dealt with separately. The methods for accomplishing objective a10, which apparently attempts to address evaluation criterion 3, need to be explained in far greater detail.

The methods call for collection and genotyping of "Approximately 2000 offspring produced by each set of potential parents"; the number of potential parents apparently numbers in the hundreds and the number of sets of parents would be very large, so this would be in aggregate an intractable task.

Is this a suitable site for this study if the planned estuary restoration projects (e.g. removal of tidegates) are implemented? How would this impact the study design?

Does the study address the following RFS questions:

• *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*

Yes

• *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*

Yes – there would be evaluations of reproductive success at several life stages.

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*

The project has the potential to address whether the population grows and how genetic diversity and fitness are affected by hatchery origin spawners, but the proposal does not address the question directly.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*

No, but the results would be directly applicable to the Lower Columbia River Southwest Washington coho ESU which are a Candidate for listing under the Endangered Species Act and which the recent report by the Biological Review Team categorized as endangered and the project would address are significant and address important uncertainties for other listed ESU's

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*

Yes - the study could be extended.

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*

Yes, but the proposal does not respond explicitly to this question.

• *Potential to commit to a long-term study (beyond F2); and*

Yes – the project could commit to a long term study.

• *Overall cost effectiveness*

Relatively inexpensive compared to other proposed projects, benefits from collaboration of different groups and from other related projects.

Proposal 15: Natural Reproductive Success and Demographic Effects of Hatchery-Origin Steelhead in Abernathy Creek, Washington: Can Newly Developed, Native Broodstocks of Steelhead Derived from Captively Reared Parr Potentially Contribute to Recovery of Naturally Spawning Populations? (USFWS)

ESU: Southwest Washington Steelhead; Systemwide Applicability across the ESUs

ISRP Final Recommendation:

Fundable. This is a proof-of-principle study that cuts across the RPAs and the broader needs of the BiOp. The responses reasonably address the ISRP's questions as well as issues related to RPAs 182 and 184. The ISRP agrees with their prelude to specific comments. It could potentially fail because the questions posed in the RFS are too narrow, thus causing this study to lack some appropriate contents. That should not doom it. As the proponents stress, it is a research project that has systemwide application. The proponents do not attempt to cloak it in a garb that is not intended.

The authors have responded well to each question: past hatchery effects, measurements in F_2 and F_3, use of controls and impacts of out-of-basin effects, impact of resident rainbows, etc. The monitoring system in this proposal is sound and extensive sampling (for DNA etc.) can be maintained over time. There are two control streams to compare with and to measure out-of-basin effects. The issue of resident fish is much less in this system due to its location. This is a scientifically sound proposal.

The response is a little weak on the QA/QC question. They describe capability of managing data, but do not elaborate on quality control aspects.

The comment from the H/H group is uninformed, but the proposers provided a lengthy response. Why the H/H would refer to this as captive brood (as in the sense used elsewhere), the ISRP did not understand, but the methods developed in this proposal could definitely be used elsewhere in the basin.

> **ISRP Preliminary Recommendation:**
> Fundable, contingent on an adequate response to the ISRP's questions. The proposed research (favorably reviewed by ISRP after submission in three earlier contexts) would investigate a novel method for developing native broodstocks of steelhead as required by the NMFS BiOp on Artificial Propagation, i.e. rearing captured parr to maturity as a source of broodstock rather than taking eggs from mature native fish. Thus, even though the research is not directly on one of the listed ESU's, its results will be directly applicable to recovery of listed steelhead. It is noteworthy that, unlike other research on the reproductive success of hatchery origin (HOR) and natural origin (NOR) salmon, in this study the productivity of natural origin fish will be observed in control streams, without the presence of hatchery origin fish; most of the other projects would compare reproductive success of natural origin and hatchery origin fish, but only in the presence of each other. The Deschutes River study, proposal 1, also has controls.
>
> Questions arising in the present review include one concerning the putatively native fish in Abernathy Creek—how influenced has this population been by hatchery production in the past, from other populations? Secondly—how will straying of steelhead into the control streams influence their comparison of productivity in the supplemented Abernathy Creek population to productivity in the control streams? Is there inter-breeding with resident trout and if so, how is it accounted for in the analyses?
>
> Does the study address the following RFS questions:
>
> • *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
>
> Yes

• *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*

Yes – the research would evaluate reproductive success, survival of offspring, during the freshwater phase of the life history, as smolts emigrating from Abernathy Creek, and as mature adults. Mechanistic hypotheses relating parentage of fish to survival at sequential life stages aren't addressed explicitly in the proposal, but are implicit and will be susceptible to testing with the data gathered.

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*

The project has the potential to address whether the population grows and how genetic diversity and fitness are affected by hatchery origin spawners, but the proposal does not address the question directly.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*

Yes – this study will not occur in a listed ESU but is designed to produce results applicable to listed ESU's.

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*

Yes - study design will allow determination of whether statistically significant differences in reproductive success exist between NOR and HOR in generations beyond F1 (to F3). It contains a good selection of treatment and control sites.

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*

Yes - the rationale is that this is a model system for developing supplementation strategies for upper and mid Columbia steelhead.

• *Potential to commit to a long-term study (beyond F2); and*

Yes

• *Overall cost effectiveness*

The proposed research would build on and the requested funds would be leveraged by other ongoing work. Methods are well established and reliable.

Proposal 2: Evaluation of the Reproductive Success of Wild and Hatchery Steelhead in Natural and Hatchery Environments (UW)

ESU: Systemwide Applicability across ESUs

ISRP Final Recommendation:

Fundable, highest priority. This proposal is applicable systemwide and specifically to the RPA. A huge strength is that the project is well underway and already has two-three generations of pedigree and fitness data available for analysis. The region should take advantage of this opportunity. It will provide data much sooner than other proposals that are in planning stages.

The response is excellent, extremely thorough, and goes beyond addressing the stated review concern to also address deeper underlying issues. The response is persuasive about how it addresses RPA 182. The ISRP has no hesitation in supporting this study. This is the only study that could possibly address the concern of "reverse domestication." This proposal offers opportunities to monitor essentially all the genetic issues raised in regard to hatcheries. The fact that this is out-of-basin does not temper its strengths and applicability. The Conclusions section of the response provides five good reasons why the proposal should be funded. The letter of support from Fred Utter was persuasive as well.

The responses for this proposal highlight what is analogous to a "positive control," that is, having effects strong enough that they can be seen. The ISRP agrees with the proponents that it does little good to study something so close to reality that it is impossible to see any real differences for reasons of experimental design or sampling capabilities. Close genetic stocks pose the risk of a Type 2 error. This aspect of their proposal should increase interest in it, not disqualify it. The temporal control that they have is valuable. A spatial one would be good, especially if fish numbers are the main objective, but it is not crucial for the genetic discriminations that are sought.

The ISRP agrees with the sponsor that coordination among projects would add significantly to the work funded by BPA and that annual workshops should be funded by BPA to facilitate this process.

> **ISRP Preliminary Recommendation:**
> Fundable, a high quality proposal, but it does not specifically address RPA 182 and some questions in the RFS. The study is investigating the natural spawning success of an imported, highly domesticated, steelhead stock and to that extent addresses questions posed by RPA 182. In Forks Creek a decision was made to exclude all future hatchery-origin adults access to the natural spawning grounds after they had access for two return years. Preliminary studies have examined the spawning by these hatchery-origin adults. The current request is to follow the trajectory of hatchery and hatchery x wild lines to examine their fate. Even though the situation is not an ideal model of a supplementation program, understanding the uncertainty addressed here is important for the basin.
>
> This study is different from most of the others because it is investigating pedigrees both inside and outside the hatchery. This adds a dimension that potentially could contribute to understanding the consequences of selection differences between the hatchery environment and the natural environment. It is a fascinating study, with considerable overarching policy implications from the results. It is less clear how the results will assist in parameterizing the extinction risk models used in recovery planning.
>
> Is there inter-breeding with resident trout and if so, how is it accounted for in the analyses?

Does the study address the following RFS questions:

• *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
Yes

• *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*
Yes

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*

Not easily - It is not clear how the results will assist in parameterizing the extinction risk models used in recovery planning because the hatchery origin fish in this study are not native, have inherently maladaptive life history traits in the receiving environment, and have been subject to artificial culture and selection.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*

The study site and ESU fall outside of the Columbia basin – the studies conclusions would be applicable to situations in which the HO population is not native or locally adapted to the watershed of the hatchery of concern.

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*
Yes

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*
Yes

• *Potential to commit to a long-term study (beyond F2); and*
Yes

• *Overall cost effectiveness*
Good

Proposal 6: Relative Reproductive Success of Hatchery-Origin and Wild-Origin Steelhead Spawning Naturally in the Hood River (OSU)

ESU: Lower Columbia River Steelhead; Systemwide Applicability across ESUs
ISRP Final Recommendation:
Fundable, high rank. A response was not needed. If selected for funding the principal investigator should respond to the general ISRP comments in the contracting process. The ISRP ranks this proposal high even though it is not one of the priority ESUs. Preliminary data exist, the information is applicable to other locations, and useful results can be obtained on the fitness of F_2 and F_3 progeny relatively quickly.

Does the study address the following RFS questions:

> • *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
>
> Yes, these questions can be answered by the proposed research.
>
> • *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*
>
> Yes, these questions can be answered by the proposed research.
>
> • *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*
>
> Yes, this question can be answered by the proposed research.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

> • *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*
>
> The study addresses the Lower Columbia River steelhead ESU and results should be applicable to the Mid-Columbia steelhead ESU, and other locations.
>
> • *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*
> Yes.

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*

Yes.

• *Potential to commit to a long-term study (beyond F2); and*

Yes.

• *Overall cost effectiveness*

At approximately $215K to $270K per year the costs seem to be in line with other proposals.

General comments and evaluation.

The study contrasts "state of the art" supplementation derived hatchery-origin adults and conventional hatchery-origin adults with natural-origin adults for both winter and summer runs. The study has considerable data already analyzed. The contrasts are sequential, not contemporaneous. The same stock in the same stream is being evaluated. Reproductive success of the entire population over several generations is possible because of sampling at Powerdale Dam. There will be direct estimates of reproductive contribution.

This study will evaluate reproductive performance of all the potential parents to a steelhead population over nearly two decades. The selection of loci is finished and preliminary analyses from selected years are included in the proposal. The project uses one of the better managed and monitored supplementation projects. Since this is a retrospective reconstruction of two decades of pedigrees the timeline to completion should be shorter than for those projects that must rely on future returns.

The principal investigator should outline an approach for investigating possible causes of difference in reproductive success. The ISRP notes that the cause of differences will not be evaluated, nor will the analysis partition survival to different life-stages.

Is there a possible “control” stream with only WxW crosses?

The methods are a little vague, but the preliminary analyses have been conducted and the ISRP judges that this is probably not a problem.

Is there inter-breeding with resident trout and if so, how is it accounted for in the analyses?

Proposal 1: Investigation of the Relative Reproductive Success of Hatchery and Wild Steelhead in the Deschutes River Basin (ODFW)

ESU: Mid-Columbia River Steelhead
ISRP Final Recommendation:
Fundable in phases.

In the initial ISRP review, this proposal had good support and was evaluated as having scientific merit. The responses are adequate to most questions. The research should continue through the F_2 generation even though the timeframe for the project will be extended substantially.

Less convincing is the sponsor's response to the question of whether there are genetic differences between natural origin adults and hatchery strays. The sponsors state that this question will be answered during the first two years of the study, but they do not discuss in any substantive way how the research will be modified if there is little genetic divergence. In fact, if there is little divergence another research design may be warranted and the value of the project may be reduced.

In answer to the comment about the potential difficulty of differentiating between progeny of strays and "wild" fish, the response provides an example of two extremes (no introgression vs. high levels of introgression), then describes how each will be tested for. But isn't the real question how tests will be able to determine the "middle ground" cases in between the extremes? The potential inability to assess the influence of strays seems to continue to be a weakness of this proposal, so the same recommendation to fund in phases still applies.

The response to the sampling question was not complete. That question concerned whether the abundance of juveniles will be sufficient to sample contributions from the spawning adults. Two issues remain: the sample size necessary and how to determine that, and what value the juveniles have compared to the monitoring of adults only. The juvenile sample size issue seems more important than they recognized. Other proposals refer to large samples sizes (a couple of thousand) in order to have confidence in the parentage of the progeny. Given the concerns about genetic background and the unknown number adults in the treatment stream, this remains an issue for them to consider. However, if the real issue is source of returning adults, maybe the juvenile sampling is unnecessary in this study especially if the numbers of wild fish are limited. These seem to be important questions that the ISRP asked, and that remain now.

The location proposed may not be suitable to do this study. The potential for spawning with resident rainbows is high (e.g., studies by Zimmerman and colleagues). The proposers recognize the problem. If this ends up just a parentage analysis, it won't move things along as the initial proposal promises. Their existing database is a plus for this proposal.

The ISRP recommends that BPA fund a preliminary study to first document the incidence of straying in each stream and the degree of genetic difference between naturally spawning adults and hatchery strays. Funding of Phase 2 should be contingent on ability to document straying in phase 1. The results of this study should be reviewed by the ISRP prior to final approval of the project.

The ISRP was mystified by the comments from the H/H group. The issue of strays is an issue throughout the Basin. Strays are probably a larger problem basinwide than intentional supplementation with hatchery fish. It makes sense to opportunistically use the strays that are "naturally" provided by returning fish to these well-controlled streams. They should give as good

information as intentionally supplemented fish. The ISRP supports the proposers' response to the H/H questions. The issue of whether hatchery strays meet the intent of the RFS request should be resolved by the RFS team.

ISRP Preliminary Recommendation: Qualified fundable. The study should be implemented in phases (see below for recommendations) given adequate response to reviewer's questions and comments. The study is important and addresses RPA 182, but does not directly address the requirements of the RFS.

Does the study address the following RFS questions:

• *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*

The evaluation is designed to test for significant differences in reproductive success between natural-origin and hatchery-origin steelhead. The evaluation does not include a design to extend to the F_2 adult returns, but could be easily modified to include F2 adult returns.

• *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*

Two life-stages, egg-to-smolt and smolt-to-adult, will be evaluated along with contrasts of life-history characters between adjacent treatment and reference streams. The sponsors indicate they will estimate the heritability of life-history characters. This will be less informative than establishing phenotypic and genetic correlation's between "traits" and "fitness".

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*

The study would contribute data important to parameterizing the extinction risk models used by NOAA-Fisheries.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*

The study involves Mid-Columbia steelhead.

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*

This is not included in the proposal but could be addressed.

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*

Good for steelhead above Bonneville.

• *Potential to commit to a long-term study (beyond F2);*

Good.

• *Overall cost effectiveness.*

Maybe a little on the expensive side, but there are more startup costs in this one. $400K the first year and $300 each year after does not seem to be too much out of line with the other proposals.

General comments and evaluation.

This well-written proposal addresses the potentially important problem of effects of hatchery straying on naturally spawning steelhead in the Deschutes subbasin. However, the primary weakness of this study is unknown implementation conditions in the two creeks. The applicants should first document the incidence of straying in each stream and the degree of genetic difference between naturally spawning adults and hatchery strays. Both streams appear to have had an extensive history of stray hatchery fish spawning with “wild” steelhead. If genetic introgression has been extensive the genetic makeup of “hatchery” strays and “wild” spawners may be similar, reducing the ability of the researchers to detect genetic differences between progeny of strays and “wild” fish. Also, the abundance of adults and juveniles should be evaluated because parentage assignments will be made on a sample of juveniles, and all adults once they start returning. It is not clear whether the abundance of juveniles will be sufficient to sample contributions from the spawning adults. Future funding should be contingent upon the results of the preliminary work.

The project evaluates "stray" hatchery-origin adults spawning in the wild rather than individuals from "state of the art" hatchery programs and for this reason does not seem to directly respond to the requirements of the RFS. It does respond to RPA #182. The focus of the proposal is on assessing the effect of stray hatchery fish, period. It seems that the study plan should also somehow incorporate the local Round Butte or Warm Springs hatchery fish. As it stands, this study may do a great job of evaluating the consequences of hatchery strays, but ignores some of the large issues that need to be addressed.

This would be a fascinating study to see what would happen to fitness of later generations when you cut the hatchery strays off from the control stream.

The experimental approach does not permit evaluating the "same stock in the same stream", since the strays are not necessarily from the same Mid-Columbia ESU and could change from year to year.

Use of a “control” stream is a MAJOR plus for the study. Most of the proposals do not have adequate control streams in the study design. Bakeoven and Buck Hollow creeks are probably good choices because they are relative warm on the east side of the Deschutes and may represent extreme environmental conditions to which wild steelhead are adapted.

The principal investigator proposes to consider inter breeding with resident rainbows. The proposal does not carefully describe how the principal investigator would evaluate differences in reproductive success if they find large effects of resident fish (no testable hypotheses) but other proposals did not do that, either.

Proposal 3: Pedigree Approach to Determine Reproductive Success of Natural and Hatchery Origin Spring/Summer Chinook Salmon Spawners in Johnson Creek, Idaho (NPT)

ESU: Snake River Spring/Summer Chinook

ISRP Final Recommendation:

Not fundable, the proposal and response did not provide a compelling rationale for funding this project. While the authors provided extensive comments, too many questions remain.

On page 5, concerning the "conclusions that supplementation has had or will have a positive effect"; reviewers do not follow this answer. The authors' respond that the adult-to-adult return rate for supplementation fish was twice the wild fish. How do they know this? To estimate these returns would mean that adults are "typed" to origin, further it ignores any information from smolt production. If adult typing is being done already, why is this proposal being submitted again and who is paying for what now?

The interpretation of results, bottom page 7, remains a valid issue but clearly falls into the "explanation" type of data (as ISAB identified in the Supplementation report). The authors acknowledge that they cannot differentiate results without doing the mating studies or observations in the stream (which would require some external mark). The general issue of what needs to be sampled affects most of the remaining points, including the discussion on demographic aspects. While the authors' acknowledge that not conducting the juvenile sample would reduce costs, they could not answer most of the remaining questions or explain interactions in the stream. They do not seem firm on this issue, one way or the other.

On page 10, question 5 is not really answered. They do need to consider the loading ratio for these studies … what will be done if there is very poor production from the naturally spawning fish?

On page 10, question 8. If the lab currently analyses 4 loci, that is simply not sufficient for parentage analyses. Other proposals responding to the RFS, intended to use 16-20 microsatellite loci for similar analyses, thus providing considerably more analytical power. This low number of loci would have to influence what they can say about analyses-to-date. The number of loci must be increased, as they say in the last sentence of that paragraph.

Reviewers were not very supportive of modeling to "attempt to generate a "control" of demographic parameters that would … if supplementation was not occurring". Reviewers doubt that this is a worthwhile component of what the RFS funds should cover. If the proposers want to create a model, the first thing reviewers would suggest is for them to develop a flow diagram of effects, interactions, necessary parameters, etc. By doing this, they may at least determine what the minimum information (the critical or vital statistics) necessary is for their study.

The discussion regarding proposed use of Lake Creek as a quasi-control stream was highly redundant and not clearly presented. The same comments from the initial ISRP review about the brevity of information provided for Objectives 3 & 4 still apply. The methods for Objective 3 are still quite briefly described, and the model proposed for Objective 4 is not only briefly described, it also sounds potentially uninformative and optional.

The underlying theme of this proposal "to document a success" raised questions of objectivity. Recent ISRP comments on the ISS design may also influence the value of Johnson Creek for this solicitation.

ISRP Preliminary Recommendation:
Fundable in part for Objectives 1 and 2, contingent upon adequate response to the ISRP's questions and comments. Objectives 3 and 4 require major revision as to how they will be accomplished. Regardless this proposal is low priority. It appears to duplicate other studies in this ESU. This ESU is not one of the four priority ESUs listed in the RFS, but the proposal could have application to other spring/summer chinook populations.

Does the study address the following RFS questions:

• *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
Yes, with inferences to F3 progeny during the course of the study.

• *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*
Yes. They have the ability to look at survival during different life stages and periods of migration.

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*
We believe so, but this aspect of the research is poorly addressed.

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*

The study involves spring/summer chinook in Johnson Creek, East Fork of South Fork of Salmon River. The ESU is not one of the above four, but the study should be of interest. Johnson Creek is in an ESU listed as threatened under the ESA.

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*
Yes. The study will address both F_2 and F_3 generations.

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*
The study is of interest to spring/summer chinook in other ESUs.

• *Potential to commit to a long-term study (beyond F2); and*
The study will carry through three complete F_3 brood years.

• *Overall cost effectiveness*
The cost for this eight-year study seems reasonable.

This proposal seeks to address elements of each of the three major criteria posed in the RFS. In addition the proposal attempts to address several of the additional criteria including evaluation of reproductive success

beyond the F_2 generation and the potential to commit to longer-term studies. It does not specifically address the ESU's listed in the additional criteria but may be applicable to them.
The study proposes to evaluate reproductive success of hatchery and wild spring/summer chinook spawners in Johnson Creek, Idaho. A supplementation project was begun in Johnson Creek in 1998. Facilities for collecting adults and juveniles have been established. Apparently genetic and demographic data were obtained from wild x wild crosses prior to the first return of supplemented adults in 2001. The study dovetails with ongoing trapping and monitoring at Johnson Creek. The applicants provide better than average detail regarding the approach to evaluate possible differences in reproductive success.

The applicants seem to have reached the conclusion that supplementation has had or will have a positive effect on adult returns and genetic diversity without full analysis of current and future data. On page 2, apparently as a result of comparing adult returns in 2001 and 2002 to returns in previous years, the conclusions is drawn that " the supplementation effort has succeeded in increasing adult returns to Johnson Creek." Similarly, on page 11-12, the statement is made that " the model can be used to determine the optimal number of supplementation fish that could spawn with wild fish to maximize genetic diversity or effective population size." The assumption here appears to be that supplementation will indeed enhance genetic diversity of the population. The applicants have not provided concrete evidence that supplementation has been successful in Johnson Creek in part because they have failed to account for increases in population size that could have occurred as a result of changes in out-of-basin factors such as ocean conditions and to provide information on the relative contribution of wild and supplemented adult to population growth. The failure to address out-of-basin factors pervades the proposed demographic analysis.

Genetic aspects of the study:
1. How valid is the assumption of random mating between wild and supplemented adults. A justification for this assumption is needed, especially in light of research by investigators such as Fleming and Gross that demonstrated differences in reproductive behavior between wild and hatchery spawners.
2. Will the effects of crosses of different sexes of fish with wild and hatchery origins (e.g., wild male x hatchery female, hatchery male x wild female) be analyzed. If not, why?
3. There is little discussion of how the data will be analyzed and tested statistically.
4. A comparison of changes in fitness of WxW crosses with changes (or lack of changes) in fitness of WxW crosses in a "control" stream of the ISS would be useful. Is it sufficient just to compare fitness of HxW and HxH crosses with WxW when the fitness of the WxW may be changing because of the supplementation?
5. The ratio of hatchery to wild spawners was not clear. From the proposal, it appears that all returning supplementation fish are passed above the weir and allowed to spawn, regardless of the number of wild spawners that return. Is this wise, or does it allow a unique look at supplementation?
6. The applicants propose that the study could be restricted to adult sampling to reduce the cost. This would also put the study results in line with what is possible in some of the other proposals, i.e., fitness as measured by "adult to adult" returns.
7. Only wild adult chinook salmon are retained for brood stock so this should provide a unique look at the issues of design and success of supplementation projects.
8. Who will perform the laboratory work?

Demographic aspects of the study:
This aspect of the research is very weak and is described in a cursory manner. The methods for accomplishing objectives three and four, which address demographic aspects of the study, are not clearly and comprehensively discussed, and the problems and assumptions with the proposed approaches are not identified and evaluated. A population ecologist/modeler should be a co-investigator on the proposal and provide the necessary technical expertise for the demographic and model development section of the proposal.
1. The specific demographic and genetic information available for the population prior to supplementation is not given. It is unclear if and how this information will be used in assessing whether supplementation has changed the demographic parameters of the population.
2. The applicants propose to develop a model to evaluate the effects of supplementation on demographic parameters. The model is not described in sufficient detail to permit assessment of its adequacy in evaluating supplementation effects. It is unclear exactly what the structure and parameters of the model will be, how the model will be used to compare population demographics of the supplemented and unsupplemented population, how uncertainty will be incorporated, and how out-of-basin factors affecting adult returns and juvenile survival will be accounted for. Are there similar types of models in use today or is this a pioneering effort? The qualifications of the proposed modelers were not given.

Proposal 11: Comparative Reproductive Success of Wild and Hatchery Origin Spring/Summer Chinook Salmon that Spawn Naturally in the Pahsimeroi and Upper Salmon Rivers (IDFG)

ESU: Snake River Spring/Summer Chinook

ISRP Final Recommendation:

Fundable, but low priority. The response was adequate but did not elevate the proposal above a low priority. They propose additional work to examine possible causes of differences in wild/hatchery fish reproductive success, but do not tie that proposed work to results or ideas gleaned from their extensive and long-standing ISS project. It is not clear what the budget will be for the additional objectives. Some of the methods proposed are questionable.

An extensive portion of the reply deals with the ISRP comment regarding "possible causes for differences…". There seems to have been some misunderstanding about the need for this in every proposal. The text introducing the kelt reconditioning proposals differentiates between reproductive success and reproductive fitness (the latter meaning a more long-term measure). These statements are true, but it is a leap to conclude that every proposal needs to measure fitness over 2 to 3 generations and to explain all differences. As the ISAB just stated in the Supplementation report: everything that can be measured does not need to be measured. Projects should measure the vital statistics, and the explanatory variables as possible or needed.

In this case, the extensive addition of methods generates more concerns for procedures. To do much of what they propose, they would have to be able to identify supplemented or hatchery fish from wild fish. For example, they propose to use Peterson disc tags put on at the fences. These spring/summer chinooks may be in the stream for a prolonged period of time. If so, then weight loss may lead to tag loss (as the tag loosens), this could lead to confusion between marked fish. The proponent should definitely watch for this problem. Under 'Relative Spawning Success" … observed spawning activity will be a measure of "spawning success" … an assumption and highly questionable.

The responses to the remainder of the questions were appropriate. They are very straightforward in response to ISRP queries about control streams and truly wild fish. Neither probably exist any longer in the project area. The specific ESU question is nearly always going to be a problem. The work is proof of principle, not specific to the ESUs of interest to the RFS.

> **ISRP Preliminary Recommendation:** Fundable contingent upon adequate response to the ISRP's questions and comments. This proposal is of low rank. The proposal is responsive to two of the three evaluation criteria posed in the RFS. It appears to duplicate other studies in this ESU. The ESU is not one of the four listed ESUs. The applicants are unsure as to whether their results will be applicable to the ESUs specified in the additional criteria in the RFS. The work will not extend beyond the F_2 generation. The proposal should be strengthened by the addition of control streams.
>
> Does the study address the following RFS questions:
>
> > • *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
> >
> > Yes

• *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?*

The applicants propose to sample multiple juvenile life stages and the study has the potential to detect allelic and genotypic changes for juveniles.

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*
No

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of the following listed ESUs (for which there are currently no reproductive success studies underway): Upper Columbia steelhead, Mid-Columbia steelhead; Snake River fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*
This is not one of the four priority ESUs listed in the RFS, but the work would have application to other spring/summer chinook populations.

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*
The applicants did not propose to extend the study beyond the F_2 generation.

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*
The applicants are unsure as to whether their results will be applicable to the ESUs specified in the additional criteria in the RFS.

• *Potential to commit to a long-term study (beyond F2); and*
The applicants did not propose to extend the study beyond the F_2 generation.

• *Overall cost effectiveness*
The costs seem reasonable and are in line with other proposals

The applicants propose to compare reproductive success of wild and hatchery origin fish in two Idaho rivers. The work builds on research and data collection undertaken by the Idaho Supplementation Study.

The proposal is a least partially responsive to two of the three evaluation criteria posed in the RFS. As acknowledged by the applicants, the proposed work does not address possible causes for differences in reproductive success between hatchery and wild salmon. The proposal also does not address differences in demographic parameters recommended by VSP. The applicants are unsure as to whether their results will be applicable to the ESUs specified in the additional criteria of the RFS. Releases of hatchery adults will cease in 2007 and consequently the research will not assess effects beyond the F_2 generation. The budget seems reasonable for the work proposed.

Adding "control" streams from the ISS to the study and extension to evaluate fitness of F2 and F3 progeny would be beneficial. This study would add significant value to the interpretation of information from the ISS. Potential useful and unique results could be obtained, but the applicants did not emphasize the extra benefit that this project would add that the others do not have. Namely, what happens to the productivity of F2 and F3 progeny in wild by wild crosses after supplementation is stopped for both supplemented and control streams? That is, when supplementation is stopped, how does the reproduction (fitness) of WxW crosses compare with reproduction of WxW crosses on formerly supplemented streams at the F2 and F3 levels?

Proposal 12: Evaluating the Relative Reproductive Success of Natural- and Hatchery-Origin Snake River Fall Chinook Spawners Upstream of Lower Granite Dam (WDFW)

ESU: Snake River Fall Chinook

ISRP Final Recommendation:

Fundable for phase 1 to test the utility of the mixture analysis methodology. Funding priority (for Phase 1 only) still seems medium, because of the indirect nature of the study and its potential to only gauge relative reproductive success.

The proponents essentially agree with the ISRP evaluation to fund phase 1 and elaborate on that agreement in their responses. The authors' explanation of why to select the mixture model versus a pedigree analysis is appropriate; they explained in adequate detail why pedigree analysis is not feasible. This is clearly a preliminary study to see if the LGD sampling of adults can be matched with progeny to detect a difference. This is a logical first step, and needed before any commitment can be made to later phases (and meeting the listed criteria). It is a good example of using what we have to good advantage (including historical samples already in hand). It was also clear that other ISRP concerns cannot be addressed until their Phase 1 is complete.

The major difference between this proposal (other than it is the only one on Fall chinook) and others is that an admixture model would be used to estimate spawning success as opposed to the more detailed pedigree or parentage models. The history of using such models is that they can be very accurate but need to have good baseline data. The data should be adequate for the use in this proposal, but those tests should be conducted and verified (i.e., using blind tests using fish from known origins). Based on the accuracy of the blind tests and the estimates of precision, sample sizes can be determined for numbers of adults and juveniles.

This may be a useful application of the mixture model and does not require the sample sizes for parentage analysis. The staff managing the data has extensive experience in managing large databases (e.g., the coastwide electrophoretic databases for chinook and coho salmon, and steelhead).

Bottom line, the ISRP supports Phase I based on good responses to our questions. The issue of Phase II, however, should consider when a decision would have to be made on the utility of these results. If Phase II is to proceed, pending results, then a timeframe should be established for decisions and these funds likely protected until a decision not to proceed is made.

> **ISRP Preliminary Recommendation:**
> Qualified funding of Phase 1, pending adequate revision. Genetic divergence should be demonstrated and the utility of the mixture analysis approach should be convincingly demonstrated. Do not fund Phase 2 at this point, pending successful demonstration of the approach. The applicants do not address evaluation criteria two and three in the RFS. This limits the utility and applicability of the research. The work is in a priority ESU and the project could be done in a relatively short time because the tissue samples are already collected.
>
> Does the study address the following RFS questions:
>
> • *Are there statistically significant differences in reproductive success between natural-origin and hatchery-origin fish when measured at the second generation (F2)? Do F1 progeny with HxW parents differ from F1 progeny with HxH parents in the production of F2 progeny?*
> Yes
>
> • *What are possible hypotheses to explain this difference? For example, can the difference be attributed to reduced genetic fitness of hatchery-origin compared to natural-origin fish? Are differences more significant during any specific life history stages?* No

• *What is the likely effect of any difference, in terms of population growth, population recovery, and genetic diversity/fitness in subsequent generations according to the Viable Salmonid Population (VSP) criteria?*
No

Does the proposal address the additional criteria for selecting among well-designed and responsive proposals include:

• *The degree to which studies are directly applicable to one or more of thefollowing listed ESUs (for which there are currently no reproductive successstudies underway): Upper Columbia steelhead, Mid-Columbia steelhead; SnakeRiver fall chinook; and Columbia River chum. Studies not occurring in those ESUs, but with clear applicability to those ESUs will also be considered;*
The Snake River fall chinook ESU is involved.

• *The degree to which the study is designed (or is capable of being extended) to address whether and to what extent any difference in reproductive success of hatchery spawners persists in subsequent generations (beyond F2);*
No

• *The degree to which proposals may provide information more broadly applicable to multiple species/ESUs identified above;*
The analytical approach could be applicable to other situations where a pedigree analysis is unsuitable.

• *Potential to commit to a long-term study (beyond F2);*
Yes

• *Overall cost effectiveness*
Costs are in line with other studies.

The proposal is to conduct a pilot study to determine if genetic divergence among natural and hatchery origin Snake River fall Chinook salmon is sufficient to estimate differences in relative reproductive success. The proposal does not adequately address any of the evaluation criteria in the RFS. It is, however, the only proposal directed at Snake River fall Chinook.

The project evaluates the contribution of reproduction by state of the art hatchery production and "stray" hatchery-origin with natural-origin adults. The same stock (primarily) in the same river system is being contrasted. A mixture analysis, rather than a direct pedigree analysis, will be evaluated, because the sampling conditions preclude the direct pedigree approach. However, a more thorough explanation of why pedigree data cannot be collected is needed.

The proposal does not describe how the applicants will know if the observed genetic divergence is adequate to estimate differences in relative reproductive success. Based on the success of the pilot study, the applicants propose to estimate relative reproductive success of hatchery and natural origin spawners, but they do not provide any details as to how they will accomplish this estimation. Phase 2 of the proposed work is dependent upon successfully finding genetic divergence and will continue the as yet unexplained estimation of reproductive success from 2004 to 2008 for the F_2 generation.

Not all of the important Columbia basin salmon ESU's will be amenable to a pedigree analysis because of logistical constraints on trapping adults and juveniles. This proposal approaches this difficult circumstance by exploring mixture analysis as an alternative (assignment of individuals to a group rather than to specific parents). Since the biological material (fin clips etc) is already collected for a retrospective reconstruction of the mixtures, an answer to whether or not the approach will be fruitful should not take long. On the basis that this is one of the more precariously situated ESU's, the analytical approach is worth pursuing, and the initial investigation will be retrospective.

Table of Proposals