

Peer Review of:

Snider and Titus, Use of the Delta for rearing by Central Valley chinook salmon

2001-K202-3

1. Scientific Merit

1a. Objectives and hypotheses.

Five objectives of this proposal are stated in section C1a (pg 3); comments are provided in sequence of the stated objectives:

1. Measuring the proportion of adults that used the Delta is feasible but depends on the ability to discriminate habitats based on the techniques presented, expressing the proportion as a function of “macro-conditions” is certainly less feasible given the limited duration of the study and the numerous variables that could influence the growth pattern.
2. again feasible within spawning populations assuming adequate information in the different patterns ... the meaning of race is never clarified in the proposal.
3. This objective seems to be the same as (2) but presumes a level of habitat assessments that are not described in the proposal.
4. This objective seems to be the reciprocal of objectives 1-3 but introduces life history in the study.
5. On the basis of the content of this proposal, I see no way to estimate the number of fry migrants or the survival rate of those that utilize the estuary for rearing. The methods proposed might identify those adults that utilized the estuary but the survival rate requires much more information on the numbers of fish surviving over time, etc.

The hypotheses to be tested are stated in C1c (pg 4) but, in my assessment, are far too vague and not testable. Why is there a need for two hypotheses when the study is focused on the use of the estuary? Others portions of the proposal seem to acknowledge an inability to test these hypotheses. In section C2b (pg 9) Hypothesis Testing, the authors’ state “... will be used in an attempt to reject the paired null hypotheses. We anticipate that neither hypothesis will be clearly rejected (...), and that the building of alternative hypotheses will pursue addressing the various study objectives stated in section C1a of this proposal.”

Presumably if the hypotheses are not “clearly rejected” then the study will have failed to “significantly reduce that uncertainty” (section C1d. pg 5) associated with the role of the Delta in the chinook salmon ecosystem.

Hypotheses are also referred to in several other sections of the proposal. In the Executive Summary, the authors state that their working hypothesis is “that there is a direct relationship between salmon survival and use of the Delta ...”. This statement could lead to more testable relationships than the H_0 presented by the authors.

Data needs to test the hypotheses are presumably addressed in the text, but the section on Hypothesis Testing is not adequate in terms of the data to be collected or the tests conducted.

Finally, I am left wondering what will be concluded if H_{0a} was accepted? In a highly altered environment such as the Delta, does accepting this hypothesis mean that the estuary is not

important to chinook production and their ecosystem, or that in its current condition/availability that it is not important? Given what is known in other fall chinook populations, and that these results will be based on 3 years of study, this study by its self may not provide the information needed to draw such conclusions.

1b. Sound Approach

The Conceptual Model ...

Section C1a portrays the debate/uncertainty being addressed as a current belief that the Delta's importance is related to smolt passage and survival but does not recognized the role for and value of fry ... with the potential for loss of chinook production and life history diversity. The authors' also note that survival is a function of the full life cycle and the habitats encountered. I expected the conceptual model description to address these topics.

The conceptual model description is really only three short paragraphs with essentially no discussion of alternative life history theories, other chinook populations or rivers, and only cites one general book, one contract report (not available in our review period), and one reference concerning scale pattern analysis. This does not constitute a conceptual foundation for such a broad and potentially important issue. I was also uncertain why the authors' referred to optimizing survival as opposed to maximizing chinook production ... is optimizing intended to imply a life history response to increase fitness, etc.?

Further, I can not agree with the statement (C1b. pg 4) that the "concept for linking analysis of otoliths to salmon rearing and migration patterns, and survival to adulthood, is simple." Maybe in concept the notion is simple ... but in reality it is much more difficult! The authors' have not addressed how to validate the daily growth pattern, how to control for error in aging chinook salmon (commonly 25% by total age), or the repeatability of characteristics between individuals within sample strata (i.e., a habitat type, a natal stream, or a race, etc.).

The Study Design ...

The approach proposed is basically to measure daily growth patterns on otoliths and associate these to habitat types, stream of origin, race, etc. These patterns would then be used to evaluate patterns on the otoliths of "successful adults" to determine the role of habitat use in chinook production. The proposal frequently refers to survival but it is not clear if the authors' intend to monitor survival rates or just some index of relative survival in the returning adults.

While I don't disagree with the utility and potential importance of using growth increments for such assessments ... I am not convinced by the proposal of their utility to provide the extent of data necessary for this proposal. I was surprised to read section 2g (pg 10) on feasibility. If previous results have been reviewed and endorsed why was this not more fully developed in the proposal? Is there a problem to be addressed yet?

I have the sense that this proposal was submitted as a largely exploratory study and not intended to provide the level of understanding implied. Even so, there were several concerns that occurred to me:

- a) How is the term life history to be interpreted in this proposal? Are the authors' suggesting that life history is simply an expression that results from juvenile displacement and their

opportunistic use of habitats, or is it possible that fry migrants are an alternative strategy of fall chinook that has a genetic basis? The latter is an essential question for conservation of diversity and strongly related to the ecosystems necessary for expression and preservation of that diversity.

- b) The proposal refers to “macro-conditions”, “natal stream conditions”, etc. but does not elaborate on the habitat monitoring/assessments needed and/or available to provide this data. Are habitat assessments conducted over the full range of habitats expected to be encountered (natal stream types, migration corridors, and Delta habitat types, etc.)?
- c) The analysis section of the proposal is very limited in describing the parameters to be considered and the methods section could utilize other techniques to assist in the analyses. For example, DNA analysis (or other stock identification tools) would assist in identifying the populations of origin, and the use of chemical assessments can be used to identify habitat-types experienced during otolith growth. Methods are available to use micro-bores (lasers) and chemical assays to identify freshwater and marine environments occupied by the animal during formation of the daily growth ring.
- d) Most disconcerting is the assumption that survival can be assessed based on adult return proportions. Excluding concerns for differential harvest and interactions with hatchery fish, adult returns may not be representative of the true relative survival rates of the different juvenile life histories. For example, if few adults with a Delta-use pattern return, that would likely be what was expected and easily accepted. However, without knowing the population size of the fry migrants (in terms of animals surviving to smoltification), we don't really know if their survival was less than, equal to, or greater than the other life history forms. Further, fry could experience poor survival in the corridor and then very good survival in the Delta, but the conclusion could easily be drawn that the Delta habitats are poor and not important!

My overall assessment of the study approach is that it is too general and all consuming for a three-year study, overly reliant on one technique, and not fully considered in terms of explaining potential outcomes.

1c. Adaptive Management Approach

The authors' refer to their work as a Targeted Research program and relate to adaptive management in two ways (pg 5): “be responsive to specific information needs” and “be responsive to on-going evaluations ...”.

These are obviously appropriate responses to new research programs but likely do not fully capture the intent of an adaptive management approach in the ERP ... i.e., learning and responding in a structured manner as research progresses. For example, this proposal is large in scope with very broad focus ... but it could easily be partitioned into a series of more testable and progressive research projects. For example, I might suggest this sequence of projects:

- a) Can life history types be differentiated from the otolith patterns (and potentially the chemical content) in returning adults? If yes, proceed to (b), if not consider others studies of Delta habitat use and quality.
- b) Are fry migrants a unique life history type or a function of habitat quality, quantity, and/or population size in natal streams? If unique, then the value and need for Delta habitat just increased; if not unique what is the relative value of increased production from fry versus costs of habitat restoration?

- c) What habitats are utilized by fry migrants and are habitats limited by quality, access, or variation in annual environmental conditions? If habitats are limiting, what habitats are most important and what restoration can be undertaken?
- d) How can important habitats be restored, what methods are necessary, and at what costs?
- e) Final stage maybe an integrated chinook production model incorporating chinook diversity, habitat conditions, and ecosystem function.

Clearly it is not a reviewer's job to redesign projects; I simply included this to demonstrate a means to incorporate adaptive management and to partition this study into more testable investigations. Food for thought only.

2. Adequacy of monitoring, information assessment, and reporting plans.

The proposal contains limited information on these topics (page 10) **but does rely on other monitoring programs** to collect basic samples and data. Is there a concern for the future of those monitoring programs?

3. Technical Feasibility.

My comments on this topic are contained in my responses above; however, I do need to identify that this is **not a simple three-year program**. A three-year sampling program of juveniles equates to, at least, a 7-year program to get the adult returns. This is not included in the budget presented. Further, the budget does not seem to include two salary scales for employees (Table 2) as stated on page 12 of the proposal.

4. Qualifications.

The background of the two principal investigators is very good but most of the work will rely on the persons hired to conduct the analyses of the otoliths, etc.

OVERALL EVALUATION FAIR

While I completely endorse the need for such studies in estuaries, it is my assessment that the authors have not adequately considered what is necessary to significantly reduce the uncertainty in the value of the Delta to chinook production or ecosystem function. I have tried to provide suggestions in my above comments and would certainly encourage the authors to continue in this research topic. I would suggest though that they consider progressing from more testable studies and gradually build upon what they learn. I also believe that more quantitative investigations will be necessary concerning chinook population sizes (over time and area), more extensive habitat assessments (or inclusion of programs that are currently being undertaken), and habitat restoration studies. The latter is to reasonably evaluate the potential importance of Delta habitats to chinook, particularly if recovery is successful in increasing population sizes and diversity in life histories.

Some of my concerns my result from not being aware of the scope of other programs in that region, if so I am sure others from the area can correct by concerns.